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No. 1

A COMPARISON OF LOGICAL AND VERBATIM LEARNING OF PROSE PASSAGES OF DIFFERENT LENGTHS

By CHARLES N. COFER, Brown University

In the learning of a passage of prose, there are two primary types of mastery in which the learner may be interested: one is a verbatim or word for word knowledge of the material; the other is knowledge of the essential ideas that the passage contains. Learning of the first type is called rote or verbatim learning, and the second is called logical, substance, or idea learning.

In the comparison of these two forms of achievement, three methods may be utilized: (1) comparison of the course of acquisition, (2) comparison of the course of retention, and (3) comparison through correlation. No comparisons of the first type have been found in the literature. Comparison through retention has had extensive application with various forms of retention tests and a more limited use in studies employing reproductive recall. In general, retention as measured by substance learning criteria is found to be much greater for immediate and delayed retention tests than when measured by verbatim criteria.¹ Correlational comparisons have resulted, usually, in low coefficients between various measures of verbatim and substance learning.²

The present study was designed to compare these two types of achievement in terms of the time and trials required to reach the criterion of mastery. The effect of such variables as amount of material to be learned,

* Accepted for publication August 19, 1940. This study was done under the direction of Professor Walter S. Hunter.

¹E. L. Welborn and H. B. English, Logical learning and retention: A general review of experiments with meaningful verbal materials, *Psychol. Bull.*, 34, 1937, 1-20.

²M. G. Jones and H. B. English, Notional vs. rote memory, this JOURNAL, 37, 1926, 602-603. Cf. also Welborn and English, *op. cit.*

kind of instructions given; and different methods of scoring has also been studied. In addition, a minor portion of this study deals with retention over a long period of time.

PROCEDURE

Two experiments are to be reported, a learning experiment and a relearning experiment. Preliminary work over a period of one year, in which somewhat different procedures were utilized, resulted in the adoption of the present methods.

Subjects. Twenty-four Ss, 18 to 24 yr. of age, who were paid for their services, were used in the experiment. This group was composed of 4 graduate students, one man and three women, and 20 undergraduates of all classes, 19 of them men. None was aware of the nature of the investigation. Eleven of the Ss, 1 graduate and 10 undergraduate men, were utilized in the relearning study and were again paid for their services.

The Ss reported at approximately the same time either daily or on alternate days. The first two sessions were devoted to learning practice passages, each one 100 words in length. Two passages were learned at each session, one verbatim and one for substance. Then the experimental periods followed, varying in number with the learning ability of S and with the specific order in which the passages were learned. From three to five experimental days were thus consumed. The length of an experimental period was limited to 1 hr., and the length of time between the learning of successive passages ranged from a few seconds to as much as 48 hr. Learning was by massed practice.

Instructions. Two sets of instructions were given to the Ss. These were alternated through the experimental series. For verbatim learning they were:

Read this passage aloud, reading at your normal rate and exposing one line of material at a time in the manner which I shall show you. After you have finished reading the passage, I wish you to try to report it word for word. Repetition will continue until you have learned the wording exactly and have said the passage perfectly twice in succession.

The instructions for logical learning were:

Read this passage aloud, reading at your normal rate and exposing one line of material at a time in the manner which I shall show you. After you have finished reading the passage, I wish you to repeat orally all the essential ideas or points it contains. You need not report word for word, so long as you give the essential ideas correctly. Repetition will continue until you have given all the essential ideas twice in succession.

In addition to practice passages, a further control of practice effects was introduced by varying the order in which the passages were learned by the different Ss. When this was done, each passage occupied an equal average position in the series of passages when the whole group was considered. Differential practice effects thus should not influence the average results.

To give some assurance that a single reading of the material would consist of only one perusal of the passage, S was required to read aloud, and he was permitted to see only one line of material at a time. The passage was typed, double-spaced, on white paper, and this was then pasted on cardboard. S held this in his hand and covered it with another piece of cardboard containing a slit through which only one

line of the text could be seen at a time. He moved the cardboard down the page, exposing line after line at his usual reading speed. Errors in reading were corrected by the experimenter at the time of their occurrence.

Immediately after reading the material, *S* attempted a recitation. A reading and the recitation immediately following it constituted one trial. If an *S* required 10 trials to learn a passage to the criterion of two successive errorless recitations (either verbatim or logical), he had read and recited 10 times each. The recitations were recorded by *E* on mimeographed copies of the passage in question. Appropriate additions, elisions, and changes in word order were made on these sheets. Some inaccuracy undoubtedly resulted from this method of recording, although to make the method as easy as possible *S* was asked to recite slowly. The greatest inaccuracy probably occurred on trials one and two, where many errors were usually made. *S* was allowed to modify his recitation—changing word order, addition of material, etc.—until he began the next reading. This was begun as soon as the recitation was completed.

The relearning sessions occurred 9 mo. plus or minus one week after the original learning. Six of the original passages were used, two each of 50, 100, and 150 words. No preliminary practice was given, but the passage order was varied from *S* to *S* as in original learning. All other conditions were the same as in original learning.

The passages used were brief Indian folk tales and were adapted from reports by Boas,³ Bartlett,⁴ and Newman.⁵ Four passages were available at each of the four lengths used: 25, 50, 100, and 150 words. Four 100-word practice passages were taken from the same sources. The passages may be found elsewhere;⁶ one of them, with the various ways of scoring it indicated, is presented below. It should be noted that the 25-word passages were used with only 14 of the *S*s.

Scoring. Two general methods of scoring were used: (a) word-scoring and (b) idea scoring, each of which included three specific procedures.⁷ The details of these six procedures were as follows.

(a) *Word scoring. Procedure 1.* In the first method, the total word score (*Verb.*), one point credit was given for every word correctly reproduced. Omissions meant that no credit was earned, and word for word substitutions were also counted as failures. Extra phrases, sentences, and single words which could be construed as standing for something in the original passage were penalized one point per word. Where two or more words were clearly substituted for a word in the passage, one point was deducted for each word beyond the actual number in the passage. Thus, if "the boy" was substituted for "he," one point was counted off for the extra word,

³ F. Boas, The mythology of the Bella Coola Indians, *Memoirs Amer. Mus. Nat. Hist.*, 2, 1898-1900, 24-127; *Kwakiutl Tales*, New Series, Part I, Translations, 1935.

⁴ F. C. Bartlett, *Remembering*, 1932.

⁵ E. B. Newman, Forgetting of meaningful material during sleep and waking, this JOURNAL, 52, 1939, 65-71.

⁶ C. N. Cofer, A comparison of logical and verbatim learning of prose passages of different lengths, Ph.D. thesis on file in the John Hay Library, Brown University.

⁷ All reproductions of all lengths of material were scored in all six ways, except those for the 25-word passages. In these cases, two of the idea measures (*Logical-b* and *Logical-n*) were not used, because inspection of the data indicated that in these cases learning was so rapid that such measures would give results no different from those for *Logical* score.

and the substitute received no credit. The reverse, in which "he" would be substituted for "the boy," cost one point for the omitted word, and the incorrect substitute received no credit. Further penalties were assessed for changes in word order. One point was deducted for each word placed in an incorrect position. Substitute words were also scored in this way, where it could be clearly seen for what word the substitute was used. If a whole sentence or phrase was misplaced, the penalty was assessed for each single word therein.

Procedure 2. In the second word-scoring method, one credit was given for the report of significant words (*Signif.*). This method was employed to secure a word score that would approximate the idea score. Certain words, which carried the essential meaning of the passage, were demarcated. The *Signif.* score was the number of these words reproduced correctly. With this method of scoring, changes in word order were not penalized.

Procedure 3. The third word-scoring method consisted in counting the number of sentences recalled without error (*Sent.*). This measure was used in an attempt to find one which would approximate the scoring procedures used in Stroud's experiment.⁸

(b) *Idea scoring.* In idea scoring the intention was to secure measures which would reflect S's achievement in reproducing the sense of the passage without reference to the specific words or word-order of the text. To supply scoring units, the original passages were divided into their constituent ideas in three ways.

Procedure 1. In the first method, which yields the logical score (*Log.*), the essential ideas of the passages were demarked by two judges. S received a credit of one, if enough of the unit was recalled to give the sense of the passage. Thus, in a few cases, where an idea consisted of a major or a minor aspect, the reproduction of the major one only was required. This procedure was adopted instead of one involving partial scores, because cases where the latter method would be applicable were infrequent. Changes in word order were not penalized with this method of scoring, and the addition of words received no penalty unless an error in meaning was thereby introduced. If one word or a group of words gave an erroneous connotation to the passage, one point was deducted from the score.

Procedure 2. The second method of idea scoring (*Log.-b*) was based on broad units. More material was included per unit than in Procedure 1, otherwise it was the same.

Procedure 3. The third procedure (*Log.-n*) was based on units which were less inclusive of material than were the other two sets. It should be noted that some of the *Log.* units are identical with both *Log.-b* and *Log.-n* units. The *Log.* units were determined entirely on the basis of meaning and without regard for the amount of material they contained.

One of the 50-word passages used in both experiments is presented below with the various scoring units indicated.

The people stole the ball of fire from the sleeping fire-god. He awoke and searched for it. He came to their village and saw them playing ball. This angered him, so he joined the game and caught the ball. He swallowed it and burned the village, killing the people.

The maximum *Verb.* score is 50—the total number of words; the maximum *Sent.*

⁸ J. B. Stroud, Learning curves for poetry, this JOURNAL, 43, 1931, 684-686.

score is 5—the total number of sentences. The maximum *Signif.* score is 22, and the units for it are italicized. The maximum *Log.* score is 13, and the units for it are as follows.

The people stole / the ball of fire / from the sleeping fire god. / He awoke / and searched for it. / He came to their village / and saw them playing ball. / This angered him, / so he joined the game and caught the ball. / He swallowed it / and burned the village, / killing the people. /

The maximum *Log.-n* score is 19, and the units for it are as follows:

The people stole / the ball of fire / from the sleeping fire god. / He awoke and searched for it. / He came to their village / and saw them playing ball. / This

TABLE I
NUMBER OF UNITS IN EACH EXPERIMENTAL PASSAGE

Passage no.	Method of learning	Word scoring			Idea scoring		
		Verb.	Signif.	Sent.	Log.	Log.-n	Log.-b
13	V*	25	13	3	7	—	—
14	L†	25	11	4	7	—	—
15	V	25	12	3	7	—	—
16	L	25	13	4	7	—	—
1	V	50	22	5	13	19	10
2	L	51	24	6	13	20	10
3	V	50	21	5	13	18	9
4	L	50	19	5	13	19	9
5	V	99	45	8	22	31	14
6	L	99	43	10	22	33	14
7	V	100	46	9	22	34	16
8	L	99	40	8	22	33	13
9	V	149	66	12	33	45	20
10	L	150	68	13	33	46	25
11	V	150	66	15	33	45	23
12	L	150	63	15	33	45	20

* V=verbatim.

† L=logical.

angered him. / so he joined the game and caught the ball. / He swallowed it / and burned the village, killing the people. /

The maximum *Log.-n* score is 19, and the units for it are as follows:

The people stole / the ball of fire / from the sleeping fire god. / He awoke / and searched / for it. / He came / to their village / and saw them / playing ball. / This angered him, / so he joined / the game / and caught / the ball. / He swallowed it / and burned / the village, / killing the people. /

The passages varied in the number of the different score-units they contained. In Table I the maximum score for each method is given for every passage. It should be noted that the increases in number of units are about in the same proportion as the increases in the number of words (*Verb.*), especially in the case of *Signif.* and *Log.* scores.

In order to determine the consistency of scoring, a comparison was made of the

Verb. and *Log.* scores compiled at two times separated by several weeks. Scores for five passages were so analyzed. In 84% of the cases for word scoring and in 88% for idea scoring, no difference appeared in the two scores that had been assigned. Where differences did appear, they rarely exceeded one point.

RESULTS

(1) *Mastery in original learning.* The total number of trials required for learning the various passages is indicated in Tables II, III, and IV and in Fig. 1. In Table II the average number of trials required for logical learning, when instructions were so to learn, is given for each of the methods of idea scoring together with the range, the standard deviation of the distribution, the standard error of the mean, the number of Ss, and the lengths of the passages. It is to be noted that the averages for the two passages of each length are quite similar; only the average for each pair has been plotted in Fig. 1. Similar results are shown in Table III for passages learned verbatim and scored by the word methods. Table IV gives the results for passages learned under verbatim instructions and scored by idea methods. In this table some of the differences between averages for passages of equal length are greater than the corresponding values in Table II. This difference in variability, it would seem, may be attributed to the effect of instructions, as these were different for the two sets of data. In other words, where the Ss' performances were scored by a method irrelevant to the task set, there was a greater variability in result than where the performances were scored in the way corresponding to the instructions.

A consideration of these results may now be made in terms of Fig. 1, which shows the number of repetitions for learning plotted as a function of passage length. The total number of words is the index of passage length in this graph, since the number of idea score units in the passages is approximately in the same ratio as the number of words (see Table I); a slight distortion of the results for the idea scores is thus introduced. The first point to be noted is the great similarity between the two curves for the word-scoring methods. The *Verb.* and *Sent.* scores by definition must have the same curve, and the curve for *Signif.* score closely parallels it, while showing that consistently fewer trials are required to reach the criterion for this measure than for the other two. These curves are for passages learned with verbatim instructions. The lowest curve in the figure is for *Log.* score, and the points just above and below this curve are for *Log.-n* and *Log.-b*, respectively. The instructions here were to learn for substance, and the three curves drawn or indicated are all of the same form. *Log.-b* scores require consistently fewer trials for learning than the other two, and the points for *Log.* and *Log.-n* overlap.

TABLE II
AVERAGE NUMBER OF TRIALS REQUIRED TO LEARN (IDEA SCORING) WITH INSTRUCTIONS
FOR LOGICAL LEARNING

Pas- sage	No. of Words	Log.					Log.-b					Log.-n				
		Av.	Range	N	S.D.	S.D. _m	Av.	Range	N	S.D.	S.D. _m	Av.	Range	N	S.D.	S.D. _m
14	25	1.28	1-2	14	0.46	0.12	—	—	—	—	—	—	—	—	—	—
16	25	1.14	1-2	14	0.35	0.09	—	—	—	—	—	—	—	—	—	—
Av.	25	1.21			0.42	0.07										
2	51	1.75	1-3	24	0.59	0.12	1.54	1-3	24	0.57	0.11	1.75	1-3	24	0.59	0.12
4	50	1.65	1-3	23	0.63	0.13	1.12	1-2	24	0.34	0.07	1.89	1-3	19	0.65	0.15
Av.	50	1.70			0.61	0.09	1.33					1.82				
6	99	2.58	2-4	24	0.65	0.13	2.25	1-4	24	0.66	0.13	2.58	2-4	24	0.65	0.13
8	99	2.12	1-3	24	0.55	0.11	2.00	1-3	24	0.50	0.10	2.25	2-3	12	0.36	0.15
Av.	99	2.35			0.60	0.08	2.12					2.42				
10	150	2.79	2-6	24	1.00	0.20	2.74	2-6	23	1.12	0.23	2.67	2-6	22	1.34	0.28
12	150	2.66	2-4	24	0.57	0.11	2.33	1-4	24	0.69	0.14	2.66	2-4	24	0.70	0.14
Av.	150	2.72			0.78	0.11	2.53					2.66				

TABLE III
AVERAGE NUMBER OF TRIALS REQUIRED TO LEARN (WORD SCORING), WITH INSTRUCTIONS
TO LEARN VERBATIM

Passage	No. of Words	Verb. and Sent.					Signif.				
		Av.	Range	N	S.D.	S.D. _m	Av.	Range	N	S.D.	S.D. _m
13	25	2.64	1-4	14	0.55	0.14	2.67	1-3	14	0.88	0.23
15	25	2.00	1-3	14	0.13	0.10	1.20	1-3	14	0.58	0.16
Av.	25	2.32			0.34	0.06	1.98				
1	50	4.95	2-13	24	2.50	0.51	4.37	2-13	24	2.11	0.43
3	50	5.08	3-8	24	1.48	0.30	4.50	2-8	24	1.52	0.31
Av.	50	5.01			1.99	0.28	4.43				
5	99	7.57	4-12	19	2.37	0.53	7.00	2-12	20	2.66	0.59
7	100	7.18	4-13	22	2.45	0.52	6.77	4-12	22	2.15	0.46
Av.	100	7.38			2.41	0.37	6.88				
9	149	9.10	5-14	19	2.21	0.50	9.19	5-14	21	2.34	0.52
11	150	8.84	5-13	13	2.40	0.66	8.15	4-12	13	2.29	0.62
Av.	150	8.97			2.31	0.40	8.67				

TABLE IV
AVERAGE NUMBER OF TRIALS REQUIRED TO LEARN (IDEA SCORING), WITH INSTRUCTIONS
TO LEARN VERBATIM

Pas- sage	No. of Words	Log.					Log.-b					Log.-n				
		Av.	Range	N	S.D.	S.D. _m	Av.	Range	N	S.D.	S.D. _m	Av.	Range	N	S.D.	S.D. _m
13	25	1.30	1-3	14	0.55	0.15	—	—	—	—	—	—	—	—	—	—
15	25	1.00	0	14	0.00	0.00	—	—	—	—	—	—	—	—	—	—
Av.	25	1.15														
1	50	3.00	1-9	24	1.87	0.38	2.70	1-8	24	1.77	0.36	3.00	1-9	24	1.80	0.36
3	50	1.91	1-4	24	0.68	0.13	1.75	1-4	24	0.77	0.15	2.25	1-4	24	1.00	0.22
Av.	50	2.44					2.22					2.62				
5	99	3.83	2-13	24	2.53	0.51	3.08	1-7	24	1.44	0.30	4.12	2-13	24	2.49	0.50
7	100	3.41	1-7	24	1.59	0.32	3.08	1-7	24	1.55	0.31	3.58	1-7	24	1.53	0.32
Av.	100	3.62					3.08					3.85				
9	149	3.83	2-9	24	1.97	0.40	3.29	1-9	24	2.07	0.49	4.00	2-9	24	1.87	0.37
11	150	5.71	1-11	21	2.28	0.49	5.71	1-11	21	2.28	0.49	5.71	1-11	21	2.28	0.49
Av.	150	4.77					4.50					4.85				

The curves for both logical and verbatim learning show negative acceleration, *i.e.* as the passage lengths increase the number of trials increases at a diminishing rate. More trials are required for verbatim learning than for logical learning at every passage length, and trials are added at a faster rate for verbatim than for logical learning; the difference between *Log.* and *Verb.* scores increases from 1.11 to 6.25 trials as the passages become

TABLE V
RATIOS OF NUMBER OF TRIALS FOR LONGER PASSAGES TO NUMBER FOR SHORTEST PASSAGE.
DATA FOR *Log.* AND *Verb.* SCORES, WITH CORRESPONDING INSTRUCTIONS

Verbatim passages		Substance passages	
Ratio of increase in		Ratio of increase in	
words	trials	ideas	trials
1	1	1	1
2	2.2	1.8	1.4
4	3.3	3.1	1.9
6	3.9	4.7	2.2

TABLE VI
AVERAGE LEARNING TIME FOR *Verb.* AND *Log.* SCORES, WITH CORRESPONDING INSTRUCTIONS

Length of passage	<i>Verb.</i>			<i>Log.</i>		
	Total sec.	Sec. per unit	No. cases	Total sec.	Sec. per unit	No. cases
25	84	3.4	12	75	10.7	10
50	372	7.4	12	186	14.3	10
100	1026	10.3	12	390	17.7	10
150	1806	12.0	12	567	17.2	10

longer. This difference may be further indicated by comparing the increases for *Log.* and *Verb.* scores in relation to the respective increases in the number of units. Table V shows that, for a fourfold increase in the number of words, the number of trials for *Verb.* score increases 3.26 times, whereas for a roughly comparable increase in the number of *Log.* units, the increase in number of trials is 2.25. Hence, it is seen that both forms of achievement are affected by increased length of material, but that the *Verb.* score is the more influenced of the two.

The statistical significance of the differences in trials required for *Verb.* and *Log.* scores by passages of different lengths was determined. An analysis of the critical ratios (based on standard deviations) for the *Verb.* curve shows that all are well above the level of significance (3.00) except for the increase from 100 to 150 words. In this case, the critical ratio is 2.94, which may be considered significant. For the *Log.* score curve all

critical ratios exceed 3.00. If the individual passages of both curves were considered, the significance of the differences would not be so great as

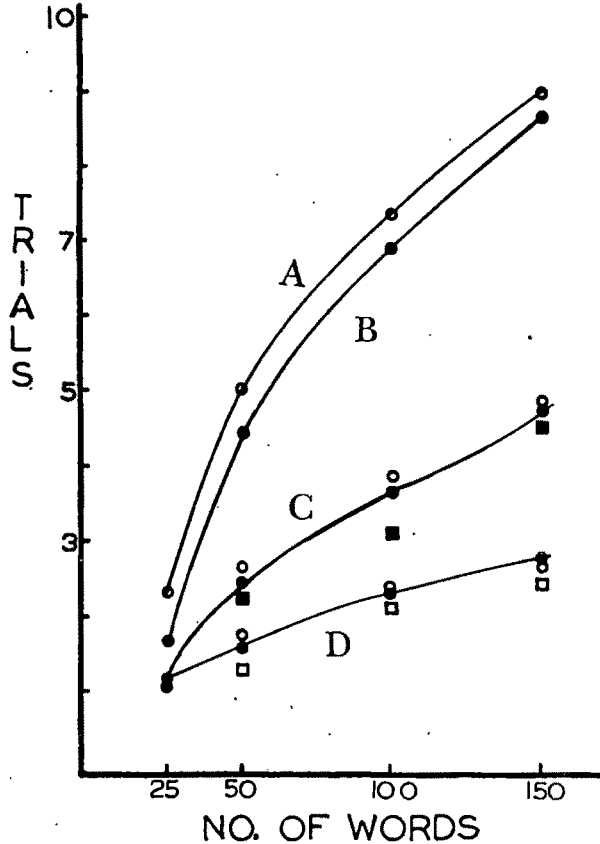


FIG. 1. REPETITIONS REQUIRED FOR LEARNING PLOTTED AGAINST PASSAGE LENGTH
Curve A is for *Verb. and Sent.*; and Curve B is for *Signif.*; in both instructions were verbatim. Curve C is for *Log.*, verbatim instructions; the open circles are for *Log.-n*, the filled squares for *Log.-b*. Curve D is for *Log.*, instructions to learn logically; the open circles are for *Log.-n*, the open squares for *Log.-b*. Every point of all the curves is based on two passages.

for the averages in Fig. 1. In no case, however, was the tendency shown by the pertinent curves in the figure reversed by individual passages.

One further point must be made with respect to the data graphed in the middle of this figure. The points are for *idea* scores when the passages were learned with *verbatim* instructions. The trials required under these

circumstances average higher than for similar lengths of material learned with logical instructions. Length of passage is again an effective variable in influencing trials required for learning, but the slope of the curve describing this relationship is intermediate to the slopes of the curves for which

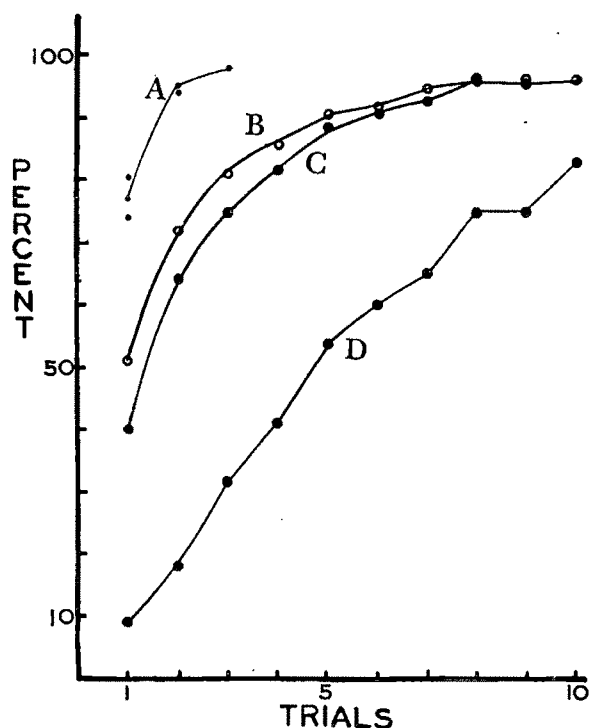


FIG. 2. LEARNING CURVES FOR TWO 150-WORD PASSAGES

Curve A is for *Log.*, logical learning; the point above the curve is for *Log.-b* and the points below for *Log.-n*. Curve B is for *Signif.*, Curve C for *Verb.*, and Curve D for *Sent.*; in all of which learning was verbatim.

the scoring methods used were in harmony with the type of instructions given.

Although all the *Ss* reached mastery for logical learning when they worked under instructions to learn verbatim, the opposite was but rarely true. Thus, under the latter condition, no *S* achieved the *Verb.* and *Sent.* criteria for passages longer than 25 words. A few *Ss* did so for *Signif.* score with the 50-word passages and one 100-word passage, but the data are too meager for analysis. Several *Ss*, however, achieved the criteria for

Verb., *Sent.*, and *Signif.* scores with the 25-word passages when instructed to learn for substance. For these Ss verbatim learning was as rapid as when the instructions were to learn passages of this length verbatim, and for one passage it was more rapid.

The major conclusions to be derived from the foregoing results are:

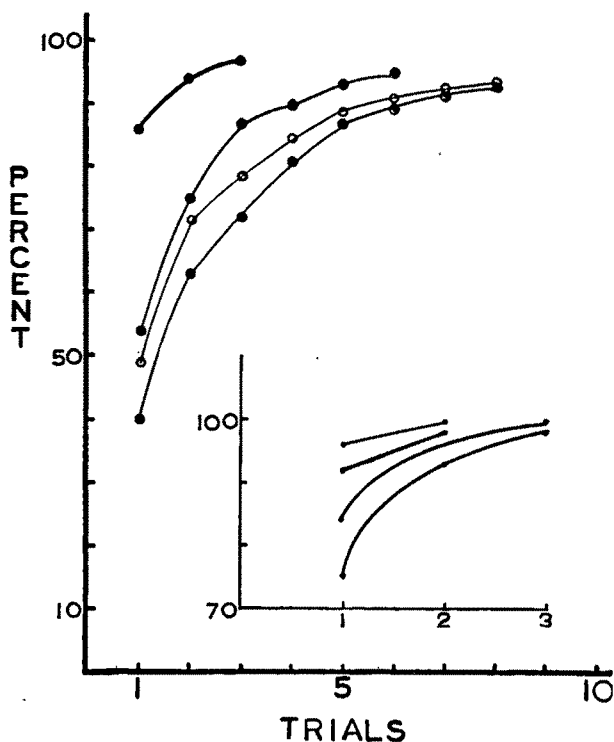


FIG. 3. LEARNING CURVES FOR *Verb.* AND *Log.* FOR ALL LENGTHS OF MATERIAL. The curves on the large coördinates are for the 25-, 50-, 100-, and 150-word passages (verbatim instructions and scored by word methods) in order from top to bottom. The inset curves are for passages learned with logical instructions and scored for ideas. From top to bottom they are for passages of 7, 13, 22, and 33 idea units. Every point for all curves is based on results from two passages.

(1) that different numbers of trials are required for logical and verbatim learning; (2) that, when trials are plotted against passage length, the verbatim curve has a steeper slope than the logical curve; and (3) that the curve of idea-scores for verbatim passages is steeper than the one for idea-scoring of passages learned for substance.

A brief comparison of these results may be made with those of previous investigators. The length-difficulty relationship has been discussed in numerous studies of nonsense syllable learning. While there is general agreement that difficulty (in trials) increases as a function of length of list, there are divergent views as to the rate at which this occurs. Ebbinghaus⁹ found a steep, though negatively accelerated curve, when trials for learning were plotted against length of list. Meumann¹⁰ and Lyon,¹¹ however, reported data which indicate that an initially rapid rise in the curve is followed by an abrupt slowing down in rate. Differences in these results may be due to variations in the Ss' abilities, differential practice effects, different rates of syllable presentation, and variation in inter-trial intervals,¹² to mention only a few possibilities. Although nonsense syllables are not comparable to the units used in this study, some comparisons between the two types of materials may be made if we assume that 7 or 8 syllables are about as difficult to learn as 25 connected words. This seems justifiable since both approximate the memory span. Comparison of the present data for *Verb.* score and of the data in the literature on nonsense syllable learning shows that, for increases in length beyond the memory spans, difficulty increases at a much faster rate for the syllables than for *Verb.* units. It should be noted, however, that for poetry or for other selections of prose this relationship may be different.

Comparisons may also be made between the data on verbatim learning of this study and some of the results reported by Lyon¹³ and by Henmon.¹⁴ Lyon needed 1, 9, 18, and 22 repetitions for the learning of prose of 25, 50, 100, and 150 words, respectively. Repetitions here were added at a faster rate than in the present study. Some data for poetry reported by this investigator with himself as S are similar to those here reported, except for the increase in length from 100 to 150 words. Henmon secured data from one S which showed an absolute decrease in trials as the prose increased in length from 100 to 150 words, and Lyon's data are somewhat similar for the range between 150 and 300 words. Henmon's results for poetry

⁹ H. Ebbinghaus, *Memory* (trans. by H. A. Ruger and C. E. Bussenius), 1913, 60-80.

¹⁰ E. Meumann, *The Psychology of Learning* (trans. by J. W. Baird), 1913, 1-393.

¹¹ D. O. Lyon, The relation of length of material to time taken for learning and the optimal distribution of time, I., *J. Educ. Psychol.*, 5, 1914, 1-9; II., *ibid.*, 5, 1914, 85-91; III., *ibid.*, 5, 1914, 155-163.

¹² C. I. Hovland, Experimental studies of rote learning theory. III. Distribution of practice with varying speeds of syllable presentation, *J. Exper. Psychol.*, 23, 1938, 172-190.

¹³ D. O. Lyon, *op. cit.*

¹⁴ V. A. C. Henmon, The relation between learning and retention and amount to be learned, *J. Exper. Psychol.*, 2, 1917, 476-484.

vary with the material and the group of Ss used. Thus, 3 Ss added trials at a rate roughly comparable to that of the present study for similar lengths, whereas nine other Ss who learned the same poem and another S who learned a different poem required fewer trials as the selection became longer, at least over a part of the range investigated. Hence, it is again difficult to generalize concerning the agreement of the present results with those in the literature. Differences in Ss, practice, material, presentation of the material, and other factors may obscure basic agreement if it exists.

(2) *Time required for learning.* The total time in seconds required for learning was computed from the notation on the record of the time each passage was begun by the S in question. (These data were not available for all the Ss.) The data in Table VI indicate that for *Verb.* and *Log.* scores, the number of units of which are about in the same ratio for the various passages, total time increases approximately 21 and 6 times, respectively, with the increase in the length of material. *Verb.* score also shows a greater increase in time per unit than does the *Log.* score.

The absolute values in Table VI for *Verb.* score are quite similar to nonsense syllable data reported by Meumann, while the values reported by Ebbinghaus show a greater increase with the longer lists than do the other data. A curve similar in form to Meumann's was presented by Robinson and Heron.¹⁵ The comparison of these data with those for *Verb.* score is valid only if it be assumed that 25 connected words are about equal in difficulty to 7 or 8 nonsense syllables. The increase in time reported by Lyon¹⁶ for massed practice with prose selections of lengths comparable to those used in this study are greater than the increases here reported.

(3) *Learning curves.* The learning curves presented in Figs. 2 and 3 are based on the data given in Tables VII, VIII, and IX. The score for each trial is the number of correct units minus penalties converted into a percentage of the total possible score for the passage. Percentages were used to permit comparisons among passages having different numbers of the various score units. Data were included for every S up to but not including the second criterial trial. First criterial trial scores had to be included in order to permit the utilization of data from the Ss whose first trial score was a perfect one. Since the Ss reached the second criterial trial in different numbers of trials, the number of Ss varies through the course of each learning curve, as indicated in the tables.

¹⁵ E. S. Robinson and W. T. Heron, Results of variations in length of memorized material, *J. Exper. Psychol.*, 5, 1922, 428-448.

¹⁶ D. O. Lyon, *op. cit.*

In Fig. 2 and Tables VII and VIII are given data for two 150-word passages, one of which was learned verbatim and one logically, the scores corresponding to the type of instructions. Since the curves for all other passages revealed essentially the same picture for the different idea scoring methods, they will not be presented here. It should be observed in Fig. 2 that the curves (not all drawn) for the three idea measures are very similar. Differences appear on the first trial, but they are insignificant.

TABLE VII
AVERAGE TRIAL BY TRIAL PERCENTAGE SCORES FOR THE WORD SCORES FOR A
150-WORD PASSAGE, LEARNED VERBATIM

Trial	Verb.		Signif.		Sent.	
	Av.	N	Av.	N	Av.	N
1	40	24	51	24	9	24
2	64	24	72	24	18	24
3	75	24	82	24	32	24
4	82	24	88	24	42	24
5	88	24	91	24	54	24
6	91	23	93	22	60	23
7	93	20	95	20	66	20
8	95	19	96	18	75	19
9	96	16	97	15	75	16
10	97	13	97	13	83	13

TABLE VIII
AVERAGE TRIAL BY TRIAL PERCENTAGE SCORES FOR THE IDEA SCORES FOR A
150-WORD PASSAGE, LEARNED LOGICALLY

Trial	Log.		Log.-b		Log.-n	
	Av.	N	Av.	N	Av.	N
1	77	24	81	24	77	24
2	95	24	95	24	94	24
3	98	13	98	13	98	17

The highest points are for *Log.-b*, the middle ones (through which the curve is drawn) for *Log.*, and the lowest points for *Log.-n*. The relationships among the three word measures show essentially parallel curves for *Verb.* and *Signif.* scores, with the former being lower in ordinate position through most of its length. *Sent.* score gives a curve much lower on the ordinate than the other two curves for word scores; its form is likewise different, approximating a straight line. Curves for the other verbatim passages were plotted and revealed the same relationships. It should be noted that no S-shaped curves were found for *Sent.* score, although some of the curves were more irregular than the one presented. The low ordinate values for the *Sent.* score curve are due to the fact that

a whole sentence had to be absolutely correct before credit was assigned for even one unit.

Comparison may also be made in Fig. 2 of the logical and verbatim curves. It is clear that the logical curves occupy much higher ordinate positions than verbatim curves for corresponding trials, indicating the mastery of relatively more material at each trial. Fig. 3 and Table IX make possible this same comparison for all lengths of material, the average *Verb.* curves for the two passages of each length learned verbatim being plotted

TABLE IX
AVERAGE TRIAL BY TRIAL *Verb.* AND *Log.* PERCENTAGE SCORES FOR PASSAGES
LEARNED VERBATIM AND LOGICALLY
(Each number the average for two passages)

Trial	25 words				50 words				100 words				150 words			
	Verb.	N	Log.	N	Verb.	N	Log.	N	Verb.	N	Log.	N	Verb.	N	Log.	N
1	86	28	96	28	53	48	91	48	49	48	82	48	40	48	75	48
2	96	25	100	4	75	48	98	40	72	48	97	46	64	48	94	48
3	98	8	—	—	89	46	—	—	82	48	99	17	76	48	98	13
4					91	36			87	48			81	48		
5					93	26			89	43			87	48		
6					95	18			92	37			90	43		
7									93	30			87	40		
8									95	26			96	36		
9									95	20			96	33		
10									96	16			96	26		

on the large coördinates, and the average *Log.* score curves for the two passages of each length learned logically appearing in the inset of the figure. From these data it may be seen that the curves for logical learning have higher ordinate positions than do all the verbatim curves for passages of corresponding length. Furthermore, this is true for all the logical and verbatim curves except that the verbatim curves for the 25-word passages, if plotted on the inset axes, would lie between the 13 and 22 unit (50 and 100 words) logical curves.

The comments made above concern scores made relative to the total possible score. It is also of interest, however, to compare the different passage lengths with respect to the absolute number of units recalled. Thus, on trial one, 22, 27, 49, and 60 *Verb.* units were reported for passages of 25, 50, 100, and 150 words in length, respectively. These results, which show larger numbers of units being reported as the total number presented increases, are consistent with those reported by Hunter and Sigler¹⁷ for the

¹⁷ W. S. Hunter, and M. Sigler, The span of visual discrimination as a function of time and intensity of stimulation, *J. Exper. Psychol.*, 26, 1940, 160-179.

span of visual discrimination. Their *Ss* were shown varying numbers of dots, time and intensity of exposure being constant, and the *S* reported the number of dots he saw. In a typical experiment, when 5 dots were presented all 5 were seen in only 24% of the trials. However, 43%, 64%, and 85% of the reports were 5 or greater when 6, 7, and 8 dots, respectively, were presented.

The effect of length of passage on the form of the learning curves may also be seen in Fig. 3. The curves for *Verb.* score are essentially parallel and decrease in ordinate value for corresponding trials as length increases. Similar statements may be made for the *Log.* score curves.

The foregoing comparisons have been made for curves for which the numbers of trials were different. Thus, conclusions can be made only for scores made on corresponding trials and not in terms of the total learning process. In order to make the latter comparison, some of the data have been treated by the Vincent procedure. The data for the 25-word passages and for the passages learned logically have not been so manipulated, because in these cases learning was completed in three trials or less. Under such conditions the comparison of these data with those for passages requiring many trials for learning would undoubtedly introduce serious artifacts with few compensating advantages.¹⁸ Hence, the Vincent procedure has been used only for data from one each of the 50-, 100-, and 150-word passages learned with verbatim instructions. (The other comparable passages reveal the same type of curves.) Hunter's procedure¹⁹ was used, and the learning process was fractionated into 5 divisions. To simplify computation, the average amount (in percentage) added to the total score at each trial was used. Thus, if an *S's* scores on trials one and two were 35% and 50%, respectively, the values used for the Vincent operations were 35 and 15, the amounts added on trials one and two. By this procedure the total of the values for the five divisions is equal to the actual score made on the trial just prior to the first criterial trial. Following Melton's²⁰ suggestion, criterial trials were eliminated from the computations. The data secured from this treatment are presented in Table X. It is seen that the Vincent values for the 100- and the 150-word passages are essentially the same. Those for the 50-word passage are similar to the others except in the first Vincent division in which the value is much lower than corresponding

¹⁸ E. R. Hilgard, A summary and evaluation of alternative procedures for the construction of Vincent curves, *Psychol. Bull.*, 35, 1938, 282-297.

¹⁹ W. S. Hunter, Experimental studies in learning, *A Handbook of General Experimental Psychology*, 1934, 497-570.

²⁰ A. W. Melton, The end-spurt in memorization curves as an artifact of the averaging of individual curves, *Psychol. Monog.*, 47, 1936, (no. 212), 119-134.

values for the other passages. The similarities among the Vincent values for the passages of different lengths agree with the findings of Robinson and Darrow²¹ and of Kjerstad,²² who found little difference in Vincent curves for different lengths of various materials. Robinson and Darrow, however, did call attention to the fact that the scores in the first Vincent division for their shortest list of digits, 4, and for the shortest list of nonsense syllables, 6, used by Robinson and Heron²³ were lower than the corresponding Vincent values for the longer lists.

In the discussion on page 10f., it was indicated that the number of trials required for learning the different amounts of material was partly a func-

TABLE X
VINCENT CURVE VALUES FOR A 50-, A 100-, AND A 150-WORD PASSAGE, SHOWING THE PERCENTAGE OF NEW MATERIAL MASTERED IN EACH FIFTH OF LEARNING (Learned verbatim and scored for *Verb.*)

Fifths	50 words	100 words	150 words
1	32%	54%	56%
2	27%	26%	21%
3	15%	9%	12%
4	7%	4%	4%
5	7%	3%	3%

tion of instructions. This is, however, only slightly the case when learning curves showing the average percentage score per trial are compared. Thus, these curves for idea scores for passages learned with verbatim instructions are slightly lower on the ordinate than the curves for the idea scores for passages learned logically. Exceptions occur for the 25-word passages, and none of the differences for a single trial exceed 7 points. Even less difference appears when the curves for word scores for passages learned logically were compared with similar curves for passages learned verbatim.

The major findings from the data on learning curves may now be summarized. (1) Most of the curves were of the typical, negatively accelerated variety, although for the shortest passages, for the selections learned logically, and for the *Sent.* score this tendency was sometimes obscure. (2) The curves for logical learning occupied higher ordinate positions than did verbatim curves for corresponding trials; exceptions were noted in the case of the 25-word passages. (3) Ordinate positions were progressively lower for corresponding trials for both logical and verbatim learning as

²¹ E. S. Robinson and C. W. Darrow, Effect of length of list on memory for numbers, this JOURNAL, 35, 1924, 235-243.

²² C. L. Kjerstad, The form of the learning curves for memory, *Psychol. Monog.*, 26, 1919, (no. 116), 1-117.

²³ E. S. Robinson and W. T. Heron, *op. cit.*

the length of the selections increased. This was true only for the percentage scores, and the tendency was reversed when some of the absolute scores were considered. (4) Curves for verbatim learning were essentially the same for the 50-, 100-, and 150-word passages when treated by the Vincent method. (5) The curves for the three idea scores were essentially the same, and the curves for *Signif.* and *Verb.* were parallel. The *Sent.* score curve was lower in ordinate value than the others and tended to be irregular in form.

The results of two other studies may be compared with the present results. Stroud²⁴ published curves for the verbatim learning of poetry which were based upon the number of lines correctly recalled per trial, a measure which seems most similar to the *Sent.* score of the present study. (It should be noted, however, that the sentences of the present study were not of equal length but were complete in thought, whereas Stroud's units were equal in length but were presumably not complete thought units.) He used two poems, one difficult and one easy, of 20 lines each. Thirty Ss learned these poems, and Stroud divided this group into the 15 best and the 15 poorest learners. The curve for the poorest learners and the easier poem approximates the *Sent.* score curve of Fig. 2 quite closely, except that it, like all the curves in Stroud's paper, is slightly S-shaped. The meaningfulness of this correspondence cannot be ascertained, as neither the name of the poem nor the number of words it contained appears in Stroud's report. Gordon²⁵ had students study some Shakespearean sonnets containing from 90 to 110 words. Learning was verbatim but was carried through only five trials so that complete learning did not occur. Curves for the five trials are of essentially the same form as those in Fig. 3 over a similar range.

The studies referred to above have dealt solely with verbatim learning. So far as learning curves for logical learning are concerned, the ones presented in this report seem to be the first that have been plotted.

(4) *Relearning.* To determine whether there were any retention effects after a period of 9 mo., two measures were used: one was the savings in trials required for relearning by the 11 Ss available as compared with their records in original learning; the other was the comparison of trial by trial scores made by these Ss in original learning and relearning.

The small number of Ss in the relearning study makes a statistical treatment inappropriate. Some savings, however, were indicated for learning the 50-, 100-, and 150-word passages verbatim. Savings of 2, 9, and 12% were evidenced for these lengths, respectively, for *Verb.* and *Sent.* scores; and of 9, 23, and 13%, respectively, for *Signif.* score. The results for logical learning do not clearly indicate retention. Savings of 6, -1, and 8%

²⁴ J. B. Stroud, *op. cit.*

²⁵ K. Gordon, Some records of the memorizing of sonnets, *J. Exper. Psychol.*, 16, 1933, 701-708.

were indicated for *Log.* score for the 50-, 100-, and 150-word passages respectively. The results for the same passages for *Log.-b* showed savings of -3, 0.9, and 7% and for *Log.-n* of 1, -1, and 8%. The negative numbers indicate that relearning required more trials than original learning.

There is a slight tendency in all cases except that of *Signif.* score for the longer passages to exhibit somewhat more savings than the shorter passages. While the significance of the tendency cannot be determined from the data, it is consistent with the results of Ebbinghaus,²⁶ Robinson and Heron,²⁷ Robinson and Darrow,²⁸ and Woodworth,²⁹ which showed that the longer lists of nonsense syllables, digits, and paired associate lists of words were better retained than were the shorter lists.

Average scores per trial made by the 11 Ss who relearned were essentially the same in relearning and original learning for all the passages learned logically and for the 50-word passage learned verbatim. For the 100- and 150-word passages learned verbatim, however, the *Verb.* scores per trial were consistently better for relearning than for original learning. The differences in favor of relearning for the 100-word passage range from 3 to 11%. The differences for the 150-word passage for the first five trials range from 5 to 16%; for the rest of the trials the differences are smaller and one reversal occurs.

Some retention then may be indicated for the two longest passage lengths in terms of trial by trial scores. The significance of the differences is probably small; but, taken in conjunction with the similar result for trials required for learning, they would seem to show that after a nine months' interval there is some retention of passages of 100 and 150 words learned verbatim.

SUMMARY

An experiment designed to compare logical and verbatim learning of prose selections is reported. 24 college students learned 16 Indian folk tales of 25, 50, 100, and 150 words either logically or verbatim. Trial by trial records of the course of learning were made, and these data were scored by means of three word and three idea scoring methods. The results secured lead to the following conclusions.

- (1) Verbatim learning requires more trials than does logical learning.
- (2) Difficulty, in terms of trials to learn, increases more rapidly with increase in amount of material for verbatim than for logical learning.
- (3) Where logical learning is done under instructions to learn logically, the

²⁶ Ebbinghaus, *op. cit.*

²⁷ Robinson and Heron, *op. cit.*

²⁸ Robinson and Darrow, *op. cit.*

²⁹ R. S. Woodworth, The influence on retention of conditions favoring quickness of learning, *J. Philos.*, 12, 1915, 246.

DETERMINANTS OF APPARENT VISUAL SIZE WITH DISTANCE VARIANT

By ALFRED H. HOLWAY and EDWIN G. BORING, Harvard University

The size of the retinal image is a peripheral determinant of visual size. Presumably, if all other determinants were constant, perceived size would vary directly with the visual angle, which might even be used then as a measure of apparent size. It has been known for a long time, however—since Fechner¹ and Hering,² at any rate—that the visual angle does not provide a consistent measure of perceived size when the distance from *O* to the stimulus-object is varied. Martius' experiment³ in 1889 demonstrated that the apparent size of objects may change scarcely at all when distance changes, and nowadays it is customary to use the term 'size constancy' as a reminder that, when perceived size is constant, the visual angle sometimes is not.⁴

Ordinarily, of course, size is not constant in spite of distance. Even the philosophers of the eighteenth century remarked that two parallel rows of trees appear to converge as one views the vista between the rows, and they attributed the convergence to the law of the visual angle and the underestimation of the greater distances.⁵ To get things started toward a solution of this problem, Hillebrand⁶ and others worked out the form of the curve that the walls of a short, narrow alley should have in order to appear equally separated at every distance;⁷ these studies were factual in emphasis. Recently, Thouless⁸ has conceived of the organism as regressing in perception from a proximal perceptual datum (the retinal image) toward a more remote one (the real object), so that actual perception can be regarded as

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¹ G. T. Fechner, *Elemente der Psychophysik*, 1860, II, 311-313.

² E. Hering, *Beiträge zur Physiologie*, I, 1861, 13-16.

³ G. Martius, Ueber die scheinbare Grösse der Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder, *Philos. Stud.*, 5, 1889, 601-617.

⁴ Cf. K. Koffka, *Principles of Gestalt Psychology*, 1935, 87-97, 235-240.

⁵ Cf. J. Priestley, *The History and Present State of Discoveries Relating to Vision, Light and Colours*, 1772, 700-704; see also W. Porterfield, *A Treatise on the Eye*, 1759, II, 381-384.

⁶ F. Hillebrand, Theorie der scheinbaren Grösse bei binocularen Sehen, *Denkschr. d. kais. Akad. d. Wiss. zu Wien*, math.-nat. Kl., 72, 1902, 255-307.

⁷ E.g., W. Blumenfeld, Untersuchungen über die scheinbare Grösse in Sehraume, *Zsch. f. Psychol.*, 65, 1913, 241-404, who also gives an excellent history of this problem, 243-274.

⁸ R. H. Thouless, Phenomenal regression to the real object, *Brit. J. Psychol.*, 21, 1931, 339-359; 22, 1931, 1-30.

a compromise between the proximal datum and objective constancy. Brunswik⁹ has stressed this compromise in his concept of an intermediate perceptual object, the *Zwischengegenstand*, and his associate Holaday¹⁰ has shown the properties of the *Zwischengegenstand* to depend upon a variety of perceptual data.

Size constancy is thus an hyperbole, except as the description of a limiting case. It is not a general rule. Only at times does the organism succeed in seeing an object with no change of size at all when distance is altered. On the other hand, these remarks apply with equal force to the *law of the visual angle*, the relation which prompted Fechner and Hering to make their original observations on the relation of size to distance. This law, too, is a special case. Let us scrutinize these cases.

Accommodated objects which subtend equal visual angles are equal in apparent size. That is the law of the visual angle. If the angle Θ_s , subtended by a standard stimulus, is equal to the angle Θ_c , subtended by a comparison stimulus, then

$$\tan \Theta_c = \tan \Theta_s \dots\dots\dots [A]$$

and

$$S_c = (D_c/D_s)S_s \dots\dots\dots [B]$$

where S_c is the linear size of the comparison object; S_s , the linear size of the standard; D_c , the distance from O to S_c ; and D_s , the distance from O to the standard S_s . The size of the comparison stimulus (= apparent size of the standard) is equal to the size of the standard stimulus multiplied by the ratio of their respective distances.

The law of size constancy, on the other hand, states simply

$$S_c = S_s \dots\dots\dots [C]$$

where S_c and S_s have the same meaning as in Equation [B]. Equation [C] expresses exactly the idea communicated to many investigators by the term *size constancy*. The size of the comparison stimulus is equal to the size of the standard, irrespective of their distances from O .¹¹

Fig. 1 shows these two relations in the way in which they are presented later in the present paper. The standard and comparison stimuli are circular in outline, uniformly and equally illuminated as O sees them. The diameter of the standard stimulus subtends a constant visual angle ($\Theta_s = 1^\circ$). S_c is the size, in inches, of the comparison stimulus. D_c is constant at 10 ft. D_s is varied from 10 to 120 ft. The broken line drawn parallel to the axis of abscissas is the locus of all data which obey the law of the visual angle. The oblique line is the locus of values conforming to the law of size constancy.

If size constancy were a general rule, then the apparent size of the standard

⁹ E. Brunswik, Die Zugänglichkeit von Gegenständen für die Wahrnehmung und deren quantitative Bestimmung, *Arch. f. d. ges. Psychol.*, 88, 1933, 377-418.

¹⁰ B. E. Holaday, Die Grössenkonstanz der Schdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen, *Arch. f. d. ges. Psychol.*, 88, 1933, 419-486.

¹¹ For the more general mathematical implications of the principles of size constancy, see E. G. Boring, Size constancy and Emmert's law, this JOURNAL, 53, 1940, 293-295.

stimulus (*i.e.* the measured size of the comparison stimulus after the subjective equation is made) should be related to D_s by a function that is linear in form (slope = $\tan 1^\circ$). If, on the other hand, the law of the visual angle were of general validity, then S_c should be constant, *i.e.* independent of distance. What actually happens is to be found for specific conditions. Systematically determined relations of this sort are wanted and wanting.¹² It must be kept in the mind that the

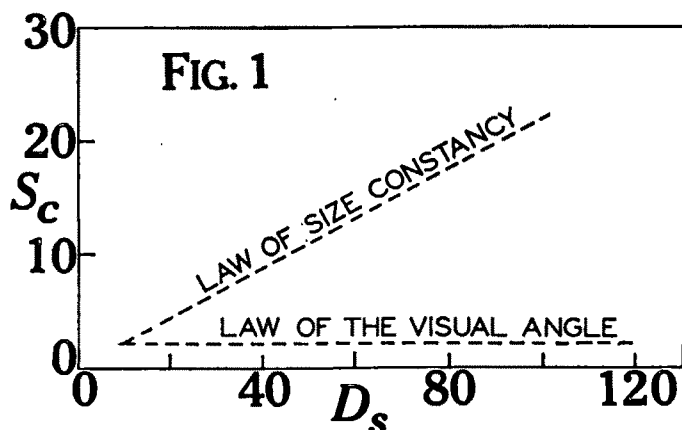


FIG. 1. LAWS OF VISUAL ANGLE AND OF SIZE CONSTANCY FOR OBJECTS OF ONE DEGREE

S_c is the diameter in inches of the comparison stimulus as equated in perceived size to the diameter of a standard stimulus (angle subtended = 1°). The comparison stimulus is at a constant distance (10 ft.) from O . The abscissa values are the distances in feet from O to the standard stimulus. The oblique broken line designates the locus of all data obeying the law of size constancy. The broken line parallel to the axis of abscissas is the locus of all points obeying the law of the visual angle.

arrangement of the experiment is not in the usual form for testing size constancy, since the visual angle subtended by the standard stimulus is kept constant at 1° , so that the physical size of the standard stimulus must be increased proportionally to the distance. It is for this reason that size constancy is represented in the graphs by a straight line through the origin with slope equal to $\tan 1^\circ$, and the law of the visual angle is a horizontal line with slope equal to zero.

The present paper is a study of such functions, obtained under conditions in which distance is a common variant, as various effects of binocular regard, of accommodation, and of the visual frame of reference are successively eliminated. Functions relating the size of an adjusted stimulus

¹² Koffka, *op. cit.*, 1935, 91, complained of the lack of complete data for this functional relation: "Although the first experiments of this kind were made in 1889 by Götz Martius, we have to the present day no complete knowledge of the quantitative relations, the range of distances over which the investigations have been carried out being rather limited."

to the distance from O to a standard stimulus subtending a constant visual angle ($\theta_s = 1^\circ$) were studied under four different sets of conditions: (1) binocular regard, (2) monocular regard, (3) monocular regard through an artificial pupil, and (4) monocular regard through an artificial pupil and a long black reduction tunnel stretching from O to the standard stimulus, eliminating most of the visual frame of reference. The consequent data provide quantitative functions for a greater range of distances than has heretofore been available for such a variety of conditions.

PROCEDURE

The general plan of the experiment is sketched in Fig. 2. O sat in a chair at the intersection of two long darkened corridors where he had an unobstructed view

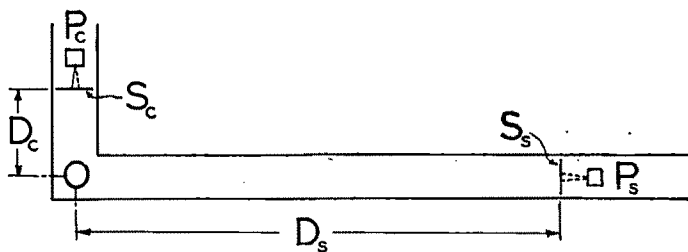


FIG. 2. PLAN VIEW OF THE CORRIDORS

S_c indicates the position of the comparison stimulus located at a constant distance ($D_c = 10$ ft.) from O . S_s at a distance D_s from O , indicates one of the positions occupied by the standard stimulus. The standard stimulus always subtended a visual angle of 1° . Distance from O to the standard was varied from 10 to 120 ft. P_c and P_s indicate the positions of the projectors.

of a standard and a comparison stimulus. The comparison stimulus S_c , a uniformly illuminated circular light-image, was centered on a large white screen (8 x 8 ft.) by an ordinary projector. The screen stood at a constant distance (10 ft.) from O throughout the experiment. The image on this screen could be continuously varied in size by means of an iris diaphragm conjugate with the screen. The standard stimulus S_s was provided in a similar manner by another projector. The distance from the O to S_s , however, was not constant but was systematically varied by placing the screen at various fixed distances, ranging from 10 to 120 ft. The light images for these stimuli were formed by means of circular apertures cut in thin brass plates conjugate with the standard screen. At all distances, S_s subtended a constant angle (1°) at the eye of O . The intensity of the light (flux per unit area) from S_s was constant and equal to that from S_c . The intensities of the light from S_s and S_c at the eye of O were thus identical for all measurements.

E regulated the size of the comparison stimulus by varying the opening of the adjustable diaphragm conjugate with S_c until O signified that the standard and comparison stimuli were perceived as equal in size. O first fixated the standard stimulus (1°), then the comparison, looking back and forth until satisfied with the equation.

E measured the diameter of S_c with a meter stick. No restriction was imposed upon *O* in regard to the length of time taken for the judgments.

All experiments were performed after midnight. Except for a few high lights, the corridors were dark. The brightest high lights were formed by light reflected from the waxed surface of the dark green tile on the floor of the corridor. Thus constellations of light images, not simply the primary images of the 'stimulus,' were located on the *O*s' retinas.

Five *O*s were employed: A. C. S. Holway, L. M. Hurvich, M. J. Zigler, A. H. Holway, and E. G. Boring. E.G.B. and M.J.Z. served as *O*s for the first complete sets of measurements. For them, 20 measurements for size were made at every

TABLE I
BINOCULAR OBSERVATION: APPARENT SIZE OF STANDARD STIMULUS AS A FUNCTION OF ITS DISTANCE

D_s = distance (ft.) from *O* to standard stimulus. At all distances, standard stimulus subtended a constant visual angle of one degree. S_c = av. size (in.) of *N* settings of comparison stimulus, located at a distance of 10 ft. from *O* and equated in perceived size to standard stimulus. Intensity of light from the stimuli was constant at eye of *O*. m.v. = mean variation. *O* sat erect, facing the stimuli successively with direct binocular regard.

D_s	E.G.B.		A.C.S.		A.H.H.		L.M.H.		M.J.Z.	
	(N=20)		(N=5)		(N=10)		(N=5)		(N=20)	
	S_c	m.v.	S_c	m.v.	S_c	m.v.	S_c	m.v.	S_c	m.v.
10	2.2	0.21	2.2	0.19	2.2	0.19	2.2	0.14	2.4	0.18
20	4.6	0.44	4.8	0.40	4.7	0.32			4.5	0.45
30							7.0	0.44		
40	9.5	0.48			9.4	0.81			8.9	0.35
50			13.5	0.42			12.0	0.51		
60	11.5	0.71	13.2	0.60	15.0	0.71			13.7	0.48
70			15.9	0.39			15.5	0.62	16.4	0.75
80	15.8	0.70			17.6	0.37			19.8	0.36
90			17.1	0.55					19.9	1.08
100	18.7	1.13			24.0	0.93	23.0	0.93	25.3	0.93
120	20.6	1.07	25.5	0.66	24.5	1.02	25.2	1.16	28.4	1.75

distance; 10 were made by increasing the size of S_c until *O* reported that the visual impression produced by it was equal in extent to that produced by S_s ; 10 more were made by decreasing the size of S_c . The two procedures gave practically identical results, and for each distance the 20 measurements were averaged to obtain the desired measure of central tendency. A smaller number of results was secured from the other *O*s.

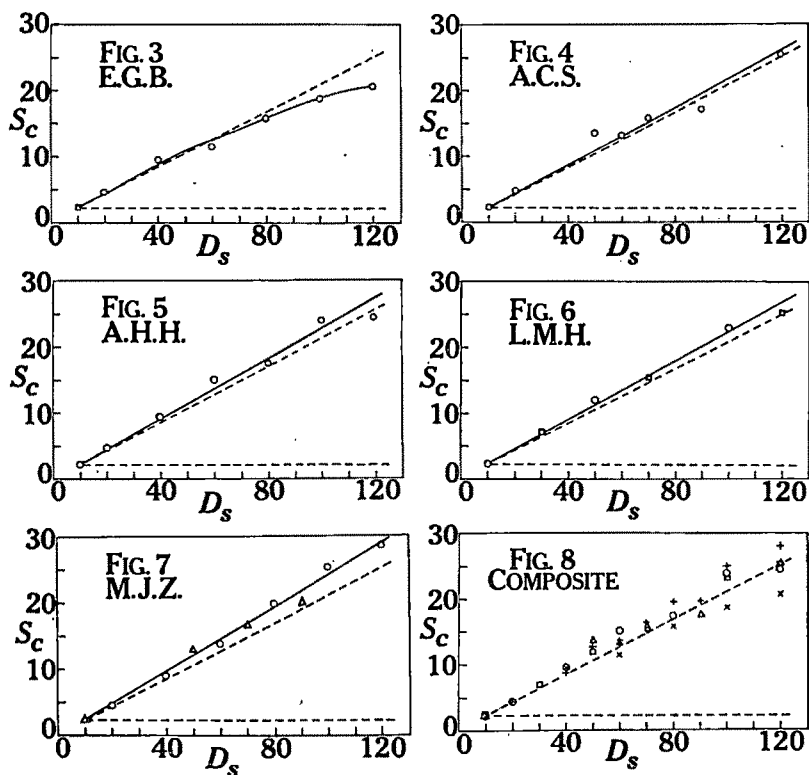
BINOCULAR OBSERVATION

The measurements for the binocular observations are shown in Table I. These data were obtained with binocular regard by altering the diameter of the comparison stimulus S_c , until it appeared equal to the standard stimulus (1°) in respect of perceived size, as the distance from *O* to the standard stimulus was varied from 10 to 120 ft.

The measurements for each *O* are also exhibited in Figs. 3-7. The coordinates are expressed in linear units. The ordinates give the size of the

comparison stimulus (in.), the abscissas the distance (ft.) from O to the standard stimulus. Fig. 8 is the composite of the binocular data for all O s.

The size of the comparison stimulus S_c in all instances increases with the distance of S_s from O . In other words, the apparent size ($= S_c$) of a standard stimulus varies



FIGS. 3-8. BINOCULAR OBSERVATION: APPARENT SIZE OF THE STANDARD STIMULUS AS A FUNCTION OF ITS DISTANCE

Figs. 3-7 show the apparent size of the standard stimulus as its distance from O is varied from 10 to 120 ft. Standard stimulus subtended a constant angle of 1° at eye of O . Circles are for first sitting; triangles and rectangles are for later sittings. For values of N , see Table I. The oblique broken line is the locus of all data obeying the law of size constancy. The broken horizontal line is the locus of all data conforming to the law of the visual angle. Fig. 8 is a composite of the data for all O s. Different symbols denote different O s.

directly with the distance from O to the standard. The specific form assumed by the majority of these functions is surprising, since commonplace experience would lead one to expect diminishing returns for S_c at great distances. Except for the data of E.G.B., which exhibit a curvature that is concave toward the axis of the abscissas,

the values of S_e are related to the distance of the standard stimulus by a linear function.

Much has been said concerning the likelihood of securing results intermediate between the limits of size constancy and the visual angle. The binocular data for most of our O s do not, however, lie within these limits, but *outside* of them. The most probable function is not only linear in form but it also has a slope greater than that demanded by the law of size-constancy.¹³

At first thought, it seems as if we had here to do with a case in which apparent size is 'more than constant,' that is to say, a case where the apparent size of a receding object increases slightly while the retinal image diminishes greatly. It is quite possible, however, that there is in our equations a space error, such that S_e on O 's left is seen a little smaller than an S_e of equal physical size on O 's right, so that for subjective equation S_e has to be made a little too large. The discrepancy is of the order 1.12:1, *i.e.* a value for binocular observation is about 1.12 times the corresponding theoretical value for size constancy. In two other similar experiments we have encountered this discrepancy in this direction once and failed to find it once.¹⁴ The chief argument for suggesting that a space error may be operative is that there is available no other sensible interpretation of why constancy should be 'exceeded.' It is our belief that this error—if indeed it be an error, for we have no other evidence than the foregoing—is not psychophysical but instrumental, like some mistake in the distance from O 's head to the screen for the comparison stimulus. If such be the source, then the error would also apply to the other three functions discussed below, and a correction would be achieved by a slight clockwise rotation of the functions.

MONOCULAR OBSERVATION

During the monocular observations, O wore a leather stop which completely covered one eye. The apparatus, method, procedure and O s were the same as employed in the binocular observations. The results for the monocular equations with respect to size are entered in Table II.

Figs. 9-13 show the function that S_e is of D_s for each O under the conditions of monocular observation. The open circles represent the first measurements taken at the first sitting; the rectangles and triangles, measurements taken about one week later. The agreement between the first and second sets of measurements provides an index as regards the repro-

¹³ Analogous results for 'over-constancy' in color equations have been reported by W. Burzlaff, *Methodologische Beiträge zum Problem der Farbenkonstanz*, *Zsch. f. Psychol.*, 119, 1931, 177-235.

¹⁴ The discrepancy is measured by the ratio 1.12:1, which applies actually at $D_s = D_e = 10$ ft. as well as elsewhere. We would seem truly to be dealing with a space error and not with a phenomenon of size constancy since the deviation occurs when the standard and comparison stimuli are equidistant. In certain other unpublished experiments with this apparatus we got no space error of this sort. In still other experiments, A. H. Holway and E. G. Boring. The dependence of apparent visual size upon illumination, this JOURNAL, 53, 1940, 587-589, we found a space error in this direction ranging from 1.02 to 1.15, when $D_s = D_e = 100$ ft., and also 200 ft.

ducibility of the functions. The data for each O can be fitted with a straight line. No solid lines have been drawn for the data of A.H.H. and L.M.H. (Figs. 11 and 12) since the trend so nearly parallels the law of size constancy. While the results for E.G.B. and A.C.S. tend to lie below the hypothetical line for constant size, those for M.J.Z. lie slightly above it. The scatter of the measurements about the fitted line varies inversely with

TABLE II
MONOCULAR OBSERVATION: APPARENT SIZE OF STANDARD STIMULUS AS A FUNCTION
OF ITS DISTANCE
(See Table I for explanation of symbols.)

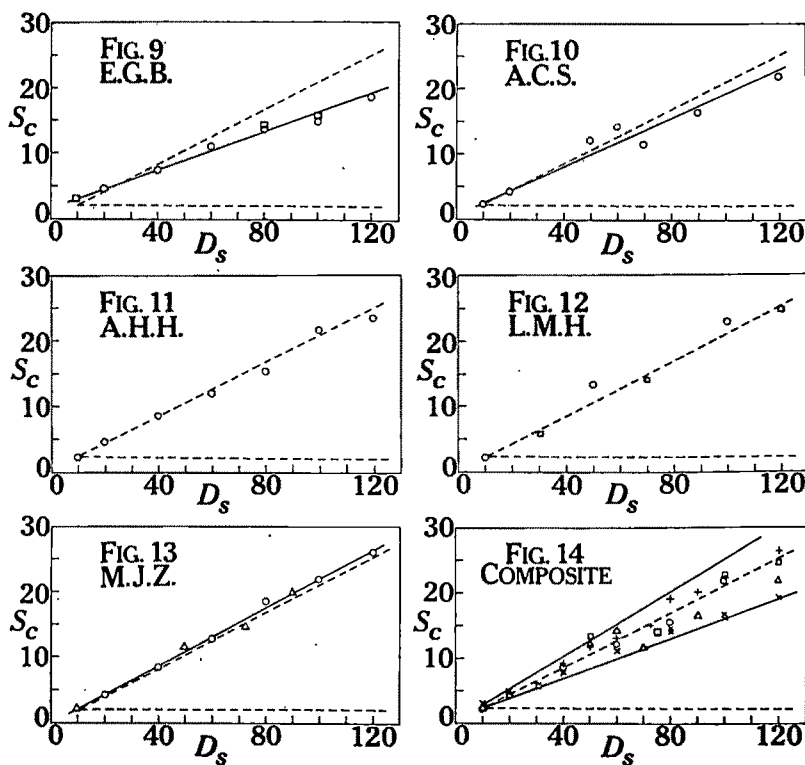
D_s	E.G.B. ($N=20$)		A.C.S. ($N=5$)		A.H.H. ($N=10$)		L.M.H. ($N=5$)		M.J.Z. ($N=20$)	
	S_c	m.v.	S_c	m.v.	S_c	m.v.	S_c	m.v.	S_c	m.v.
10	3.0	0.27	2.1	0.17	2.0	0.15	2.1	0.18	2.1	0.20
20	4.7	0.40	4.3	0.36					4.1	0.34
30							5.8	0.38		
40	7.5	0.38			8.5	0.51			8.5	0.29
50			12.2	0.34			13.3	0.44	11.5	0.38
60	11.1	0.39	14.1	0.51	12.0	0.81			12.7	0.28
70			11.5	0.40			14.0	0.49	14.5	0.52
80	{ 12.8 13.5 }	{ 1.11 0.70 }			15.3	0.45			18.7	0.62
90			16.4	0.24					18.9	1.08
100	{ 15.0 15.8 }	{ 0.91 1.13 }			21.7	1.40	22.7	0.43	22.0	0.43
120	18.9	1.06	22.0	0.20	23.5	0.75	24.7	0.79	26.1	0.55

the number of measurements. Thus, the data manifest the least departures for E.G.B. and M.J.Z. ($N = 20$), and the greatest departures for A.C.S. and L.M.H. ($N = 5$). The data for all O s are plotted in Fig. 14.

The size of the comparison stimulus is a linear function of the accommodated distance from O to the standard stimulus. The rate of change in the size of the comparison stimulus, with respect to the distance of the standard stimulus, is accordingly constant, and the slope of the straight line fitted to the average of all the data is 0.0170, which differs insignificantly from the tangent of 1° ($= 0.0175$). We cannot, however, conclude that the data for monocular observation closely follow the law of size constancy because of the probable existence of the constant error which we noted in the preceding section. The data for binocular observation are presumably closest to size constancy among the sets of data for the four conditions of this experiment; hence these data for monocular vision, when compared with those for binocular vision, represent a regression away from the law of size constancy toward the law of the visual angle.

MONOCULAR OBSERVATION WITH AN ARTIFICIAL PUPIL

In these experiments, the *O* wore a leather stop over one eye and successively observed the standard and comparison stimuli through an artificial



FIGS. 9-14. MONOCULAR OBSERVATION: APPARENT SIZE OF THE STANDARD STIMULUS AS A FUNCTION OF ITS DISTANCE

Figs. 9-13 show the apparent size of the standard stimulus as its distance from *O* is varied from 10 to 120 ft. Standard stimulus subtended a constant angle of 1° at eye of *O*. Circles are for first sitting; triangles and rectangles are for later sittings. For values of *N*, see Table II. The oblique broken line is the locus of all data obeying the law of size constancy. The broken line parallel to the axis of abscissas is the locus of all data conforming to the law of the visual angle. Fig. 14 is a composite of the data for all *O*s. Different symbols denote different *O*s.

pupil worn in front of the other eye. The pupil was 1.8 mm. in diameter, an aperture in a thin metal disk, which was held in a fixed position at a distance of about 4 mm. from the anterior corneal surface of *O*'s eye by one of the curled rims on a pair of adjustable trial-frames. The results obtained

under these conditions are presented in Table III, and graphically in Figs. 15-19.

Fig. 20 contains the results for all *O*s. A comparison of the data in Fig. 20 with the data in any one of the Figs. 15-19 shows that the inter-individual variability exceeds the variability in the data for any individual *O*. These data, lying well below the theoretical line for size constancy,

TABLE III
MONOCULAR OBSERVATION WITH ARTIFICIAL PUPIL: APPARENT SIZE OF STANDARD
STIMULUS AS A FUNCTION OF ITS DISTANCE
(See Table I for explanation of symbols.)

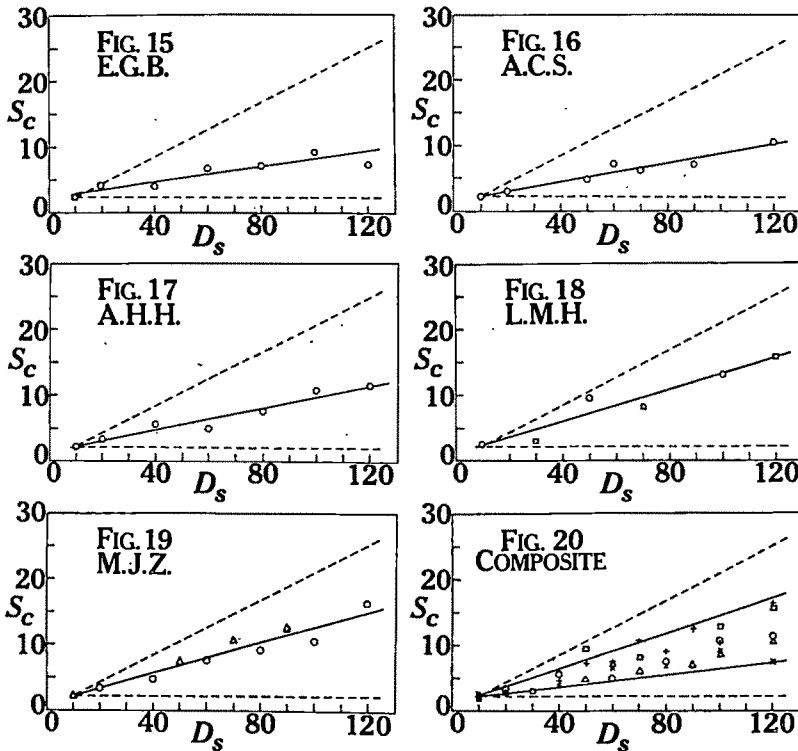
D_e	E.G.B. ($N=20$)		A.C.S. ($N=5$)		A.H.H. ($N=10$)		L.M.H. ($N=5$)		M.J.Z. ($N=20$)	
	S_e	m.v.	S_e	m.v.	S_e	m.v.	S_e	m.v.	S_e	m.v.
10	2.4	0.18	2.2	0.20	2.1	0.18	2.5	0.11	2.2	0.14
20	4.1	0.44	3.0	0.19	3.3	0.27			3.3	0.27
30							3.0	0.19		
40	4.0	0.45			5.6	0.28			4.8	0.20
50			4.8	0.40			9.5	0.36	7.3	0.32
60	6.8	0.39	7.2	0.37	5.0	0.29			7.7	0.36
70			6.2	0.29			8.2	0.34	10.8	0.59
80	7.1	0.67	7.2	0.35	7.7	0.39			9.2	0.35
90									12.6	0.60
100	9.3	0.53			10.8	0.57	12.9	0.52	10.6	0.67
120	7.4	0.39	10.6	0.65	11.6	0.60	15.7	0.80	16.4	0.92

represent a further regression toward the law of the visual angle, a regression that results from the introduction of the artificial pupil as an observational constraint.

MONOCULAR OBSERVATION WITH ARTIFICIAL PUPIL AND REDUCTION TUNNEL

The experiments described in the foregoing sections were carried out in reduced illumination. At *O*'s eye the intensity of the light reflected from the surroundings on to a magnesium oxide plate was less than 10^{-3} millilamberts. The reflectance of the walls, ceilings, and floors of the corridors, however, was not uniform as viewed from *O*'s position. The consequence was that there were formed various light patterns which provided a sensory ground for the perception of the stimulus. It is plain that the addition of extraneous sensory stimulation of this sort—especially when it might even constitute a vague perception of the space intervening between *O* and the stimulus—ought to be eliminated if we are concerned with an analysis of the parts played by various cues. In other words, we ought to attempt still further to 'reduce' the perception to its fundamental feature, which is the

retinal image, just as color constancy and brightness constancy may be 'reduced' to bare retinal excitation by the use of a 'reduction screen.' A reduction screen is a screen with a hole in it. We determined to build a reduction tunnel, a long black tube which would eliminate the perception



FIGS. 15-20. MONOCULAR OBSERVATION WITH ARTIFICIAL PUPIL: APPARENT SIZE OF THE STANDARD STIMULUS AS A FUNCTION OF ITS DISTANCE

Figs. 15-19 show the apparent size of the standard stimulus as its distance from O is varied from 10 to 120 ft. Standard stimulus subtended a constant angle of 1° at eye of O . Circles are for first sitting; triangles and rectangles are for later sittings. For values of N , see Table III. The oblique broken line is the locus of all data obeying the law of size constancy. The broken line parallel to the axis of abscissas is the locus of all data conforming to the law of the visual angle. Fig. 20 is a composite of the data for all O s. Different symbols denote different O s.

of reflected light from the surfaces of the corridor, and presumably therefore 'reduce' observation more nearly to the retinal image alone.

Accordingly a long tunnel was constructed to reduce the number and intensity of these extraneous images. The side walls of the tunnel were

made of heavy black cloth. The whole affair was supported by steel rods welded together in the form of a square and held up by wooden posts. The greatest length of the tunnel was 100 ft. For shorter distances, the cloth was folded back on itself. Each side of the tunnel measured 3 ft.

In order to determine the nature of the new visual field, we had *O* look down the tunnel monocularly through the artificial pupil, fixate the center

TABLE IV
MONOCULAR OBSERVATION WITH ARTIFICIAL PUPIL AND REDUCTION TUNNEL: APPARENT
SIZE OF STANDARD STIMULUS AS A FUNCTION OF ITS DISTANCE
(See Table I for explanation of symbols.)

D_s	$\frac{\text{A.C.S.}}{(\overline{N}=20)}$		$\frac{\text{A.H.H.}}{(\overline{N}=20)}$	
	S_c	m.v.	S_c	m.v.
10	2.2	0.19	2.2	0.21
50	4.4	0.37	3.4	0.32
100	6.0	0.55	6.8	0.67

of the standard stimulus, and report the appearance of the field. The standard stimulus was seen standing out in clear relief against the darkened background. Most of the various reflections were diffuse and greatly reduced in intensity. Nonetheless, there still remained a perceptible (and annoying) haze which surrounded the primary stimulus. This border-like haze took on the form of a square and was localized midway between the *O* and the standard stimulus.

The results obtained by equating the perceived size of the stimuli under these conditions are presented in Table IV and Fig. 21 for two *O*s, each of whom worked entirely without knowledge of the other's results. Measurements were made at three distances, 10, 50 and 100 ft. The slope of this line, which indicates the trend of these observations, is less than for any of the other conditions. These results are, obviously from Fig. 21, closer to the law of the visual angle than they are to the law of size constancy. They show that the tunnel actually did 'reduce' the perception, although not entirely to the visual angle.

DISCUSSION

The net result of these experiments is exhibited in Fig. 22, which shows the functions for the various conditions brought into relation with each other as straight lines with different slopes. These functions summarize about 1,500 measurements altogether. Their slopes diminish regularly if the functions are considered in the order in which they have just been discussed. How are we to interpret such a relationship?

In 1911 Katz introduced into the psychology of perception the concept of *reduction*.¹⁵ The perceived phenomenon is a resultant of many determinants. Some one of them may be *primary* in the sense that it is essential to the perception although it may not play the principal role in determining

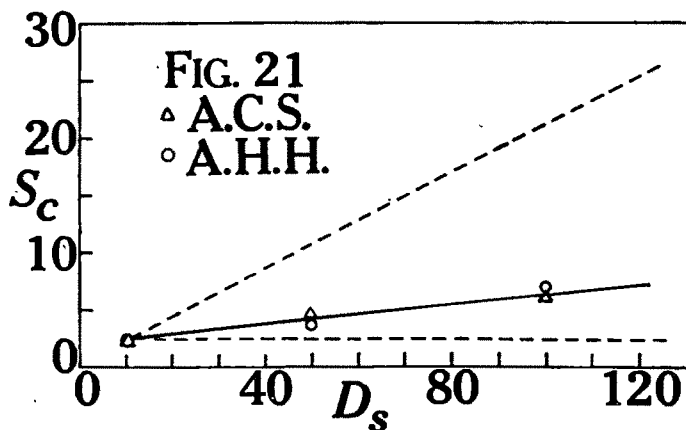


FIG. 21. MONOCULAR OBSERVATION WITH ARTIFICIAL PUPIL AND REDUCTION TUNNEL: APPARENT SIZE OF THE STANDARD STIMULUS AS A FUNCTION OF ITS DISTANCE

The figure shows the apparent size of the standard stimulus as its distance from O is varied from 10 to 100 ft. The standard stimulus subtended a constant angle of 1° at eye of O . The oblique broken line is the locus of all data obeying the law of size constancy. The broken line parallel to the axis of abscissas is the locus of all data conforming to the law of the visual angle. The circles represent the data for A.H.H.; the triangles for A.C.S. Each plotted datum is based on 20 measurements.

the exact form or quality or amount of the perception. For instance, in the case of visual brightness, which Katz was considering, illumination of the perceived object is a primary factor, yet the phenomenal brightness is actually determined by many other factors that enter otherwise into the perception. If some of these additional determinants can be eliminated, then the perception can be reduced in the direction of the primary determinant. So Katz invented the "reduction screen," a screen with a hole in it so arranged that, when a colored surface is seen through the hole and all the circumstances of its relation to the surrounding field are excluded by the remainder of the screen, then the brightness, instead of remaining "constant" at the value proper for the perceived object, is "reduced" to a datum dependent almost entirely upon the actual retinal illumination.

¹⁵ D. Katz, Die Erscheinungsweisen der Farben, *Zsch. f. Psychol.*, Ergbd. 7, 1911, esp. 36-39.

In a similar manner we may regard the present series of conditions as representing successive reductions of the size perception. Let us list these conditions in order, adding as additional items the two theoretical limits of variation. Here the primary determinant is, of course, visual angle or retinal size. It is toward it that reduction is undertaken.

(1) *Size constancy*. It is possible that size constancy represents one limit of variation, that perceptual organization, as Brunswik has suggested,¹⁶

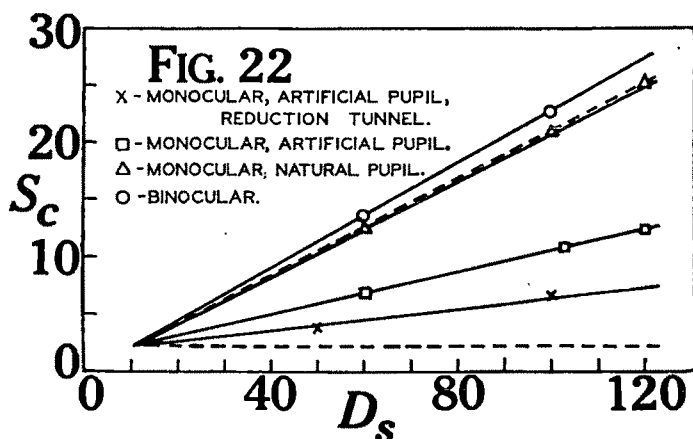


FIG. 22. DETERMINANTS OF APPARENT VISUAL SIZE WITH DISTANCE VARIANT

Apparent size as a function of distance for four sets of conditions. The figure is based on the averages of all the data obtained in the present experiment. The slope of the function relating apparent size to distance diminishes continuously as the mode of regarding the stimuli is altered from direct binocular observation, to direct monocular observation, to monocular observation through a small artificial pupil, to monocular observation through the artificial pupil and a long black reduction tunnel. As the number of extraneous cues is diminished, the slope of the function approaches zero as a limit, *i.e.* it approaches the law of the visual angle.

occurs in the interest of stabilizing the perceptual world. The organism utilizes, therefore, additional 'cues' which tend to keep the apparent size of an object constant when its visual angle varies with changing distance. According to this view, we should not expect to find an over-compensation, by which a receding object would increase in apparent size while its retinal image diminished.

¹⁶ E. Brunswik, Die Zugänglichkeit von Gegenständen für die Wahrnehmung und deren quantitative Bestimmung, *Arch. f. d. ges. Psychol.*, 88, 1933, 377-418. See esp. the section on intendierte und intentional erreichte Wahrnehmungsgegenstände, pp. 378-387, and the section on Bestimmung des intentional erreichten Gegenstandes, pp. 387-411, which deals with der Grad der Dingkonstanz. The Helmholtzian conception of unbewusster Schluss seems by this route to be reentering the psychology of perception.

(2) *Binocular observation.* Free binocular observation presumably employs all the determinants available, and would thus be, as we find, the least reduced perception. It might therefore achieve size constancy or fall short. Our data, however, show over-compensation; the slope of the function for binocular observation exceeds the slope of the dotted line for size constancy in Fig. 22. This position of the line for binocular observation, as we have already noted, may merely indicate the existence of a space error in the experiment. If that assumption be correct, then the 'true' function for these data of binocular observation lies with the line for size constancy or below it.

(3) *Monocular observation.* The stopping of vision from one eye is the first step in reduction of the perception. By it all binocular retinal conditions for the perception of distance are eliminated. It is obvious that the reduction of the perception from size constancy toward the law of the visual angle must depend upon the elimination of cues to distance, for the organism can compensate for diminution of retinal size with increase of distance only if cues to distance are available—only if it 'knows' how far away the object is and thus how much to compensate for distance. Reduction, therefore, must consist mainly in the removal of cues to distance.¹⁷

(4) *Artificial pupil.* The use of an artificial pupil with monocular observation still further reduces the slope of the function (Fig. 22) and may thus be supposed to eliminate some more cues to the distance of the standard stimulus. Perhaps the stopping down of the pupil makes accommodation less effective and thus reduces its effectiveness as a differentia of distance.¹⁸

(5) *Reduction tunnel.* Some faint illumination from the stimuli was visible by reflection from the surfaces of the corridor. The long black tunnel, 3 x 3 ft., was designed to eliminate these cues to distance and thus to reduce the perception entirely to retinal size. The result was not entirely successful. There was still a light haze visible within the tunnel which conceivably may have provided an indication of distance. At any rate the slope of the function was not reduced to zero. Nevertheless, with the tunnel the perceptual field became much more homogeneous, and the slope of the function was greatly diminished.

¹⁷ Monocular observation also reduces apparent size because it reduces the total retinal illumination; cf. Holway and Boring, *op. cit.*, this JOURNAL, 53, 1940, 587-589. Since it affects both the standard and comparison stimuli, leaving their relation unchanged, it need not be considered here.

¹⁸ The artificial pupil also reduces apparent size by reducing retinal illumination, but, like the reduction in monocular vision, it affects both the standard and comparison stimulus and can be ignored here.

(6) *Visual angle*. Retinal size, as indicated by the visual angle, must be the limit of reduction and yield a function in which the slope is zero. For all that has been said by Gestalt psychologists against the validity of the law of the visual angle, it would nevertheless appear that, when no relevant datum other than retinal size is available, then the perception of size will after all vary solely with the visual angle. That statement is a tautology and must be true. Size constancy can be the law of size, therefore, only when determination is complex.

The following ratios show the relative slopes of the functions under discussion. To them are added the values for the theoretical laws of size constancy (1.00) and of visual angle (0). The "actual data" are the slopes as they occur in Fig. 22. The "adjusted data" are the slopes as they would be if we correct for a possible space error by rotating the functions clockwise through an angle of 10', so as to bring the line for binocular observation to where we think it should be, *i.e.* below the line for size constancy. These adjusted data are also plotted in Fig. 23.

	Size constancy	Binoc. obs.	Monoc. obs.	Artif. pupil	Red. tunnel	Vis. angle
Actual data	1.00	1.09	.98	.44	.22	0
Adjusted data	1.00	.93	.81	.30	.08	0

Our general conclusion from all these data is hardly more than a restatement of the obvious. The organism can perceive the size of an object as constant, even though its distance changes, provided the perception is complex enough to provide the essential differentiae. When the perception is reduced by the elimination of some of these determinants, the law of the variation of apparent size with distance approaches the law of variation of the remaining determinants. If the perception could be reduced to a single determinant—retinal size or any other—then apparent size would have to vary in accordance with the mode of variation of this sole remaining determinant. There is no alternative hypothesis.

SUMMARY

The apparent size of a standard stimulus subtending a visual angle of one degree was measured as the distance of the standard was varied from 10 to 120 ft. Functions relating apparent size to distance were obtained from 5 Os under four different sets of conditions: (1) direct binocular regard; (2) monocular regard; (3) monocular regard through a small artificial pupil; and (4) monocular regard through the artificial pupil and a long black reduction tunnel. For each of these conditions the most probable form of the function relating apparent size to distance was found to be linear.

These conditions, considered in the order in which they have been named, represent a serial reduction of the size perception. In binocular regard apparent size is the resultant of the interoperation of many determinants, which are successively reduced in number in the three remaining sets of conditions. This reduction is paralleled by a consistent change in the slope of the line that relates apparent size to distance. The limits of variation of this slope are—at least approximately—(1)

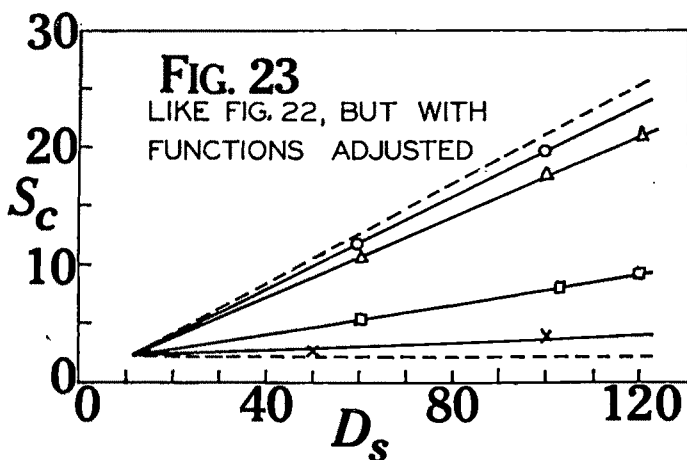


FIG. 23. DETERMINANTS OF APPARENT VISUAL SIZE WITH DISTANCE VARIANT

This figure is the same as Fig. 22, except that the four functions have been rotated clockwise through an angle of 10 min., in order to bring the function for binocular observation below the function for size constancy, as we think it would have to be, and as correction for a space error might require.

the function for size as constant in spite of change of the perceived object's distance and (2) the function for size as proportional to the visual angle subtended by the perceived object. Binocular regard gave a function close to the function for size constancy. Reduction of the perception from binocular regard brought the function nearly, but not entirely, to the slope for apparent size as wholly dependent upon retinal size.

JUDGMENT OF DIFFICULTY OF SIMPLE TASKS

By J. P. GUILFORD, University of Southern California,
and MILTON COTZIN, Cornell University

For some time we have been able to evaluate the difficulty of simple tasks, such as items in a mental test, on a linear, rational scale.¹ In scaling an item, the procedure begins with the determination of the proportion of a specified population that fails to pass the item. No doubt the testees who attempt the item have some subjective impression of how 'hard' the item is. For those who succeed, or think they succeed, the item would seem relatively easy, but for those who fail, or think they fail, the item would seem relatively difficult. Theoretically, we could derive some indication of the subjective impression of difficulty from every testee, and could then average those indicators so as to determine the central tendency of impression of difficulty for this particular population. Would such evaluations of difficulty bear any simple functional relationship to the measurements of difficulty derived from the same population by traditional scaling methods? This is our central problem.² A subsidiary problem is whether there is an absolute impression of difficulty which is independent of the kind of item or task.

A hypothesis. A first guess might be that the functional relationship between actual difficulty and appreciated difficulty would be linear in form. One consideration to support such a hypothesis in a very general way is that both variables are measured on scales of equal psychological units; neither is measured on a physical scale. Another basis for the linear assumption lies in a finding of the senior author that the scaled difficulties of test items bear a logarithmic relationship to physical magnitudes of stimuli, when the items consist of differences in pitch, intensity, and time-intervals, and of short musical melodies in the Seashore tests of musical talent, in which the physical magnitudes of the differences are known.³ In order to supply a basis for the linear hypothesis, we must consider these findings in connection with the Fechnerian law, that stimuli impress us in proportion to the

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¹ For a brief account of procedures of scaling test items for difficulty, see J. P. Guilford, *Psychometric Methods*, 1936, 440-445.

² This paper rests in large part upon a Master's thesis by the junior author entitled, "The relationships between mental difficulties of tasks involving different sense modalities," on file in the University of Nebraska Library.

³ Guilford, The psychophysics of mental test difficulty, *Psychom.*, 2, 1937, 121-133.

logarithms of their magnitudes—if we state the law in its broadest terms.

To make the application of Fechner's law to appreciation of difficulty, let us consider the favorite illustration of lifted-weights. When an *S* lifts a weight to judge its magnitude, absolutely or relatively, will it make any difference in the general psychophysical law whether we ask him to judge in terms of (1) physical value of the weight, (2) intensity of his sensations, or (3) amount of effort required to lift it? We shall have to grant that so far as his criteria of judgment are concerned, as well as his differential sensitivity and his proneness to commit the stimulus-error, there might be some differences in the results. But, assuming *S*'s ignorance of the logarithmic law and the absence of other sources of bias, we may venture to say that the same psychophysical law would apply in all cases.⁴ If this be granted, average subjective evaluations of difficulty of test items should bear a linear relationship to measurements of difficulty based upon proportions of failures, for judgments of effort required to perform a task may be assumed to be identical with or in linear relation to judgments of difficulty.

Only a few previous studies bear upon our major problem or lend proof or disproof of our hypothesis. Fullerton and Cattell called for judgment of confidence on a scale of four points immediately following the rendering of psychophysical judgments.⁵ Henmon used a similar procedure.⁶ Since one cannot be sure that these ratings yielded measurements of confidence on a metric scale of equal units, the form of the regression of confidence on objective difficulty is in doubt, although there is a strong correlation between the ratings and the proportion of correct judgments. As a matter of fact, Fullerton and Cattell's data gave a clearly linear regression while those of Henmon gave a curved regression with positive acceleration. In citing these studies of confidence as background for our own study of subjective difficulty, we are making the assumption that confidence and felt difficulty are negatively and linearly correlated.

Jersild⁷ and Greene⁸ used three categories of judgment of confidence. The confidence ratings correlated positively with the proportion of correct responses in true-false examinations in psychology courses. The regressions were linear with scaled measurements of difficulty after corrections for

⁴ Some supporting evidence for this may be found in S. W. Fernberger's An experimental study of the 'stimulus error,' *J. Exper. Psychol.*, 4, 1921, 63-76.

⁵ G. S. Fullerton and J. McK. Cattell, On the perception of small differences, *Pub. Univ. Penn., Philos. Series*, No. 2, 1892.

⁶ V. A. C. Henmon, The relation of the time of a judgment to its accuracy, *Psychol. Rev.*, 18, 1911, 186-201.

⁷ A. Jersild, The determinants of confidence, this JOURNAL, 41, 1929, 640-642.

⁸ E. B. Greene, Achievement and confidence on true-false tests of college students, *J. Abn. & Soc. Psychol.*, 23, 1929, 467-478.

chance success in the proportions were made.⁹ Farmer¹⁰ and Hertzman¹¹ made studies in which judgments of difficulty and confidence were called for, but they established nothing beyond the general correlation between degree of success and degree of ease or confidence. A study of Davis,¹² which relates actual success and felt success to muscular tensions, will be mentioned more in detail in a later discussion of our own results.

PROCEDURE

After considering different kinds of test items as the simple tasks to be judged for difficulty, we selected as our material the Seashore tests of pitch, intensity, and time discrimination. As we have already noted, it is known that the objective difficulty of the items in these tests is inversely proportional to the logarithm of the stimulus-difference. It was advantageous for us to have, in addition to the scaled difficulty values, the physical values of the items in case they should be needed, for this is something that cannot be known for most kinds of test items.

The next question was to decide in what terms the Ss were to express their subjective impressions of difficulty. Rating-scale methods were considered and then discarded, since the question of absolute magnitude could not be satisfactorily settled in this manner. Because of the manner in which the Seashore phonograph records are arranged, the methods of rank order and of paired comparisons were out of the question. The method of single stimuli would have given us too little information for the amount of experimental time required. We, therefore, resorted to a method of matching one difficulty against another. A standard scale of difficulties was set up in the form of a series of weights to be lifted. The problem for S was to judge the tonal differences in the normal administration of the Seashore tests, and then to find a weight in the series that seemed equally hard to lift. We assumed that a series of lifted-weights provides a familiar and elemental set of graded experiences with respect to degrees of effort.

The weights were 34 in number, the lightest of which was 27.3 gm. and the heaviest 633.9 gm. The increments formed a geometric series, each weight being 10% heavier than the one just below it. Although we were not sure that this represented a scale of equal psychological units, we assumed that this was approximately true and a test to be described later verified our assumption. The weights were in the form of loaded tin ointment cans 2.75 in. in diam. They were arranged in serial order along the periphery of a circular, rotating, cloth-covered table.

Since the Seashore tests offer stimuli of the same magnitude in sets of 10, our procedure was to ask S to judge a set of 10 stimulus-differences, then immediately to select the weight that seemed to him to match in difficulty the set of test items. Because our Ss were mostly untrained in laboratory procedures, we framed the instructions in the following rather naïve manner:

⁹ Guilford, The determination of item difficulty when chance success is a factor, *Psychom.*, 1, 1936, 259-264.

¹⁰ E. Farmer, Concerning subjective judgment of difficulty, *Brit. J. Psychol.*, 18, 1928, 438-442.

¹¹ M. Hertzman, Confidence ratings as an index of difficulty, *J. Exper. Psychol.*, 21, 1937, 113-119.

¹² R. C. Davis, The relation of muscle action potentials to difficulty and frustration, *J. Exper. Psychol.*, 23, 1938, 141-158.

Instructions. "This experiment is to test your ability to equate mental difficulty with physical difficulty. You will be given several series consisting of 10 trials each taken from Seashore's *Measures of Musical Talent*. You are to judge the correct response to each trial according to the directions to be given. After each series of 10 trials, you will be asked to *find a weight which seems to you to be as difficult or easy to lift as the series was difficult or easy to judge*. Directions for the judgments of each measure of musical talent will be given before its series begins."

Two experiments were conducted, one with a group of 10 Ss who completed the tests two times at two sittings, and the other with one S who took the tests 10 times, in 10 sittings. Among the 10 Ss, five were men and five were women, five had taken the Seashore tests before and five had not, and five had had some special musical training and five had not. The tests were given in a small room which was relatively free from any significant extraneous noises and other distractions. Before beginning the test each S was permitted to familiarize himself with the series of weights by lifting. He was then given three practice trials, involving three sets of auditory stimuli which he judged, selecting immediately from the series the weight to match. The practice trials included first the easiest set of stimuli from the test, then one of medium difficulty, and finally the most difficult one. It was hoped by means of these preliminary trials to enable S to establish some general basis for future matching. After each matching the number of the selected weight (concealed on the bottom) was recorded, and the table was turned so that S lost sight of the one just selected before the next matching trial. S judged the auditory stimuli always while sitting and then selected the weight while standing, a posture that allowed more natural freedom in selecting and in lifting the weights.

In the test trials the sets of weights were given in an order that was predetermined. Three orders were established and these were rotated among the Ss and the sittings. In the second test period for the group of Ss, each S's order was the exact reverse of the one for the same test on the first occasion. In this manner we expected to counteract the influences of fatigue and learning. Short rest periods were introduced between sets of 10, and longer ones between tests. The varied order of presentation was also intended to forestall the influence of systematic habits that might be set up because of starting always with the same level of difficulty or following the same order. Similar procedures were followed for the one S who came for the tests 10 times.¹³

RESULTS

Scaling the items for objective difficulty. The first step was to determine the difficulty values of the items from the proportions of failures. In Table I, column 2, are listed the proportions of failures for the group and similarly in Table II for the individual S. Except for the extremely easy or extremely difficult items in the test of pitch discrimination, these proportions may be regarded as fairly reliable. Combined with the much greater range of difficulty in the pitch test is the fact that there were smaller numbers of judgments. Except for these limitations, the scaling seems adequate. The next step was to correct the proportions of failures for chance

¹³ We are grateful to Mr. Frank Dudek who served as the individual S.

success by multiplying them by a constant equal to $n/(n-1)$, where n is the number of alternative responses.¹⁴ The corrected proportions are given in the third column of the Table. The scaled values of difficulty are the deviates of the normal curve corresponding to the corrected proportions.

As a matter of interest, and also as a test of the validity of the scaled difficulty values, we next correlated them with the logarithms of the stimulus

TABLE I
DATA FROM THE GROUP OF 10 Ss

	Stimulus magnitude R	Proportion of failures q	Corrected proportion q'	Scaled value S	Median of matched weights* W	Variability of match- ings \bar{Q}
Pitch (N=200)	30	.015	.030	-1.88	2.5	4.5
	23	.015	.030	-1.88	5.0	4.4
	17	.030	.060	-1.55	11.5	3.6
	12	.035	.070	-1.47	11.8	3.5
	8	.040	.080	-1.41	16.5	4.5
	5	.185	.370	-0.33	21.5	3.0
	3	.240	.480	-0.05	26.2	1.8
	2	.365	.730	+0.61	27.5	3.0
	1	.455	.910	+1.34	29.5	1.2
	0.5	.530	—	—	29.5	2.0
Intensity (N=400)	5	.0525	.105	-1.25	13.7	4.5
	4	.0350	.070	-1.48	15.2	6.0
	3	.0775	.155	-1.02	19.0	5.6
	2	.1250	.250	-0.67	21.8	5.0
	1	.2800	.560	+0.15	25.2	2.5
Time (N=400)	20	.0250	.050	-1.64	13.6	5.8
	14	.0825	.165	-0.97	18.2	4.2
	9	.1500	.300	-0.52	24.0	2.8
	5	.2850	.570	+0.18	27.0	3.5
	2	.4025	.805	+0.86	26.9	3.5

* N=20 for pitch, 40 for intensity, and 40 for time.

magnitudes. For the group data the Pearson r 's were -0.98, -0.95, and -0.97 for the tests of pitch, intensity, and time, respectively. These results not only verify the logarithmic law previously established, but also indicate the accuracy of the present scaling.

Results of matching. For every stimulus-magnitude in the Seashore tests there were a number of matchings with weights from the series of 34. The number of matchings varied from 10 (individual S, in the case of pitch discrimination) to 40 (group experiment, in the cases of intensity and time discrimination) as may be seen in Tables I and II. In determining the central tendencies, medians were computed because of the relatively small

¹⁴ Guilford, *op. cit.* (see foot-note 9).

number of observations in some instances, and because we were not absolutely certain that the geometric series of weights was actually one of equal units. The distributions were typically widespread, some having a total range of as much as 20 points on the 34-point scale. In general, the dispersions were greater for the group data than for the individual data, as might be expected, since inter-individual variations were here added to

TABLE II
DATA FROM THE INDIVIDUAL S

	Stimulus magnitude R	Proportion failures \bar{q}	Corrected proportion $e\bar{q}$	Scaled value S	Median of matched weights* W	Variability of match- ings \mathcal{Q}
Pitch (N=100)	30	.00	.00	—	8.0	1.6
	23	.01	.02	-2.05	8.0	1.2
	17	.00	.00	—	11.0	2.4
	12	.01	.02	-2.05	9.0	2.0
	8	.00	.00	—	13.0	2.7
	5	.16	.32	-0.47	18.5	4.0
	3	.22	.44	-0.15	22.0	2.0
	2	.37	.74	+0.64	23.0	2.5
	1	.46	.92	+1.41	26.0	2.6
	0.5	.49	.98	+2.05	26.0	2.0
Intensity (N=200)	5	.015	.030	-1.88	11.7	1.4
	4	.010	.020	-2.05	12.8	2.0
	3	.045	.090	-1.34	16.8	2.5
	2	.135	.270	-0.61	19.0	3.5
	1	.255	.510	+0.03	23.0	1.6
Time (N=200)	20	.010	.020	-2.06	11.0	2.2
	14	.025	.050	-1.64	12.0	3.5
	9	.170	.340	-0.41	19.2	2.2
	5	.220	.440	-0.15	21.8	2.8
	2	.355	.710	+0.55	23.8	1.5

* N=10 for pitch, 20 for intensity, and 20 for time.

intra-individual differences in judgment. The relatively large dispersions are to be expected because we have not only variations in subjective impressions of difficulty of the auditory tasks, but also the variability of the impressions of the weights to be considered. The distributions were rarely skewed or other than unimodal—as well as one can judge from such small samples. The extreme weights, 1 and 34, were seldom selected. In the group, weight 1 was chosen only 11 times out of 600 total matchings and weight 34 only 3 times. In the individual data the extremes were never selected. We mention this point to show that the total range of weights seemed extensive enough to meet the demands of the experiment, though we are aware of the possibility that the Ss may have adjusted their matchings to fit the range that was available to them.

The relative dispersions for the different stimuli are indicated by the semi-interquartile ranges presented in the last columns of Tables I and II. The generally greater variability of the matchings by the group as compared with those of the individual is apparent. For the test of pitch discrimination, and to a less extent for the others in which the ranges of difficulty are smaller, the general rule holds that the greater the difficulty of the item, the lower is Q . We have not computed standard errors of the medians, for we

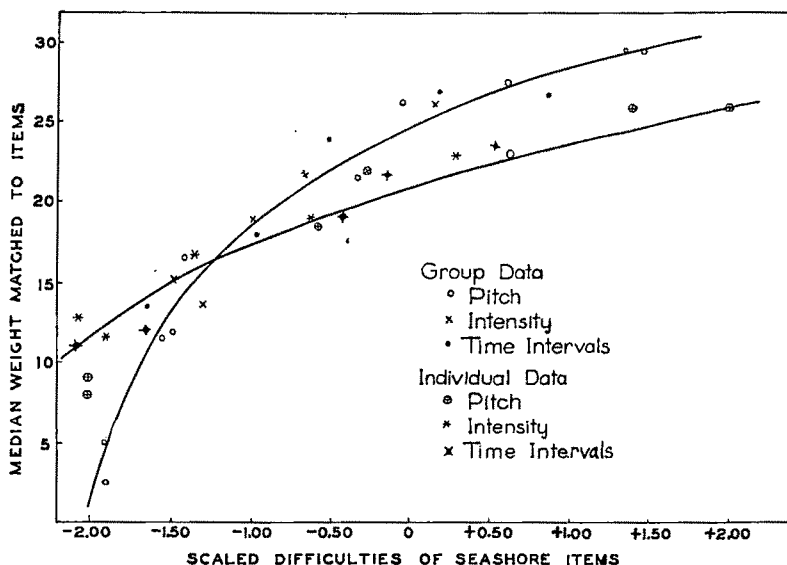


FIG. 1. REGRESSION LINES FOR THE RELATION OF JUDGED DIFFICULTY OF LIFTING WEIGHTS TO THE OBJECTIVE DIFFICULTY OF SEASHORE TEST ITEMS AS DETERMINED BY MATCHINGS

are not particularly concerned with the reliabilities of single matchings. It is the general picture in which we are most interested.

Relationship of average matchings to difficulty of items. The general picture is a relationship shown graphically in Fig. 1, for both the group and the individual data. The results from all three Seashore tests are treated together, for if the S s have any absolute impressions of difficulty, all the data should fall into the same continuous function. The trend of the points shows that they most certainly do. No matter whether S is judging difficulty of discriminations of pitch, intensity, or time intervals, for an item of the same objective difficulty he will select about the same average weight to match.

Fig. 1 definitely proves our linear hypothesis not to be correct for these items. In both functions there is undoubted curvature, with a negative acceleration apparent. Granting that the geometric series of weights is one of equal units for appreciated effort of weight-lifting, we should have to say that the greater the objective difficulty of the auditory discriminations, the smaller the increments of perceived difficulty for a corresponding increment of objective difficulty. Possible reasons for this will be brought out in later discussion. There are further facts concerning the relationship to be mentioned first.

The shape of the regression suggests, in this atmosphere of psychophysical thinking, a logarithmic function. If the felt difficulty of an item increases as the logarithm of the actual difficulty, and if the felt difficulty in lifting a weight increases as the logarithm of the stimulus, then the inference follows that there is a linear relationship between the physical values of the weights and the scaled difficulties of the Seashore test items that are matched with them. This conclusion was tested empirically and found justified. The goodness of fit is indicated by Pearson coefficients of 0.95 for both sets of data.

Validity of the scale of weights. Thus far we have merely raised the question as to whether the geometric series of weights really constitute a yardstick of effort with equal psychological units. From the systematic nature of the results of matchings, and the continuity and self-consistency of the results, we are somewhat reassured. There is still, however, a question about the possibility of a systematic change in the unit of the scale of weights. The question is all the more crucial after recognizing the failure of our expected linear hypothesis. Since the difficulties of the auditory tasks are directly proportional to the physical series of weights in terms of grams rather than to the logarithms of that physical series, we may question whether the Ss did not actually take the weights according to equal physical increments rather than psychological increments. It is possible, for example, that the Ss fell completely into the stimulus-error. They may have been encouraged to do so by the very wording of the instructions which told them to "equate mental difficulty to physical difficulty."

There is a need here of some independent determination of the actual psychological spacing of the weights. We attempted to answer the question with what data we already had in the following manner. The process bears a close kinship to the scaling of paired comparisons, in that by making some assumptions we extracted comparative judgments from the data.¹⁵ We must

¹⁵ L. L. Thurstone, Psychophysical analysis, this JOURNAL, 1927, 38, 368-389.

remember that, after listening to 10 test items (*e.g.* a pitch-difference of 12 c.p.s.), *S* has to find a weight in the series to match it. Let us say that at this moment *S* selects weight 15. Since this weight comes in a series of lighter and heavier weights and *S* tries a few within this neighborhood, from the fact that he has chosen weight 15 he has virtually told us that weights 16, 17, 18, 34 are greater than pitch-difference 12, and that weights 14, 13, 12, 1 are lighter than pitch-difference 12.

Hence, from the frequencies of matchings of the different weights with pitch-difference 12, which can be regarded temporarily as the *standard stimulus*, we can determine scale separations between every weight and pitch-difference 12, for all weights except those judged always greater or never greater. We did the same for every pitch-difference in turn as the standard stimulus, also for all intensity-differences and time-differences. The scale separations between successive pairs of weights were thus estimated a number of times and their averages were taken as the best estimate of their psychological separations. Assigning a value of zero to the lowest weight so scaled, we determined the scale values for all the others accordingly. These scale values are independent of the spacing of the auditory stimuli on the subjective continuum of difficulty.

The results were computed for the three kinds of stimuli separately. Only one of the regressions of scaled weights upon the numerical (geometric) series was linear for most of its length, and that was in the case of the time intervals as standards. Accordingly, we took into consideration the relative differences in dispersions of the scale values as determined from different standard stimuli. There were difficulties involved in this, since the ranges of scale values from different standards overlapped very little in some instances after the less reliable end values were eliminated. In the scaling when the pitch-differences were standards in particular this was true. There was so little overlapping for the two standards 5 and 8 that we made no attempt to adopt a common unit for all 10 standards, but adopted instead two units, one with the standard pitch-difference 17 as the unit and the other with standard pitch-difference 2. That these two units were not quite the same is evident in Fig. 2, where we show trends of the scaled weights as functions of the geometric series. In this figure, two vertical lines mark the transition from the one scale to the other (between weights 16 and 21) when scaling with pitch-difference standards. With the intensity and time standards, the scaling proceeded under more orthodox conditions and it will be seen that the regressions are decidedly linear and regular.

We may conclude, therefore, that the geometric series of weights is in

fact a psychological scale of equal units. The varying slopes of the three regressions in Fig. 2 have no significance other than that the size of the unit varies in the three cases. A similar scaling was performed for the results from the individual *S*, and in spite of the fact that his data were only half as numerous as those from the group, we were able to conclude

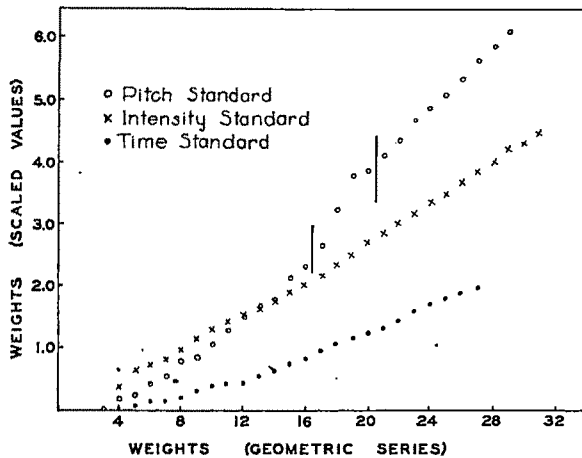


FIG. 2. LINEAR RELATIONSHIPS BETWEEN SCALED VALUES OF THE WEIGHTS AND THE GEOMETRIC SERIES OF WEIGHTS

that for him, also, because of an outcome similar to that in Fig. 2, the scale of weights was correct as a psychological yardstick.

DISCUSSION

The outcome of our verification of the scale of lifted-weights leads definitely to the conclusion that the effort appreciated in lifting, at least within the limits of the series used, is proportional to the logarithm of the physical evaluations of the weights. This much we predicted from Fechner's law. The law of appreciated effort is thus consistent with that for appreciated intensity of sensations (kinesthetic and tactual) when sensations are the reputed criteria of judgment. But the appreciated effort in lifting weights is matched, not with the objectively-measured difficulty of auditory judgments, but with the logarithm of that difficulty. The difficulty-values of discriminating the auditory differences is, therefore, directly proportional to the stimulus-values of the weights and the two are matched accordingly. We therefore face the problem of framing a hypothesis for this unexpected result.

Much is suggested by the fact that the most difficult auditory task, that of judging a difference in pitch corresponding to a stimulus-difference of 0.5 c.p.s., was so difficult that for the group of Ss taken as a whole it was a matter of guesswork. In scaling, such a level of difficulty is indeterminate but very large. Yet, the Ss, who may even realize they are guessing a large part of the time, often feel some confidence because they actually hear differences. If they really appreciated the full difficulty of the item they would recognize the impossibility of judging it and would seek to match it with a weight that was impossible for them to lift. Why did they not appreciate the full difficulty of the most difficult items? What were their cues for judging difficulty?

Cues for the appreciation of difficulty. The last question is a crucial one and its answer would do much to clarify matters. How does one ordinarily become aware of the difficulty of a task? As in most perception, the cues are numerous and they are employed in varying degree depending upon the circumstances. In general, the individual attempting a task has such cues as perceived size of stimuli or of stimulus-differences, time and effort expended, and awareness of success or failure. In judging auditory differences in our experiments, what cues were present? No knowledge of success or failure was available to the Ss. Although in most psychophysical experiments there is a rough correspondence between difficulty and time required to make a judgment, in responding to the Seashore test items a constant time of judging is enforced, within limits, by the conditions of the test. The cue of time is therefore probably of minimal value in this case. This leaves only the factors of stimulus-magnitude and effort expended as possible cues.

The auditory differences themselves could be of little direct value, since *S* was instructed to match difficulties, not sensory magnitudes. There is a functional limit to the appreciation of sensory differences in tests of this kind. Even when the stimulus-differences are zero, *S* generally senses some differences and so his confidence in judging them as such would not sink to zero. Even when *S* is wrong as often as he is right, judgments are possible because his nervous mechanisms give him differences when we should expect none if those mechanisms were perfect. What holds true for the most difficult items also holds true to a less extent for the less difficult items. The result is a law of diminishing returns. *S* neither loses confidence nor obtains an increasing impression of difficulty fully in step with what should be expected from increasing proportion of failures.

The cue of felt effort also may lead to similar results under the circumstances of the Seashore tests. When *S* faces the relatively larger differences

well within his powers of judgment, he exerts himself roughly in proportion to the magnitude of the task. With the smaller differences he realizes that more guesswork enters in and he relaxes somewhat in proportion as he knows that he is not expected to be right all the time and is justified in guessing. In other words, it does not require as much energy to guess as to try to be right every time. In so far as actually exerted energy or effort is a cue to difficulty, there results a law of diminishing returns. If *S* kept increasing his effort in proportion to the actual difficulty of the item, his judgments of difficulty might thereby follow in linear proportion to scaled difficulty. This assumes that his increased effort yielded no greater proportion of successes to alter the scaling of the items.

The operation of the factor of effort, as we have supposed, may be partly a function of the type of item which requires an *either-or* response. It would be interesting to see whether increasing the number of alternative responses would influence felt difficulty in any way. Asking *S* for more discriminations per item should have the effect of reducing the amount of self-tolerated guessing. *S* is then faced with a greater challenge and cannot easily lapse into a quasi-comfortable state of 'letting things slide.' On the other hand, we know that individuals in facing very difficult tasks tend to wilt before them and to exert little effort. Would the cue of felt effort function in this instance? Probably *S* would recognize the task as being beyond his powers and hence rate it as very difficult even though no effort were expended. This means a resort to the criterion of perceived success or failure. When this cue is operative, it would be interesting to see how well appreciated difficulty parallels actual difficulty, for they are both based upon the same data—proportions of failures—in this event.

Relation to muscular activity. In the report of Davis, previously cited,¹⁶ some interesting data bearing upon our discussion can be found. Davis gave *S* a small number of test items of the number-series type to be solved while action-current records were made of muscular activity in *S*'s neck and arm muscles. The amount of muscular activity was measured in terms of the percentage increase over a basal, resting state. The proportions of actual failures given for five test items were: 0.66, 0.96, 0.66, 1.00, and 0.32. *S* was also asked whether he thought he failed each item. The proportions of reported failures for the same items were: 0.29, 0.44, 0.07, 0.70, and 0.05, respectively.

When the items are scaled by the usual procedures, it is found that appreciated difficulty bears a clearly linear relationship to objective diffi-

¹⁶ Davis, *op. cit.*

culty, though this is based upon only four points, one item being unscalable for objective difficulty. The equation, from a graphic fit, is

$$D' = .7D - 1.4$$

where D is objective difficulty and D' is subjective difficulty.

From this equation one can see that subjective difficulty increased only 70% as fast as objective difficulty, and this lag is not due in very large part to the imperfect correlation, though the use of only four observations gives us little assurance of this conclusion.¹⁷ It can also be seen that when the item has an objective difficulty at the median, the appreciated difficulty is 1.4 standard-deviation points lower on the scale. This kind of result is immediately obvious from the fact that the proportion of reported failures for an item is never as great as the proportion of actual failures. It is possible that the width of this gap for different kinds of problems may affect in some manner the type of regression. The linear regression actually found here fulfills our first a priori hypothesis, but it does not agree with our empirical results. This discrepancy may be due in part to the fact that Davis's problems, calling for completion responses, leave little to chance success whereas in the Seashore tests the factor of chance success is maximal.

Davis's results with muscular tensions are very pertinent to our discussion of the cue of effort. While only two groups of muscles were sampled, they may be taken as symptomatic of general muscular tensions during problem solving. Davis points out a positive correlation between muscular tension and percentage or reported failures. When reported failures are scaled, the regression of subjective difficulty upon measurements of action current is found to be linear. Graphic fitting of straight lines gives the two equations:

$$D' = .09T_n - 12.5$$

$$\text{and } D' = .06T_a - 12.1$$

where D' , as before, stands for felt difficulty (scaled), and T_n and T_a are measurements of tension in neck and arm, respectively.

It will be noticed that perceived difficulty increases about 50% faster with tension in the neck muscles than it does with tension in the arm muscles. This might mean a tendency for the *Ss* to use neck tensions as cues more than they use arm tensions. This conclusion depends upon the relative dispersions of the two kinds of tension measurements. It is interesting that D' -intercepts are so large; they should be expected to be nega-

¹⁷ The deviation of the regression coefficient from 1.00 may be due in part to differing dispersions of the two kinds of difficulty, D and D' .

tive in sign. It is also significant that they are so nearly the same numerically, even though the slopes of the two lines differ. They mean that if and when the amount of tension in arm or neck is lowered to the residual, resting level during the solution of the item, it would be judged about 12 standard-deviation units below the median in difficulty. To put the conclusion in another way, even a trifling felt difficulty in solving an item goes with some increased muscular tension. The question as to which is cause and which is effect is another problem. Although we have been treating the relation as if tension were a cue or cause of felt difficulty there is probably a two-way relationship.

As would be expected from what has already been said about the linear relationships between D' and D , also between D' and T , Davis's results show a linear regression between D and T . The equations are more meaningfully expressed in the form:

$$\begin{aligned}T_n &= 122 + 7.7D \\T_a &= 176 + 11.5D\end{aligned}$$

Again, the fitting was graphic and the constants are, therefore, only approximate. The intercept values indicate the amount of muscular tension over the basal amount when the item is of median difficulty, 122% for the neck and 176% for the arm. The slopes of the lines show that tension in the arm increases with actual difficulty about 50% faster than does tension in the neck. The relations of T to D' reveal a similar, but greater, difference between arm and neck in this respect. Taking all these facts together, it seems that the arm activity may be more responsive to variations in actual and appreciated difficulty, but that in so far as tensions are used as cues, felt difficulty is more responsive to variations in neck tensions.

There seems to be a genuine hiatus between Davis's result and our own with respect to the regression of D' upon D , and we see no complete explanation for this. In his case D' is a linear function of D , and in ours D' is a logarithmic function of D . In so far as felt tensions are a cue to felt difficulty, it may be that we shall have to make a distinction between primary and secondary tensions. In the weight-lifting task, for example, there is the tension involved directly in the lifting as such and there is perhaps a secondary tension involved in the mobilization of effort for the task. In solving number-series items only the second kind of tensions would be present.

There is also the problem of variations in trying. In our experiments S knew he could lift any of the weights in the list; none was impossible. He may or may not have felt that all the Seashore items were within his

powers of auditory discrimination. In the case of the number series, in judging a failure *S* virtually said that the problem was impossible for him and that he gave up. Studies of Freeman¹⁸ may contribute much light to the whole rationale of these problems.

Motivational problems. The last trend in the discussion leads us into the tangled questions of motivation as related to effort. Ability factors and the subjective appreciation of abilities also play important rôles. The variables are many—abilities (general and specific), degree of motivation, actual difficulty of the task, effort, speed, time, confidence, and felt difficulty—to name only a few. Perhaps this number can be reduced. Thurstone has already made a notable beginning with regard to the underlying rationale dealing with the variables of motivation, ability, and speed.¹⁹ Additional dimensions will have to be brought into his picture before a complete rationale is effected. The importance of such hypotheses for the further pursuit of the problems of mental work cannot be gainsaid.

SUMMARY

The question was raised as to whether *Ss* appreciate the increase in difficulty of simple tasks in direct proportion to their actual difficulty. The tasks employed were judgments of items in the Seashore tests of pitch, intensity, and time discrimination. The same items were scaled objectively for difficulty by using proportions of failures as data. Judgments of felt difficulty were given by matching the tasks with lifted weights. The following conclusions seem justified.

(1) The difficulty of lifting weights is appreciated as being proportional to the logarithm of the weight, showing that Fechner's law holds for judgments of effort in this task.

(2) The felt difficulty of judging auditory differences is proportional, not to the objectively scaled difficulty but to the logarithm of the scaled difficulty.

(3) Items of the same objective difficulty from different kinds of tasks were matched with approximately the same average weight. Within the limits of these experiments this indicates an absolute impression of difficulty of the auditory tasks.

(4) Possible reasons for the law of diminishing returns in the appreciation of increasing difficulty in these experiments lie in the paucity and undependability of the cues in this kind of task.

(5) A rationale of the appreciation of difficulty leads inevitably into problems of motivation.

¹⁸ See in particular a very recent contribution: G. L. Freeman, The relationship between performance level and bodily activity level, *J. Exper. Psychol.*, 26, 1940, 602-608.

¹⁹ Thurstone, Ability, motivation, and speed, *Psychom.*, 2, 1937, 249-254.

ORIGINS OF BEHAVIOR AND MAN'S LIFE-CAREER

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In the pages which follow the behavior of the young infant is analyzed with the view of relating the products of the analysis to principles and problems of general, as well as to those of genetic, psychology.

Very little has been done by way of applying in a fundamental manner the materials of genetic study to general psychology. Infants, children, and adolescents have for many years been placed under methodical observation, have been tested and experimented with; but the results have not been put to a more general use by which the psychology of the adult might have profited from the psychology of the young.

Paradoxically, the main reason for this limitation is that the young have been studied more from the point of view of the adult than for their own sake. The psychology of the infant, and of the older child as well, has simply been borrowed from the adult. As a result, the genetic problem has appeared to be mainly one of *dating*. When do anger, rage, and love appear—on the first post-natal day or later? Does conditioning begin in the fetus? How big is the child's vocabulary at one year and at two years? The main inquiry seems to be *when* the familiar adult varieties of behavior first appear. If all that genetic psychology can offer is dates, it can obviously be of little use to general psychology.

The old notion that the infant is an incomplete adult dies hard. It is still not fully realized that infancy, childhood, and adulthood are qualitatively different modes of being; that infant and adult differ as one kind of animal may differ from another; that in his own way the infant is, no less than the adult, a complete, integrated individual. The foremost problem of genetic psychology is accordingly to determine what kind of animal man is in each of his genetic transformations.

Human life proceeds by stages. The life-periods of the human individual are no less real and significant than the geological ages of the earth or the evolutionary stages of life. *Each stage of man's life is distinguished by a dominant feature, a leading characteristic, which gives the period its coherence, its unity, and its uniqueness.* In the second half of the first year, for example, the infant touches one object with another, plays with toy and another child, attends to two persons, turns, reaches, pulls or calls

* Accepted for publication June 24, 1940.

at his mother for some thing, looks at an object pointed to, searches for his toy in a particular place, relates word and object. All these diverse accomplishments—involving manipulation, locomotion, social relation, gesture, speech—have one common factor, *i.e.* the capacity to deal with *two* things. This is a new capacity, the appearance of which characterizes this particular phase of infancy. The first occurrence of 'double dealing' or 'dual orientation' is thus a significant diagnostic sign. It betokens a certain stage of development.

As in this phase, so in all phases of life, *behavior is directly related to the character of the period*. In the following table the attempt is made to identify a number of natural stages in the life-course of the human individual in terms of the dominant feature or leading characteristic of each. Both the number of stages and the dominant features assigned to them must for the moment be regarded as tentative.

TIME-TABLE OF THE LIFE-COURSE OF MAN

- (1) First prenatal, embryonic period: *Acquisition of human form*
- (2) Second prenatal, fetal period: *Beginnings of motility*
- (3) First period of infancy: *Head-and-eye practice*
- (4) Second period of infancy: *Reaching*
- (5) Third period of infancy: *Dual orientation*
- (6) First period of childhood: *Domestication* (First period of socialization)
- (7) Second period of childhood: *Parting of the generations* (Second period of socialization)
- (8) First period of adolescence: *Quasi-adulthood* (Third period of socialization)
- (9) Second period of adolescence: *Beginnings of individualization*
- (10) First period of maturity: *Self-realization*
- (11) Second period of maturity: *Self-appraisal*
- (12) First period of senescence: *Retirement*
- (13) Second period of senescence: *Farewell, l'envoi*

This division of life into stages departs from the traditional, as well as from the literary, enumeration of the ages of man. The names given to the several steps in man's life-career are not arbitrary labels. They are intended to indicate the tone, the distinctive flavor of each period, and to serve as a guide in understanding man and his development. A clear understanding of the ages of man and a correct discrimination of them should make possible a practical diagnosis of developmental age.

The services of genetic psychology do not stop, however, with a mere description of the ages of man. There is the genetic problem proper of tracing the causes of successive transformations and of discovering how one stage sets the conditions for the next; how, *e.g.* 'dual orientation' derives from the 'reaching' activity which dominates the immediately pre-

ceding phase of infancy; or how 'double dealing' in its turn prepares the way for the next step in development.

For general psychology the study of the young has the same significance and usefulness as the study of the other animals, *i.e.* that of a *comparative* account. Thus the three forms of pre-adult socialization (Stages 6-8) afford materials for a comparative social psychology. Many adult phenomena and processes (*e.g.* action, emotion, language, and learning) are found in less complicated forms in the infant and in the child; and these early forms often persist to constitute the core or kernel of the later and more developed forms. As a matter of method, therefore, the analysis of adult behavior could usefully go back to the child and to the infant. Analysis would then be not only cross-sectional but also longitudinal or genetic.

THE PERIOD OF HEAD-AND-EYE PRACTICE

We select, for exemplary consideration, the head-and-eye practice of the first phase of infancy (Stage 3). Let it serve as paradigm. The purpose of the examination will be to determine, first, what man is like in this early state; secondly, how events in this period prepare the way for the next phase of infancy; and thirdly—the comparative problem—what can be turned to the use of general psychology.

It will be noted that no reference is made either to nominal reflexes or instincts or to the vague concept of maturation. Analysis goes deeper. The origins of behavior are traced to palpable properties of the body and to patent processes of change.

Events in this period, which follows the preparatory stage of fetal motility, are dominated by the body's *right-left balance*. Stimulation upsets the equilibrium and the infant's behavior adjusts it. The story of this period has three parts. (a) *Head-eye orientations*. Head and eyes are steered by a R-L balancing of stimulation to face, eyes, and ears. (b) *Mobilization of energy*. The entire body copes with serious threats to balance. (c) *Head balancing*. Because of the great disparity between the weight of the head and the infant's energy-resources, the falling head becomes the principal formative agency; as bodily energy increases, behavior is transformed by the requirements of head-balance.

(a) *Head-eye orientations*. Movements of head and eyes are elicited by stimulation to face, eyes, and ears. What laws, or steering principles, govern these movements of the infant?

Facial steering. The infant turns his head to a touch on his face. Why? Because the stimulus upsets the body's R-L equilibrium and *only a movement of the head can restore balance*. A touch on one side of the face means that stimulation is distributed

unequally over the right and left sides. When the head moves toward the stimulus, the distribution is equalized: the stimulus is centered. The movement of the head is caused by a R-L imbalance, and it terminates when right and left are again balanced. That is how the infant finds the nipple.

Binaural and binocular steering. The infant turns his head also to sound and to light. The steering factor here is disparity of stimulation in the two ears or in the two eyes. Again only a movement of the head, or of head and eyes, can eliminate or at least reduce the R-L disparity.

Monocular steering and the chiasma. Suppose that one eye is closed or lacking. How does the single eye steer? The answer is that monocular steering, too, can be effected by a balancing of right against left. The crossing of fibers in the optic chiasma permits a balancing of excitation in the retina's right half against the left half, a monocular R-L balance. Movement of the head and eyes can, because of this anatomical provision, be governed by the distribution of stimulation in the single retina, by *monocular disparity*. As steering organ, each eye is a double eye. With binocular stimulation, movement is subject to a fourfold retinal balance; man is four-eyed.

A monocular R-L balance accords with such facts as the following. (1) It is notorious that the young infant gazes into empty space. Where his eyes come to rest is determined by R-L relations of the total retinal pattern, not by any single object. Object fixation comes later. (2) In hemianopia the same half of each retina is blind. Accordingly a new R-L balance becomes established, with off-center fixation and the development of a pseudo-fovea. Blocking out half of the field of vision produces a similar effect in the normal person.¹ (3) Oblique lines produce disturbing and distorting effects.² One instance is the 'crazy room' with large oblique patterns on the walls. Persons entering such a room feel compelled to bend over. The oblique stimulus forces the head (and with it the body) to move in the direction of retinal balance.

R-L balance steers in all directions. If steering involves a R-L balance, how are head and eyes steered in other directions? The infant turns not only to right and left, but up and down as well, and he also throws his head forward and back. How is this tridimensional steering effected? (A similar question arises in relation to binaural localization: binaural disparity accounts for R-L localization, but how do we localize sounds in other directions?)³

The answer is that R-L balancing can steer in all directions, *provided the head is in motion*. Suppose, for example, that a light coming from above is equally distributed on the retina as to right and left. In that case a rotation of head and eyes would bring the light to one side of the retina, and this inequality would force a new adjustment. Through such shifts in balance, head and eyes will be steered into alignment with the stimulus-source.

¹ W. Fuchs, Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker, *Zsch. f. Psychol.*, 84, 1920, 67-169; 86, 1921, 1-143.

² Cf. J. J. Gibson, Adaptation, after-effect and contrast in the perception of curved lines, *J. Exper. Psychol.*, 16, 1933, 1-31; W. D. Orbison, Shape as a function of the vector-field, this JOURNAL, 52, 1939, 31-45; K. M. Dallenbach and M. B. Erb, 'Subjective' colors from line-patterns, this JOURNAL, 52, 1939, 227-241.

³ Cf. H. Wallach, Über die Wahrnehmung der Schallrichtung, *Psychol. Forsch.*, 22, 1938, 238-266; On sound localization, *J. Acoust. Soc. Amer.*, 10, 1939, 270-274; The rôle of head movements and vestibular and visual cues in sound localization, *J. Exper. Psychol.*, 27, 1940, 339-368.

Monastral steering. The single ear houses six steering organs. The three semi-circular canals govern head-movement by a balancing of stimulation in all directions. The two otolith organs, utricle and saccule, do the same, functioning jointly.⁴ The sixth steering organ is the cochlea. Mono-cochlear steering seems to involve a pitch-balance. Since head and pinna screen the higher frequencies of sound so that only the low frequencies are admitted to the ear, the tip of the cochlea receives most of the stimulation. When the head moves toward a sound, the higher frequencies are brought to the ear and so add stimulation to the lower part of the cochlea. Head-movement redresses an imbalance; *i.e.* the unequal distribution of excitation in the cochlea.⁵

R-L balance and nervous system. R-L balancing is supported by the organization of the nervous system. It is therefore to be expected that predominantly one-sided lesions should force abnormal R-L adjustments. For instance, near the end of such a disturbance the patient's head and eyes may deviate to one side. This symptom evidences the body's last desperate attempt at balance in face of serious lateral disparity. Other symptoms, too—spontaneous pains and other sensations in the body, anesthetics and hyperesthesias, spontaneous movements and abnormal limb-positions—point to a drastic redistribution of tension all over the body in the attempt at maintaining an impaired balance.⁶

Put in general terms, then, orientation is guided by the body's R-L balance. When modified by stimulation to face, eyes, and ears, balance is restored through movement of head and eyes.

(*b*) *Mobilization of energy.* Only moderate stimulation elicits head and eye movements in the infant. When balance is severely jolted, spasmodic behavior results. The infant 'goes into a spasm.'

Spasming. The infant's spasms result from serious threats to balance, such as acute internal and cutaneous distress and loud sounds. Any intensive, sudden, or unusual change⁷ is likely to produce a general effort or seizure; to call upon all the organs of the body (not only the head and eyes) to participate in the process of adjustment. For the infant, this total involvement is a means of mobilizing energy. And energy is mobilized largely by acceleration and redistribution of blood-flow.

Blood-flow, in the spasm, follows the lines of body asymmetry in taking an outward, forward, and upward direction. The distribution of blood shifts outward, particularly to head and face, so that the face turns purple. But how is the blood moved? Especially how is it moved up against gravity? It is in this connection that the downward tendency exhibited in other components of the spasm, the facial droop for example, becomes significant. It is because parts of the body are depressed that blood can flow in the opposite direction; it is squeezed up. *The spasm is a device for pumping blood.* When severely jolted, the infant works the pump.

⁴ E. B. Titchener, *A Text-Book of Psychology*, 1910, 180.

⁵ All six organs in the ear communicate with one another, so that cooperation is possible (H. Gray, *Anatomy of the Human Body*, 1924, 1059 f.).

⁶ P. Schuster, Beiträge zur Pathologie des Thalamus Optikus, *Arch. f. Psychiat. u. Nervenkr.*, 105, 1937, 355-622; 106, 1937, 1-233.

⁷ Cf. S. Feldman and H. P. Weld on the rôle of stimulus-level in perception (Boring, Langfeld, and Weld, *Introduction to Psychology*, 1939, 413-420).

The leg-action in the spasm is a part of the pumping device. The child in a tantrum reverts to the infantile method of pumping energy—he throws himself down and then kicks with his legs.

The lift. The spasm has its behavioral antithesis. In the antithetical form the eyes are open, the brow is smooth, and the mouth curves upward. Arms and legs fly. The breathing rhythm is inverted, so that gurgling, cooing, and laughter take the place of the sob, the whimper, and the wail. The antithesis is mainly directional. In the spasm parts of the body are forced down. Here there is indication of an upward lift. What is the source of the lift?

It is to be noted that both forms of energy-mobilization are induced in much the same way. In the peek-a-boo game, for instance, the loud noise or suddenly appearing face is no longer an occasion for bawling. It has become an enjoyable game. The child continues to delight in noise. The older child plays fear games; he finds a thrill in getting scared. He also spins on his heels to make himself dizzy. It is to the older person that dizziness is unpleasant⁸ and noise fatiguing and intolerable. But even the child can be pushed too far. When the peek-a-boo game is prolonged he may begin to cry, or while playing at getting scared he may scare himself in earnest.

The lift, first evidenced in the infant's smile, seems *an index of health and strength*. In the spasm, work against gravity strains the body's tissues. The strain is relieved when the body's lifting power is adequate. The infant can 'take it.'

Repetition-compulsion. In both forms of self-energizing, mobilization of energy is reënforced by a 'repetition-compulsion.' In the spasm, the infant's own cry and vigorous movements—themselves unbalancing—renew the agitation. The process is self-perpetuating. That is why the infant is hard to stop once he has started to cry, and why he may continue to cry after the original cause of disturbance has been eliminated. Similarly, the movements and sounds that evidence the lift also make for self-renewal.

Constituting a special source of reënforcement are the attempts at head-balance made when the head is drawn into the general agitation.

The entire body, then, is energized when adjustment is difficult. A *repetitive lifting* is induced. When the body is strong enough, the lifting is general. Otherwise the work of moving up the blood requires a depression of various parts of the body by way of a pumping action.

(c) *Head balancing.* The young infant lacks the strength to lift the head. This lack is a positive factor in his development.

Head's lever-action erects the body. In its head-eye orientations the body uses the same steering principle as is used in the see-saw or in guiding a horse with the reins. In its spasms it applies the principle of the pump. In the falling head, the body has the use of a remarkable lever.⁹

⁸ S. Russo and K. M. Dallenbach, Age and the effects of rotation, this JOURNAL, 52, 1939, 83-88.

⁹ Cf. Arthur Keith, *The Engines of the Human Body*, 1919, 47-56.

The infant tries to lift his head because it falls. For several months he perseveres in trying to keep it from falling. He practices head-mastery in the course of his head-eye orientations and general bodily agitations. He achieves a degree of mastery when the head assumes a forward posture: it then no longer falls to the side. In balancing his head, the infant assists himself with his arms.¹⁰

Finally, in the fourth month, the infant can hold his head up. Yet the acquirement is still imperfect. Even the adult's head is continually swaying, and at four months balance is even more precarious. To keep his head up the infant is therefore compelled to reach out, and to sit up. Reaching and sitting begin in attempts at balancing the head. Nor is this the end; continued reaching leads to crawling, to standing, and to walking. *The falling head is thus the force that in the long run pulls the infant up to his feet.* The lever-action of the head, persistently applied in the course of a year and more, does the trick.

The length of time is a measure of the great disparity between the human infant's powers at birth and the weight of his head. This disparity distinguishes him from his cousins in other species of animal life. No other mammal takes so long in going through the motor sequence. The chick starts pecking while still in the shell, and its head-eye orientations are soon put to use in pecking for food. Here head-weight and body-energy are nearer parity at the beginning. *Man's infancy is longer because his head is bigger.*¹¹

Man owes his *erect posture* to the same fact. For a longer time than in any other animal his body is too weak to support the head, to keep it from falling. This gives the lever-action of the head a longer time in which to operate—*long enough to put him on his feet.*

Body as gyroscope. Acquiring head-balance, the principal achievement in the first period of infancy, involves the entire body. The head is kept from falling by a redistribution of stresses and strains all over the body. *The body is the gyroscope that balances the head.* Balancing it requires much more than strong neck-muscles. The entire body must become strong enough to take up and redistribute the load. The redistribution, in fact, is such as to make things easy for the muscles of the neck. They work all the time, yet they do not readily tire. The main stress is ordinarily taken up elsewhere. 'The head lies easy.'

One consequence is that *the head becomes easy to steer.* Unbalanced, it must be moved against gravity. Balanced, it can be steered by the application of a small force or with a slight effort; the head is under control. Not every infant graduates with this degree of success from his first schooling in head-practice. Iterative nodding after the fourth month may be a serious diagnostic sign.¹²

Looking, self-energizing, head-balancing—along with sleep and feeding—such is the repertory of the young infant. The achievement of head-control opens the way to further developments. (1) The first stage in the head's persistent lever-action is finished. But since head-position is still

¹⁰ Cf. C. Landis and W. A. Hunt, *The Startle Pattern*, 1939, 60-73.

¹¹ Cf. the account of Subject E in B. J. Johnson, *Mental Growth of Children*, 1925, 13-22. The account suggests a relation between size of head and rate of development in children.

¹² L. Kanner, *Child Psychiatry*, 1935, 256-260.

precarious, the lever continues to work. The results are reaching, sitting, and locomotion. (2) Arms and body swing into line with the moving head, the orientation of which prescribes the direction of reaching and of locomotion. The head leads and the body follows. (3) Head and eyes begin to turn to a touch *below* the face.

In general, then, head-balancing is the leading, controlling activity from birth on. The falling head, acting as lever, lifts head and body. The entire body participates in the balancing of the head. *The orientation of the balanced head becomes the pivotal component in all behavior.*¹³

ORIGINS OF BEHAVIOR

Our analysis has taken a long step beyond the traditional labeling of items of behavior as 'reflexes' or 'instincts.' Behavior in the period of head-and-eye practice has been derived from certain properties of the body. This derivation has made it possible to postulate primary laws of head-and-eye movement; to suggest a functional significance for the optic chiasma, a new visual factor (monocular disparity), and a new auditory factor (pitch balance); to describe a mechanism for the emotive seizure; and to analyze the dynamic relations between head and body. This sort of analysis of behavior in terms of properties of the body could be usefully extended to all stages of life.

In succeeding stages of living, the bodily properties discussed are put to new uses, while still others then also become relevant. Thus far we have had recourse to (1) right-left balance; (2) the location of the steering organs and their balance relations; (3) the R-L balance can steer in all directions; (4) the pump action of the spasm, the 'lift,' and 'repetition-compulsion' as factors in the mobilization of energy; (5) the initial disproportion between bodily energy and head-weight; (6) the increasing energy of the bodily system; (7) the dynamic relations between head and body—the falling head as lever and the body as gyroscope; and (8) the principle that a balanced system is easy to steer. Another factor, the lever-action of limbs and objects, comes to the fore in later infancy, in the development of body-control and the control of objects.¹⁴

¹³ The nervous system has been aptly characterized as being essentially an anti-gravity device. While the first period of infancy is dominated by the necessity of maintaining right-left balance, gradually behavior becomes complicated by the super-imposed requirements of head-balance. Behavior becomes headwork, a way of keeping one's head up.

¹⁴ For the rôle of lever-action in perception, see S. Feldman, Phantom limbs, this JOURNAL, 53, 1940, 590-592.



MAN'S LIFE-CAREER

We have held that genetic psychology has three functions to perform: analysis of the stages of life; discovery of the causes of transformation; and the application of the comparative method to the entire life-course. A number of stages in the life-career have been identified in terms of their dominant features or leading characteristics. One of these stages, that characterized by head-and-eye practice in early infancy, has been defined more closely. The others will now be briefly depicted.¹⁵

Several features of infancy date back to prenatal conditions, e.g. negative orientations and grasping (with hand, foot, lips). *Negative orientations* or withdrawals derive from a 'part-whole' balance; local excitation introduces a gradient, a relation of imbalance between the region affected and the rest of the body, and movement is steered in the direction of balance: reduction or elimination of the gradient. The principle is that of homeostasis, an 'as-you-were' principle. The *grasping* movements have their origin in simple mechanical relations. Hand and foot close in like traps when touched. The lips draw tight like a noose. It is, simply enough, by virtue of their construction that these parts of the body become the infant's instruments of gripping, grasping, and clutching.

The principal developments during the periods of infancy are two. The infant acquires *control of body and objects* and he acquires an *orientational schema*.

For the post-infantile periods, several criteria of development, i.e. factors which determine the person's place in the career of living, claim attention. They are (1) socialization and individualization; (2) attitude to the older generation, to one's own generation, and to oneself; (3) range and zones of activity; (4) temporal perspective; and (5) energy, energy-mobilization, and regulation of energy.

The age of *domestication* is the first period of socialization. The child's orientational schema has a nexus, a central point of reference. This is the adult. For the young child, all roads lead to and through the parent. The parent's sphere of influence defines the child's arena of activity. The parent is the child's main source of energy and his model. Imitative and repetitive urges are features characteristic of the period. Important in understanding the young child are the techniques of control which he practices

¹⁵ For a summary of relevant experimental literature see N. L. Munn, *Psychological Development*, 1938, 1-582. See also M. W. Shinn, *The Biography of a Baby*, 1900, 1-247; Charlotte Bühler, *The First Year of Life*, 1930, 1-281; and *Der menschliche Lebenslauf als psychologisches Problem*, 1933, 1-328.

in dealing with his parents. After achieving control of his body and of objects, the child turns to the mastery of his fellow-men, and he begins with the adults. Behavior in this period serves in the humanizing and acculturation of the child.

A second period of socialization begins with the *parting of the generations*. Range of activity is extended from the home to the 'out-doors.' The child-generation forms a society of its own, the individual learning to deal with equals and, more generally, with its own generation. Fundamental to this period is the division of the child's world into a domestic zone and an outdoor or 'forensic' zone.

Quasi-adulthood is the third period of socialization. This is a period of preliminary practice in adulthood, a false dawn heralding the true dawn. The young generation now regards itself as, and it acts as if, grown up, regarding the real adult as 'old.' It has its own public opinion, and takes its own generation as model. Behavior in this period takes the form of a testing of one's powers.

In every early period of life there is a succession of passive and active phases, *e.g.* passive and active looking. So it is after the passive period of quasi-adulthood that one really begins trying to become adult. The effort involves a change in temporal perspective (the future begins to be really important) and a change in attitude toward oneself (the *beginnings of individualization*). These two periods of adolescence are periods of practice in self-mastery (regulation of one's energy-resources) and in controlling the social order. Control of body and objects, of adults and equals, of self and the social order—such is the order of development.

Maturity is the period of *self-realization*. Marks of maturity are differentiation (one aspect being vocational differentiation), sustained application (a most difficult form of energy-regulation), and responsibility.


The turn in life is signaled by a new change in temporal perspective, the beginning of retrospection, a retrospective *appraisal* of one's life-career. When one begins to think of one's autobiography, he is getting old. The old man's indulgence in autobiographical references represents the residue of a process commencing in middle life. In face of declining vigor, the level of activity may be maintained (or even increased) by virtue of skill, experience, or subterfuge. The revaluation of self that is the principal mark of this age is at times critical, leading to a change of personality.

The age of *retirement* is a period of definite contraction. When retirement involves giving up all outside interests and employments, it becomes virtually a new period of domestication.

The final period of life is a period of saying *farewell*. Farewell is said

in a variety of ways: carrying on, calm preparations, hurry, panic, resignation, flight to retrospection, giving up the game with a consequent breaking up of habits (even those acquired in the early period of domestication). Analogous forms of behavior may appear earlier in life; as when one is condemned to die, suffers from an incurable disease, is faced with loss of prospects, or determined on suicide.

Such, in outline, is man's life. We begin by acquiring the human form (Stage 1). Next we become oriented in the world (Stages 2-5). We are then socialized in three distinct ways: *with* the older generation, *apart* from it, and in *rivalry* to it (Stages 6-8). A change in time-perspective (Stage 9) leads up to the peak of life (Stage 10), while a reversal of temporal perspective marks the descent (Stages 11-13). Filling in the outline of life and searching out the causes of progressive change are primary problems for genetic psychology.



A QUANTITATIVE ESTIMATE OF CERTAIN TYPES OF SOUND-PATTERNING IN POETRY

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The phonetic elements which compose a representative sample of normal speech are not distributed at random but are to some extent grouped. This characteristic cannot always be accounted for by the repetition of meaningful material; and it must therefore be regarded as an example of 'formal perseveration' or 'formal strengthening.' It may be described by saying that when a speech-sound is once emitted, the probability of emission of that sound is temporarily raised. In addition to normal speech, formal strengthening is observed in word associations, in speech obtained *in vacuo* with the verbal summator,¹ and in the verbal behavior characteristic of certain psychopathic disorders. In literature it appears in an exaggerated form as rhyme, assonance, alliteration, and (if we include stress-pattern as a formal phonetic characteristic) rhythm. These extreme cases are in themselves worthy of investigation, and they may also be expected to throw some light on the general process.

A satisfactory demonstration of formal strengthening in poetry or prose cannot be made by pointing to instances, although this is common practice in the field of literary criticism. Seven or eight different initial consonants, for example, will usually account for more than half the instances, and many accidental 'alliterations' are to be expected. The assertion that any sample of speech demonstrates alliteration *as a process in the behavior of the writer* must rest upon a statistical proof that the existing patterns are not to be expected from chance.

One method of analyzing a poem is to examine the number of lines containing two or more occurrences of a given initial consonant and to determine whether this exceeds the expectation based upon the general frequency of the consonant in the whole sample. In a study of 100 Shakespearean sonnets it was found that the slight excess of lines containing more than one occurrence was largely accounted for by the repetition of whole words, where the strengthening or perseveration seemed to be principally derived from thematic rather than formal factors.² Little or nothing

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¹ B. F. Skinner, 'The verbal summator and a method for the study of latent speech,' *J. Psychol.*, 2, 1936, 71-107.

² Skinner, 'The alliteration in Shakespeare's sonnets: A study in literary behavior,' *Psychol. Record*, 3, 1939, 186-192.

could be learned of the nature of alliteration in such a case, and it seemed advisable to turn to a poet who almost certainly exemplifies the process in an extreme degree. The present report is an analysis of the first 500 iambic pentameter lines of Swinburne's "Atalanta in Calydon." Both alliteration and assonance are considered.

Not all the sounds in a poem contribute equally to its effect, nor do they represent equal opportunities of selection and manipulation on the part of the writer. The formal analysis of a poem is facilitated by a preliminary choice of the principal sounds, especially the syllables receiving emphasis when the line is scanned, as well as a number of other obviously important words. The omitted material may be regarded either as not contributing in any effective way to the sound pattern or as not permitting any variation in the behavior of the writer. In order to avoid unconscious prejudice with regard to patterning, the material must be selected by rule, even though the best interpretive reading is perhaps not always obtained. In the present case each line was first scanned strictly as iambic pentameter. When the accent fell on a weak syllable (e.g. preposition, auxiliary, possessive pronoun, article, copula, or such an ending as *-ness*, *-ing*, *-ance*, or *-ment*), it was shifted forward or backward whenever possible to an adjacent strong syllable not included in the scanning. Otherwise it was omitted. For example, in the line "Bite to the blood and burn into the bone," a strict scansion gave "Bite *to* the *blood* and *burn* into the *bone*." The accent on *to* was shifted backward, according to rule, to the syllable *bite*. However, the accent on *to* in *into* could not be so shifted because there was no adjacent strong syllable, and it was hence eliminated, leaving a line of four accents. This first stage generally yielded four or five selected syllables per line. Each line was then examined for strong syllables not yet receiving stress, and all accented parts of nouns, verbs, adjectives, and adverbs were added. The result was a scanned line which gave an important place to practically every significant syllable.³ The procedure yielded 27 lines of four syllables, 211 lines of five syllables, 193 of six, 54 of seven, and 15 of eight, or a total of 2,819 syllables.

Alliteration by line. In determining Swinburne's alliteration by line, the lines containing one, two, three, four, or five occurrences of the same initial consonant were counted, and the frequencies were compared with the expected mean frequencies.⁴ The latter were obtained from the binomial expansion $N(q + p)^n$, where N is the number of lines examined, n the number of syllables per line, p the probability of occurrence of the consonant under consideration (calculated from its frequency in the sample), and q the probability of occurrence of any other sound, or $1 - p$.

³ A few specific exceptions were made. For example, the prepositions *across*, *because*, *above*, *about*, *against*, and *upon* were allowed to retain the beat in the first scanning. The adverbs which were later added did not include *not*, *no*, *then*, *thus*, *so*, *but*, *only*, *all*, nor did the adjectives include *no*, *some*, *all*, *none*, and *such*. Interrogative and personal pronouns were added when they were the subjects or objects of verbs.

⁴ The writer is indebted to the National Youth Administration and to Miss Marion Kruse for the tabulations used in this study.

Separate calculations were made for the lines of various lengths given above, except that the lines of seven and eight syllables were grouped and treated as if they contained seven syllables. The total expectation was obtained by adding the resulting frequencies. The data for the ten most frequent consonants ($p > 0.03$) are given in Table I.

Practically without exception, Swinburne has too many lines containing two, three, and four instances of the same consonant. For example, according to Table I, there are 27 lines containing pairs of *b*'s in place of

TABLE I
LINES IN A SAMPLE OF SWINBURNE CONTAINING VARIOUS NUMBERS OF CERTAIN CONSONANTS
COMPARED WITH FREQUENCIES EXPECTED IN RANDOM SAMPLING

Conso- nant	Number of occurrences per line											
	0		1		2		3		4		5	
	Swin.	Exp.	Swin.	Exp.	Swin.	Exp.	Swin.	Exp.	Swin.	Exp.	Swin.	Exp.
<i>b</i>	370	353	97	127	27(24)	19	4	2	2	0	0	0
<i>d</i>	399	395	88	95	11(9)	10	2	1	0	0	0	0
<i>f</i>	338	312	107	153	47(39)	31	5(4)	3	3	0	0	0
<i>g</i>	396	391	89	98	13(11)	10	2	1	0	0	0	0
<i>h</i>	312	328	139	143	43(38)	26	4	3	2	0	0	0
<i>l</i>	349	331	110	141	32(24)	25	9	2	0	0	0	0
<i>m</i>	355	352	122	127	19(14)	19	4	2	0	0	0	0
<i>s</i>	237	222	173	192	66(61)	70	19	14	5	2	0	0
<i>th</i>	360	347	107	130	29(23)	21	3(2)	2	1	0	0	0
<i>w</i>	366	357	105	124	27(24)	18	2	1	0	0	0	0
All					314(267)	249	54(52)	31	13	2	0	0

the expected 19, four containing three *b*'s in place of the expected two, and two containing four *b*'s where none is expected. There is a consequent shortage of lines containing only one *b* (97 in place of 127) and an excess of empty lines. The alliterative effect is especially strong for *b*, *f*, *h*, *l*, and *w*. The consonant *s* shows a preponderance of lines containing three and four instances but an actual shortage of lines containing two. The effect for *th* (voicing ignored) is slight, and that for *m*, *g*, and *d* negligible. For the 10 consonants combined, there is an excess of 65 lines containing two instances of the same consonant, 23 containing three, and 11 containing four.

As in the case of Shakespeare, it is difficult to interpret repetitions of whole words. These involve formal perseveration, but presumably the meaning of the passage is of considerable importance in determining the second emission in each case. The numbers in parentheses in the table are the corrected frequencies obtained by subtracting all cases of the repetition of a whole word. The correction operates to reduce the evidence for alliteration, perhaps unduly, but significant differences are still obtained.

Although the initial consonants in this sample of 500 lines are unquestionably not distributed at random, an actual estimate of the amount of arrangement on the part of the poet is not easily made. A definite meaning can be given to the statement that the sample contains so-and-so many extra lines containing two, three, or four instances of the same consonant, but in the face of an evident grouping the expected mean frequencies used here are called in question. If some instances of the repetition of a consonant are due to formal strengthening, the total frequency used in the calculation of p is not representative, and the value of p is too high. This defect in the calculation will be roughly proportional to the amount of grouping and will operate to obscure some of the alliteration actually present. Thus, if we conclude that there are eight excess lines containing two instances of the sound b and two excess lines each containing three and four instances, and that these lines would otherwise have contained only one b (this is suggested by the frequency of the one-occurrence lines), then there are at least 18 instances which are due to formal strengthening, and the frequency used in calculating the value of p which applies to the sample in general should have been, not 171, but at least as low as 153. Ignoring the effect of this elimination on the size of the total sample, we obtain a value of p equal to 0.05428 rather than 0.06066, and we then expect only 16 lines containing two instances, and only one line containing three instances, instead of 19 and two, respectively. The excess exhibited by the table is thus less than the real excess in the poem.

When little alliteration is indicated, no correction is called for in the present degree of approximation. This is generally true of the Shakespeare data. Where an excess of "heavy" lines is demonstrated, it has probably been underestimated. For practical purposes the calculation based upon the total frequency of each consonant provides for a satisfactory measure of alliteration.

Alliterative span. The analysis of alliteration *by line* has a certain value in the criticism of poetry, but it is not an exhaustive survey of alliterative strengthening. There may be a detectable grouping in some shorter unit within the line or in a larger unit of, say, two or three lines which would not necessarily appear with the preceding method. Moreover, where alliteration has clearly been demonstrated, it is desirable to determine the actual alliterative range or span by taking the size of the analyzed unit into account. The perseverative strengthening which follows the emission of a consonant must eventually die out, and the course of the effect should provide an indication of its nature. This course may be traced by inquiring how often the repetition of a consonant follows immediately in the next syllable, how often a single syllable intervenes, how often two syllables intervene, and so on.

In collecting the necessary data, the initial consonants (or initial vowels, where these occurred) of all selected syllables were placed in order on a long tape.⁵ The tape was then passed under cards in which windows were cut to display consonants

⁵ The breaks caused by the omission of two choruses were ignored.

with any desired number of intervening syllables, beginning with adjacent pairs. The numbers of pairs of each consonant at distances up to that of eight intervening syllables were tallied by moving the tape under each card one space at a time throughout its length. The figures for the 10 commonest consonants are given in Table II, together with the expected values. The latter were approximated by squaring the frequency for each consonant and dividing by the number of syllables in the total sample. Thus, when every *b* on the tape is compared with some other

TABLE II
NUMBER OF REPETITIONS OF A CONSONANT AT VARIOUS DISTANCES FROM THE FIRST
OCCURRENCE COMPARED WITH THE EXPECTATION FROM CHANCE

Conso- nant	Ex- pected	Observed (No. intervening syllables)								
		0	1	2	3	4	5	6	7	8
<i>b</i>	10	20	21	18	21	9	9	12	9	11
<i>d</i>	5	8	10	7	7	6	5	6	7	7
<i>f</i>	18	36	32	18	17	17	22	18	21	19
<i>g</i>	5	11	8	9	9	11	5	1	4	4
<i>h</i>	15	22	20	20	19	16	16	9	18	20
<i>l</i>	14	24	25	22	18	15	21	17	14	15
<i>m</i>	11	20	14	22	12	9	11	7	13	7
<i>s</i>	52	66	59	57	52	58	52	49	55	57
<i>th</i>	12	15	19	12	21	18	16	10	12	13
<i>w</i>	10	13	15	14	8	10	8	14	8	6
Total	152	235	223	201	184	169	165	143	160	159

letter in a constant relative position, we expect to obtain, in round numbers, 10 pairs; for there are 171 *bs*, and the probability of finding a second *b* in each case is approximately $171/2819$ or 0.06066.

When the second consonant immediately follows the first (with no syllable intervening between the syllables being compared), 20 pairs of *b*'s are actually observed instead of the expected 10. When the position compared is the next but one (one syllable intervening), 21 pairs are observed. It is only when four syllables intervene that the observed value drops to the chance level. (The standard error of the expected mean frequency is in each case approximately the square root of the frequency, and this may be used in estimating the significance of the observed figures.) The separate values for each consonant are based upon too small a sample to be very illuminating, but the total frequencies for all 10 consonants show a very clear trend from a maximal influence upon the immediately following syllable to approximately a chance effect after the intervention of four syllables. These totals have been plotted as Curve A in Fig. 1, in which the chance level is also indicated with a broken line. The solid circles represent columns in Table II which differ significantly from the

column of expected frequencies; the observed frequencies being likely to occur less than once in 100 trials under random sampling (χ^2 test).

The curve clearly demonstrates that the strengthening effect of the emission of a consonant is greatest in its immediate vicinity and drops to

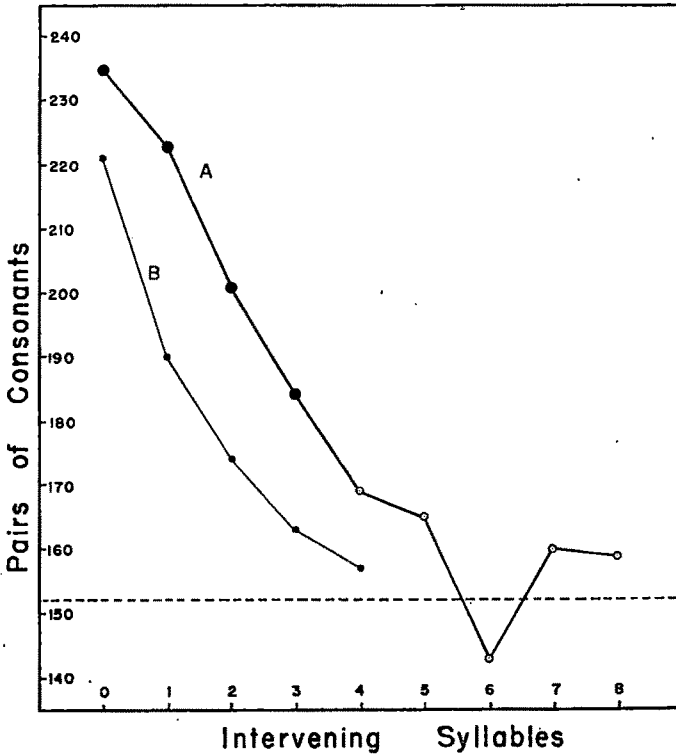


FIG. 1. SWINBURNE'S ALLITERATIVE SPAN

Curve A: The sum of the numbers of pairs of the 10 commonest initial consonants is plotted against the number of syllables intervening between the syllables examined. The dotted line indicates the sum of the mean frequencies expected under random sampling. The solid circles are for frequencies differing significantly from the expected frequencies. *Curve B:* All instances of the repetition of whole words (including all forms with a common root which determines the initial consonant) have been subtracted to show the least possible effect of formal strengthening. The dotted line is slightly high for comparison with this curve.

zero at a distance of about four strong syllables. There are, however, certain qualifications to be applied to this statement. In the sample here examined only certain consonants show the effect. Moreover, the repetition of whole words, presumably due in part to considerations of meaning,

must be allowed for. In this sample the number of pairs of whole words⁶ with no intervening syllables is 14; with one intervening syllable, 33; with two, 27; with three, 21; and with four, 12. The curve at B in Fig. 1 shows the span obtained after all these instances have been subtracted. Without defining the share contributed by purely formal strengthening to the repetition of a whole word, these curves give at least the upper and lower limits of the effect. In the case of the lower curve the line indicating the sum of the expected mean frequencies is somewhat too high, since if we eliminate some of the observed cases, we must recalculate with smaller frequencies. When one syllable intervenes, the indicated sum of the expected mean frequencies may be as much as five points too high. The effect of subtracting pairs of whole words from the curve for alliterative span is thus not quite so drastic as Fig. 1 may suggest.

Three adjacent instances of the same consonant. Further evidence of formal strengthening is provided by the presence of groups of three adjacent instances of the same consonant. The values of *p* lead us to expect very few cases, but a considerable number should result from formal

TABLE III
GROUPS OF THREE ADJACENT INSTANCES OF THE SAME INITIAL CONSONANT

	Consonant										Total
	b	d	f	g	h	l	m	s	th	w	
Expected	1	0	1	0	1	1	1	7	1	1	14
Observed	3	2	10	1	5	2	3	15	1	0	42

strengthening because both members of a pair of consonants presumably contribute to the heightened probability of occurrence in the third position. In Table III the observed and expected frequencies of groups of three instances of the same consonant are compared for the 10 commonest consonants. The excess of 28 "triplets" over the expected 14 is of sufficient magnitude to confirm the cumulative action of formal strengthening. A similar result has been obtained by tabulating groups of three consonants which contain one syllable of a different sort, as in the sequences *b-b-r-b* or *b-r-b-b*.

Shakespeare's alliterative span. A comparison of instances of the same consonants without regard to linear position may be used to examine the 100 Shakespeare sonnets

⁶ Throughout, "whole words" is to be understood as including inflected forms, nouns and adjectives from the same root, or any case where a similarity of form can be related to a single subject matter.

which previously yielded only very slight evidence of alliteration when tested by line.

It was impossible to construct a single long tape in this case, since the end of one sonnet and the beginning of another could not be supposed to have any relation, even if we had known the order of composition. Hence a tape for each sonnet was constructed and tabulated separately. All pairs up to and including those with four intervening syllables were examined. The principal effect of breaking the

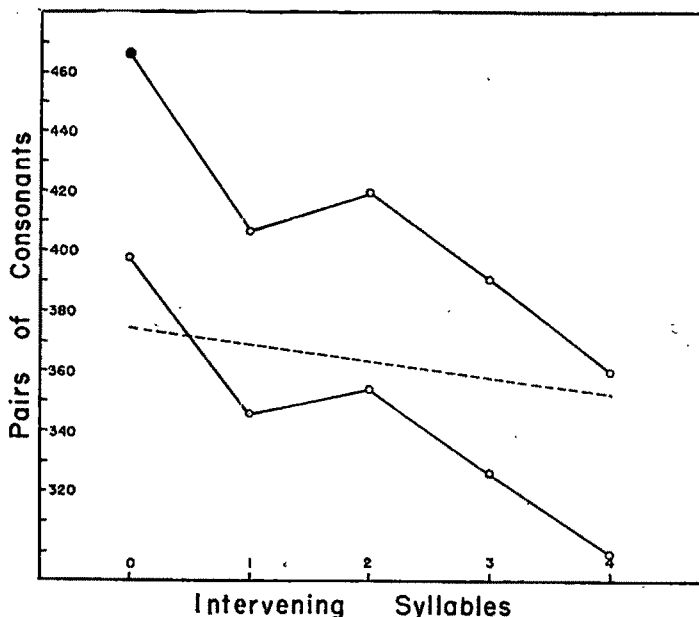


FIG. 2. SHAKESPEARE'S ALLITERATIVE SPAN

Upper curve: The sum of the numbers of pairs of the 10 commonest initial consonants is plotted against the number of syllables intervening between the syllables examined. The dotted line indicates the sums of the expected mean frequencies; the slope is due to the fact that with each additional intervening syllable fewer pairs are available for comparison. The solid circle represents a set of frequencies significantly different from the corresponding expected frequencies. *Lower curve:* All instances of the repetition of whole words have been subtracted. The dotted line is somewhat too high for comparison with this curve.

sample into short parts was to reduce the number of pairs with one or more intervening syllables; at the end of each sonnet there were syllables with which no succeeding syllable could be compared, and there were more of these the greater the number of intervening syllables. Full allowance for this was made by calculating a separate mean frequency in comparing consonants separated by different numbers of intervening syllables.⁷ The uncorrected data (no allowance being made for the

⁷ A very slight effect of the same sort was, of course, present in the single long tape in the Swinburne study, but it could safely be ignored.

repetition of whole words) are shown in the upper curve in Fig. 2. The sums of the expected mean frequencies are also shown with a broken line. The sums decline because of the reduction in the number of pairs available for examination, as just noted.

At first sight the upper curve in Fig. 2 may seem to indicate a considerable alliterative effect. Only the first point, however, is significantly different from the expected frequency. The observed numbers of pairs when no syllables intervene would be expected about once in 100 trials under random sampling. The other

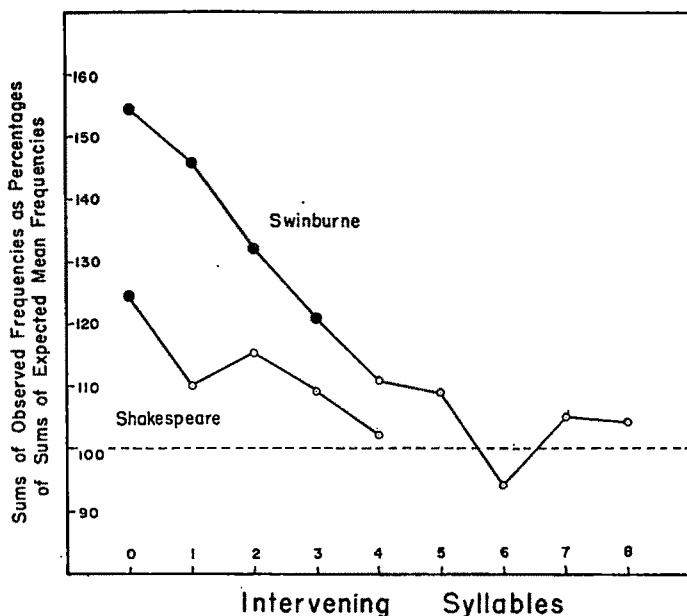


FIG. 3. COMPARISON OF THE ALLITERATIVE SPANS OF SHAKESPEARE AND SWINBURNE

The sums plotted in Fig. 1 and Fig. 2 have been converted to percentages of the sums of the expected mean frequencies. The solid circles indicate significant values. No correction has been made for the repetition of whole words; the required correction would be slightly more extensive for Shakespeare.

points show a plausible trend but are not statistically significant. The lower curve is for the data corrected by subtracting instances of the repetition of whole words. As with Swinburne, the 'expected' line is somewhat too high for comparison with the corrected curve because of the need of recalculation with reduced frequencies, but the curve is safely within the range of values characteristic of random sampling.

In order to provide a rough comparison of Shakespeare and Swinburne, the data obtained with this method were converted to percentages of the expected mean frequencies. The alliterative spans of the two poets are represented together in Fig. 3. It is obvious that with Swinburne alliteration is not only much more common, but extends over a considerably wider span, than in the Shakespeare sonnets.

Assonant span. A perseverative strengthening of vowels (or 'assonance') is also to be expected from the casual observation of many poems, and the possibility may be investigated with the present method.

A tape was prepared for the vowels in the selected syllables of the Swinburne poem. In order to avoid the necessity of making relatively fine distinctions in pronunciation and to obtain larger frequencies, the phonemic boundaries were somewhat enlarged. All vowels marked in Webster's *International Dictionary* as *ĩ*, *ẽ*, *ẽ*,

TABLE IV
REPETITION OF A VOWEL AT VARIOUS DISTANCES FROM THE FIRST OCCURRENCE,
COMPARED WITH EXPECTATION FROM CHANCE

Vowel	Ex- pected	Observed (No. intervening syllables)										
		0	1	2	3	4	5	6	7	8	9	10
<i>ā</i>	20	12	17	26	22	20	28	26	25	23	21	15
<i>ǎ</i>	18	16	14	22	33	19	25	17	22	24	19	20
<i>ē</i>	32	29	26	39	35	21	32	33	32	25	29	31
<i>ẽ</i>	33	30	42	31	33	27	28	26	27	29	28	29
<i>ĩ</i>	32	35	39	25	32	27	39	36	31	33	37	31
<i>ĩ</i>	45	36	36	43	54	42	55	50	44	39	49	42
<i>õ</i>	10	12	7	12	15	14	2	15	8	14	12	11
<i>ô, ă</i>	40	40	41	42	39	35	27	48	37	38	43	45
<i>ũ, ẽr, oo</i>	51	56	55	61	59	50	54	55	50	38	44	38
Total	281	266	277	301	322	255	290	306	276	263	282	262

and *õõ*, were counted as a single 'vowel' for present purposes. The same was true of the sounds marked *ô* and *ă*, *ou* and *oi*, and *ũ* and *õõ*. The other vowels tabulated were *ā*, *ǎ*, *ē*, *ẽ*, *ĩ*, *ĩ*, and *õ*. The resulting numbers of pairs of the nine most frequent vowels at various distances, compared with the expected numbers, are presented in Table IV and in the lower curve of Fig. 4.

The result differs considerably from that for consonants. A (barely significant) excess of pairs appears only when three syllables intervene. The frequencies in this column (Table IV) would be expected fewer than five times but more than twice in 100 trials under random sampling. A uniform trend toward this point is evident beginning (where the vowels are adjacent) slightly below the chance expectation. The fifth point of the curve, however, is below chance and is the lowest point in the curve as a whole. Such a rapid change from a point significantly above, to one probably below, chance would be puzzling were it not for the fact that it occurs in that part of the curve which represents syllables occurring in approximately the same position in successive lines. It would appear that there is a tendency not to use the same vowel in comparable position in succeeding lines, and that this conflicts with a tendency to repeat a vowel after the intervention of three or four syllables.

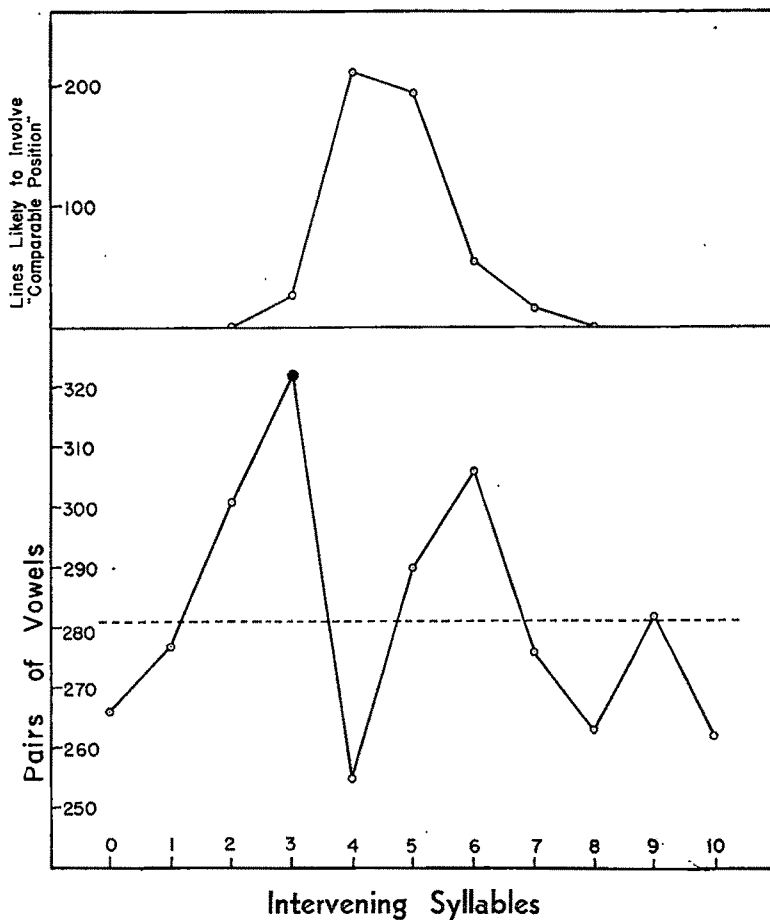


FIG. 4. SWINBURNE'S ASSONANT SPAN

Lower curve: The sum of the numbers of pairs of the nine commonest vowels is plotted against the number of syllables intervening between the syllables examined. The dotted line indicates the sum of the expected mean frequencies. The solid circle represents a set of frequencies significantly different from the expected frequencies. *Upper curve:* Distribution of lines of different length, placed on the horizontal axis in a position to indicate the numbers of lines likely to bring two syllables into comparable position in successive lines when the indicated number of syllables intervene. It is suggested that the effect of this distribution is, to some extent at least, to fill in the sharp break in the lower curve.

A test of this hypothesis was made by comparing syllables on the basis, not of a given number of intervening syllables, but of position within the line. The first and last syllables in successive lines were optimal for this purpose, because the question of the definition of 'comparable position' in the case of lines which did not have the same number of syllables was avoided.

In the 499 pairs of either initial or final syllables, we should expect 53 pairs of the same vowel, but the number observed in the initial position is only 43 and in the final, only 34. The greater sensitivity of the final position is perhaps related to the avoidance of perfect or imperfect rhymes. If this tendency of syllables in comparable position not to contain the same vowel is typical of the other syllables in the line, much of the sudden change in the curve for assonant span is accounted for.

It may be noted that a correction for the repetition of whole words brings the first two points of this curve to a position which is probably significantly below the expected mean, and that the peak of the curve at three intervening syllables is brought down well into the range of values to be expected from random sampling.

Although the result is by no means as clear-cut as in the case of alliteration, a tentative conclusion may be stated. There is apparently no process acting to increase the number of pairs of vowels which fall within a few syllables of each other; on the contrary there is apparently some suppression of similar words at those distances. Some formal strengthening is perhaps indicated after three or four intervening syllables. An observed tendency to avoid the same vowel in comparable position in successive lines interferes with the evidence for assonant strengthening. There seems to be no similar effect of comparable linear position upon the agreement of consonants.

Non-alliterative grouping. Alliteration does not, of course, exhaust the possibility of consonant-patterning. A more subtle arrangement may well exist without revealing itself in a consideration of single sounds. A preliminary check on this possibility was made by examining all successive pairs of consonants regardless of whether they were the same or not. A complete tabulation was made from which it could be determined how many times any given consonant was immediately followed by any other given consonant in the sample of 500 lines. Expected frequencies were calculated by multiplying the frequency of the first consonant by the p of the second. The standard error used in estimating the significance was computed as the square root of Npq , where N is taken as the frequency of the first consonant, and p and q are the usual probabilities of occurrence and non-occurrence, respectively, of the second. No difference between observed and expected frequencies was found as great as three times the standard error. A number of cases, however, yielded differences greater than twice the standard error, and it may be well to record them here for the sake of any future comparison.

All combinations of which at least five occurrences were expected are given in Table V. Thus, there is some tendency for *b* to be followed by *l* and not to be followed by *p* and *s*, and so on. It should be noted that a tendency for a consonant not to be followed by another consonant may be in part the result of the observed tendency of consonants to be followed by themselves. (In other words, we face the same difficulty in estimating *p* in the presense of grouping.) For example, if *b* is not followed by *s* as often as we should expect, it is partly because *b* is too often followed by another

TABLE V
ALL NON-ALLITERATIVE COMBINATIONS OF CONSONANTS OF WHICH AT LEAST FIVE OCCUR-
RENCES ARE EXPECTED AND IN WHICH THE DIFFERENCE BETWEEN THE OBSERVED
AND EXPECTED FREQUENCIES IS GREATER THAN TWICE
THE STANDARD ERROR

Consonant	Tends to be followed by:	Tends not to be followed by:
<i>b</i>	<i>l</i>	<i>p, s</i>
<i>d</i>	<i>l</i>	
<i>f</i>		<i>th</i>
<i>g</i>	<i>h</i>	
<i>l</i>	<i>n</i>	
<i>m</i>	<i>g</i>	<i>l</i>
<i>p</i>		<i>s</i>
<i>s</i>	<i>f</i>	
<i>t</i>	<i>b</i>	
<i>th</i>	<i>d</i>	<i>b</i>
<i>w</i>	<i>s</i>	

b and partly because many of the *rs* are tied up with other *rs*. A positive tendency toward association, however, is all the more significant.

Table V does not indicate any consistent sound-patterning over and above alliteration. There seems to be no tendency for one type of sound (dental, labial, and so on) to be followed by another or not to be so followed. Taking similarity of sound very loosely, we may note that *b* may tend to avoid *p* and *f* to avoid *th*, but, contrariwise, *s* apparently accumulates *fs*.

Non-assonant grouping. A similar attempt was made to discover combinations of vowels, in addition to repetitions, which showed exceptional frequencies. The only sequence which exceeded the expected frequency by more than three times its standard error was that in which *ā* was followed by *ē*. In two other sequences the excess was greater than twice the standard error: *ā* tended to be followed by *ā* and *ā* by *ō*. Two sequences showed a deficiency which was greater than twice the standard frequency: *ī* tended not to be followed by *ā* and the group *ū, ō* not to be followed by *ī*. The evidence is barely reliable, only a few sounds are involved, and no consistent trend (*e.g.* from low to high, or from back to front) is exhibited.

A '*coefficient of alliteration*.' A numerical 'coefficient of alliteration' would enable us to make a practical comparison of poems in the extent to which they indicate alliterative selection or arrangement in the behavior of the poet. A convenient and meaningful form would represent the number of sounds in some unit number of lines which produce an excess of lines containing two, three, four, or five instances of the same consonant. The value assumed by such a coefficient would depend upon the mean length of line and the range of lengths, as well as upon the arbitrary rules used in selecting syllables. Where these are comparable, a suitable coefficient may be set up by allowing one credit for every excess line containing two consonants, two credits for every excess line containing three, and so on, and by converting the total excess of the 10 commonest consonants into a (probably fractional) excess per line. The procedure is complicated by the presence of repetitions of whole words, but a practical compromise may be made by attributing to purely formal perseveration one-third of the repetitions of whole words. The precise magnitude of the excess, in any event, is obscured by factors already considered concerning the value of *p*.

In calculating the coefficient of alliteration for Swinburne, we find that the number of lines containing two instances of the same consonant, corrected by subtracting all repetitions of whole words, is 267. If we add to this one-third of the lines accounted for by the repetition of whole words, we get an observed frequency of 285, which gives us an excess of 36 lines over the expected 249, or a credit of 36. Similarly the corrected excess of 21 lines containing three instances of the same consonant may be increased by one line (approximately one-third of the two lines containing repetitions of whole words) to yield an excess of 22 lines or 44 credits. There is a similar excess of 11 lines containing four instances of the same consonant, or 33 credits. The total number of credits for Swinburne is therefore 113. Dividing this by the number of lines (500), we obtain 0.226 as Swinburne's coefficient of alliteration. A similar calculation for Shakespeare yields a value of only 0.007. If there were no alliteration whatsoever on the part of a poet, the mean coefficient would be zero, and it is clear that Shakespeare is very close to this. The upper limit of the coefficient would be obtained when all the consonants were grouped in solid blocks, but this is a case which need not be considered. It is probable that Swinburne's coefficient of 0.226 is very near the upper limit to be found in poetry which is not deliberately constructed (say, for humorous effect) upon a principle of alliteration or where alliteration is not the chief poetic device, as in Anglo-Saxon poetry.

CONCLUSION

To argue from the structure of a poem to the behavior of the poet is difficult. The pattern of a poem (in so far as it is patterned) is possibly due to something more than a process of formal strengthening. Contemporary standards or personal verbalizations concerning the function or structure of poetry will have their effects. When alliteration is in fashion as an ornament, the poet may deliberately seek it out, presumably through a kind of controlled association practiced at various points in the act of composition, or through the use of such an artificial device as a word book. On the other hand, where current taste is opposed to alliteration, instances which naturally arise from chance (as well as from formal strengthening) may be rejected. No statistical analysis of a poem will supply direct information concerning these activities, but it may nevertheless be regarded as a prerequisite to saying very much about them. We cannot trust the poet himself regarding his practices. He may be unaware of his own behavior in encouraging or eliminating alliterative words. Many poems written 'automatically' exhibit various kinds of formal patterning. In any event the poet's conception of a random order of sounds is probably inaccurate, and we may assume him incapable of estimating the extent of either process even if he is aware of it. Thus, although we may be supplied with a statement of the poet's philosophy of composition or an autobiographical account of his behavior as a poet, we are still not provided with an accurate statement of the structure of his poems. The selection of instances in illustration of the principles which he expresses is wholly unreliable.

We have no reason to assume that the amount or range of formal strengthening here exhibited is characteristic of the normal speech of either Shakespeare or Swinburne. The value of the research is not in supplying a rigorous measurement of the general process of formal strengthening but rather in estimating the length to which we may go in citing these poetic devices as examples of certain processes. The fact that rhyme, rhythm, alliteration, and assonance (the four principal elements of sound patterning in poetry) can be reduced to the single principle of formal strengthening is surely not an accident, nor is it an insignificant fact, but some such analysis as the present is required in order to relate these characteristics of poetry to normal verbal behavior.

It is to be hoped that this statement will allay the fears of those who react to such research as an intrusion into the field of criticism.⁸ It is

⁸ E. E. Stoll, Poetic alliteration, *Modern Language Notes*, 55, 1940, 388-390.

difficult to see how an analysis of this sort can have any bearing upon questions of taste, "sensitivity of ear," or, in short, upon any evaluative estimate of poem or poet. The question at issue is simply the objective structure of a literary work and the validity of certain inferences concerning literary behavior.

EFFECTS OF THE LOSS OF ONE HUNDRED HOURS OF SLEEP

By A. S. EDWARDS, University of Georgia

The first experimental study of loss of sleep in human Ss was performed by Patrick and Gilbert in 1896.¹ They kept three young Ss awake for from 88 to 90 hours. The results seem to show no significant physiological changes, a decrease in sensory acuity and in reaction-time, slower motor performance, failures in memorizing, and a slight gain in weight. One S reported visual hallucinations.

In 1922 Robinson and Hermann studied 3 Ss who lost 65 hr. of sleep.² Control periods both before and after the insomnia were used. No consistent or marked results seem to have been noted in performance scores.

Only male cases had been reported until Kleitman and his associates began experimenting which continued from 1922 to 1939.³ They used 35 Ss, most of whom were students, all but two, males. The periods of loss of sleep were "60-odd hours." The results include increased sleepiness in waves with the greatest difficulty from about 3 to 6 A.M.; increased difficulty in writing, reading, and studying; itching and dryness of eyes; sometimes seeing double; inability to count the pulse beyond 15 or 20 beats; increased tendency to fall asleep and to be resentful, irritable, and irascible; some degree of irrationality of action and some remarks that did not fit the occasion. Quantitative tests indicated no significant physiological results, but did show increased sensitivity to pain and decreasing ability to carry out tasks requiring sustained attention and effort.

Robinson and Hermann experimented with three young men who lost from 60

* Accepted for publication, October 11, 1940. The writer acknowledges with appreciation the assistance of Mr. Wendell P. Morris and his colleague, Mr. Harry Gerofsky, who were first interested in these experiments and acted as the first Ss. Early data with details of methods and apparatus were brought together by Mr. Morris in a Master's thesis; Mr. Lamar Beall was later interested in continuing the experiments in studying individual differences and in gathering and working out results of clinical observations and reports of Ss. It is impossible to give credit to the forty persons, more or less, who assisted at various times as Es, chaperons, etc. Special acknowledgment is due Dr. Florene Young and Professor Margaret May Ziegler of this department for their constant help. The recent publication of *Sleep and Wakefulness* (1939) by Nathaniel Kleitman with a bibliography of 1434 references, makes it unnecessary to give here more than a few special references.

¹ G. T. W. Patrick and J. A. Gilbert, On the effects of loss of sleep, *Psychol. Rev.*, 3, 1896, 469-483.

² E. S. Robinson and S. O. Hermann, Effects of loss of sleep, *J. Exper. Psychol.*, 5, 1922, 19-32.

³ N. Kleitman, Studies on the physiology of sleep: The effects of prolonged sleeplessness on man, *Amer. J. Physiol.*, 66, 1923, 67-92; Kleitman, S. Titelbaum and P. Feiveson, The effect of body temperature on reaction time, *Amer. J. Physiol.*, 121, 1938, 495-501; Kleitman, *Sleep and Wakefulness*, 1939, 300-321; M. A. M. Lee and Kleitman, Studies on the physiology of sleep: Attempts to demonstrate functional changes in the nervous system during experimental insomnia, *Amer. J. Physiol.*, 67, 1923, 141-152.

to 65 hr. of sleep.⁴ Their study indicated negative results except in mental multiplication. The general conclusion was that there were not any marked or consistent effects from loss of sleep.

Robinson and Richardson-Robinson used 3 women and 22 men with a control group consisting of 2 women and 37 men.⁵ The experimental group lost about 28 hr. of sleep. They were tested by means of the Army Alpha test for three days, different forms being used. Objective data seemed to be for the most part negative but the *Es* pointed to the importance of the "less objective data" which the present experiments have also shown to be of the greatest significance.

Laslett reported increased static ataxia especially for long periods of insomnia and with eyes closed.⁶ Katz and Landis reported loss of 231 hr. of sleep for one *S* without injury to health.⁷ The results emphasize the fact that, with increasing loss of sleep, delusions, queer behavior, irritability, and inability to sustain attention and do mental operations increase. Laird concluded from his study that *Ss* with insomnia, as compared with the control *Ss* make more errors in mental work after one night's loss of sleep.⁸

Laslett considers 50 hr. without sleep too short a time to give significant results. Kleitman thinks that 60 to 68 hr. without sleep gives all the results that are to be found with longer time-intervals.

Weiskotten acted as *S* and *E* and lost about 62 hr. of sleep, preceded and followed by control studies.⁹ He concluded that memory, attention, and speed were affected only if loss of sleep is sufficiently great—perhaps, about 36 hr. He and Ferguson obtained similar results later with 3 *Ss* who lost 66 hr. of sleep.

May Smith studied the effects of partial loss of sleep,¹⁰ acting as *S* and *E* over a period of about three years. She lost sleep during three days as follows: 1½ hr. the first night; 3½ hr. the second night; 5½ hr. the third night. In tests of dotting, relationship between words, tapping, and learning nonsense syllables, she concluded that positive results of significance appear only in connection with the dotting test and that of the relationship between words.

After extended experimenting, Kleitman, in his summary of all work on the subject, concludes that the effects of loss of sleep are more or less as follows:¹¹ continued muscular activity, desire to close the eyes and relax, lack of predictable changes in the visceral activities, normal mental and muscular efforts for short periods but sub-normal performance for sustained efforts, increased sensitivity to pain, impairment of disposition, tendency to hallucinations, and somewhat varied

⁴ Robinson and Hermann, *loc. cit.*

⁵ E. S. Robinson and F. Richardson-Robinson, Effects of loss of sleep, *J. Exper. Psychol.*, 5, 1922, 93-100.

⁶ H. R. Laslett, An experiment on the effects of loss of sleep, *J. Exper. Psychol.*, 7, 1924, 45-58; Kleitman, Titelbaum and Feiveson, *loc. cit.*

⁷ S. E. Katz and C. Landis, Psychologic and physiologic phenomena during a prolonged vigil, *Arch. Neurol. & Psychiat.*, 34, 1935, 307-316.

⁸ D. A. Laird, Effects of loss of sleep on mental work, *Indust. Psychol.*, 1, 1926, 427-428; Laird and W. Wheeler, What it costs to lose sleep, *Indust. Psychol.*, 1, 1926, 694-696.

⁹ T. F. Weiskotten, On the effects of loss of sleep, *J. Exper. Psychol.*, 8, 1925, 363-380.

¹⁰ M. Smith, A contribution to the study of fatigue, *Brit. J. Psychol.*, 8, 1916, 327-350.

¹¹ N. Kleitman, *Sleep and Wakefulness*, 1939.

results reported by various experimenters. Generally there were increased irritability and irascibility, and greatly increased amounts of effort demanded for any performance, especially, with loss of sleep for periods longer than 24 hr.

As to recovery after loss of sleep, Patrick and Gilbert indicated that 12 hr. are sufficient.¹² Kleitman reports that most Ss slept from 11 to 13 hr. Freeman¹³ seemed to think that recovery is slow, and Miss Smith considers recovery in her case was slow and rather great.

It appears that much of the greatest significance can be learned only by observation of the Ss, since by greatly increased efforts they can make the objective records of examinations compare favorably with those of control Ss. What does not appear in such records are the amount of effort expended by the Ss and the number of times they fell asleep and had to be awakened during the examinations, as well as the various peculiar forms of their behavior, language responses, and abnormal symptoms, and the degree of abnormality.

PROCEDURE

The present studies on loss of sleep were made in 1930-31, and in January, April, and November, 1938. They were started and carried on at the request of students. The students were eager to enter the experiment and many had to be refused permission because of the impossibility of handling so many Ss. Nineteen Ss began and seventeen completed the experiment. On the other hand, it was difficult to get control Ss of which we had only 10. There were, however, three types of controls: first, the control group who took the same tests as the experimental Ss; second, examinations of experimental Ss two days before they lost sleep; third, examinations of the experimental Ss two weeks after they had had normal rest and sleep. All tests were given as nearly as possible at the same time of day, although adjusted to the attendance of Ss at classes. Most of the tests were given from 9 A.M. to 1 P.M. The A.C.E. Psychological Examination obliged all Ss to be present at the same time. The Ss were required to have physical examinations by the college physician. Records were kept by him of blood pressure, pulse and oral temperature until time and schedule difficulties forced these tests upon the present writer. Physical tests were made daily and upon the advice of the college physician one man discontinued the experiment after two days. Severe rise in blood pressure and previous illness determined this decision. One woman dropped out of her own accord.

Tests included the following: reaction-time, tapping, aiming, hand grip, hand steadiness, static ataxia, instruction box, color perception zones, visual acuity, depth

¹² Patrick and Gilbert, *loc. cit.*

¹³ G. L. Freeman, Compensatory reinforcements of muscular tension subsequent to sleep loss, *J. Exper. Psychol.*, 15, 1932, 267-283.

perception, Ishihara color perception test, learning tests, consisting of telegraphy, typing and associative learning, A.C.E. psychological examination, memory, height, weight, patellar reflex, blood pressure, temperature, pulse. Standard procedure was used for these tests together with standard apparatus, and details are unnecessary, except for the listing of such apparatus as the following: Dunlap chronoscope and reaction-time apparatus, standard steadiness, tapping, and aiming tests as described in Whipple's manual, DeZeng perimeter, Ishihara tests for color blindness, Snellen's visual acuity chart, the telebinocular, Howard Dohleman's apparatus for depth perception test, Jastrow's tachistoscope, Ranchburg memory apparatus, four Healy instruction boxes. Some of the tests were continued only long enough to discover that results corroborated results of earlier investigators or were discarded because of the objections of the Ss. For example, Ss either refused to learn or objected to learning telegraphy and typing. Sixteen tests, however, were continued throughout the series of experiments. Thirteen men and four women completed the 100-hr. period without sleep.

RESULTS

Results of the study are reported in two main parts. First, the results of objective tests and examinations; and second, the results of clinical observations and reports of Ss themselves as to how they felt, whether or not they had headaches, eye strain, etc. Extensive statistical work was done on the recorded data, such as the computation of means, standard deviations and coefficients of variability. The most important results appeared from the combination of objective treatment with clinical observations and the Ss' own reports. It was uniformly found that by great effort, Ss, in the majority of cases, made their records highly comparable with their own two control records and with those of the control Ss, but observations and their own reports clearly indicated the extreme amounts of effort that increasingly appeared with greater and greater loss of sleep. The facts of greatest significance are the results that appeared in the combination of the two sets of reports.

Extended publication of the statistical tables is deemed unnecessary, since, for the most part, they show few positive results of loss of sleep, a result which corroborates the findings of former experimenters. The tables, however, are available in the Psychological Laboratory of the University of Georgia. Clinical observations and reports of Ss of their own experiences are given in somewhat more detail.

Table I shows the dates of experiments, and the sex, age, weight, and height of the Ss.

(1) *Reaction-time.* The Dunlap apparatus was used for reaction-time. The first two Ss complained of eye strain and visual reaction-time was discontinued with later Ss. Of the later Ss, however, only one complained of eye strain. With the exceptions of A and B, all reaction-times were auditory. The results are similar to those

already published, but the following points are to be noted. The means show that, though the differences were not large, the experimental group tended to react more slowly until they had lost 96 hr. of sleep, whereas the control group showed practically no change. After 96 hr. loss of sleep, 7 Ss reacted more quickly. The standard deviations, and coefficients of variability showed no difference of great significance when the experimental Ss and controls were compared, although the more extreme reactions, either slow or fast, occurred in the experimental group. The significant

TABLE I
EXPERIMENTS AND DATES TOGETHER WITH THE VITAL STATISTICS CONCERNING THE Ss

	S	Sex	Age	Weight	Height
Experiment 1 (1930-31)	A	M	24	199.0	72.1
	B	M	20	176.0	73.1
Experiment 2 (Jan. 18-22, 1938)	C	M	20	120.5	62.5
	D	M	21	151.0	66.5
	E	F	19	130.5	63.4
	F	F	20	97.5	59.5
	G	F	21	91.5	60.6
	H	F	20	125.5	65.0
Experiment 3 (April 19-23, 1938)	I	M	22	196.3	75.3
	J	M	21	187.0	74.4
	K	M	20	162.5	70.3
Experiment 4 (Nov. 8-12, 1938)	L	M	22	156.2	66.0
	M	M	21	174.7	69.2
	N	M	18	146.5	72.2
	O	M	22	150.0	67.5
	P	M	18	154.0	71.5
	Q	M	20	208.0	
	R	M	21	196.5	70.7
Control group (April 19-23, 1938)	E	F	19	130.5	63.4
	G	F	21	91.5	60.6
	H	F	20	125.5	65.0
	CA	M	23	154.1	73.1
	CB	F	19	126.0	66.5
	CC	F	21	117.6	61.9
	CD	M	21	137.8	66.6
	CE	F	21	92.5	60.5
	CF	F	20	122.8	64.9
	CG	M	24	135.4	68.8

fact appeared in the extreme efforts made by the experimental Ss to keep awake; the ready signal had to be loud and insistent, and, after 72 hr. without sleep, Ss frequently had to be awakened during the experiment. There were 50 reactions at each sitting. The actual reaction-times when tabulated compare favorably with those of the control group, but the difficulties of the Ss were increasingly great as loss of sleep increased.

(2) *Hand steadiness and hand grip.* The results show no differences of statistical significance. These tests did not take long and there seemed not to be as much

increase in difficulty as in the tests for reaction-time. Greater effort, however, was required as time went on. The only possible generalization appears to be that variability decreased slightly for the experimental group and increased slightly for the control group in hand steadiness. Both increase and decrease were found in strength of hand grip.

(3) *Static ataxia.* For the first two Ss the test of static ataxia in the Romberg position lasted 2 min.; for the remaining 15 Ss, it lasted 3 min. Whereas the 7 control Ss showed no regular changes from day to day, sometimes swaying more and

TABLE II
RESULTS OF TESTS FOR STATIC ATAXIA FOR EXPERIMENTAL AND FOR CONTROL GROUPS
The results for A and B are for 2 min.; all other results are for 3 min. The first two columns are the first control tests for the two days before the loss of sleep.

Ss			24 hr.	48 hr.	72 hr.	96 hr.	2 weeks
Experimental	A		26	85	118	183	20
	B		23	38	38	88	19
	C	131	—	199	130	111	213
	D	—	139	102	130	185	278
	E	296	333	194	463	342	796
	F	167	167	148	111	83	120
	G	204	185	204	139	162	148
	H	611	148	250	407	241	537
	I	414	160	301	150	197	206
	J	320	179	282	244	432	507
	K	320	282	226	113	376	169
	L	76	119	295	167	254	321
	M	198	169	132	91	288	114
	N	127	265	223	280	319	172
	O	427	287	183	290	216	514
	P	103	93	105	90	123	185
	R	—	179	331	284	338	621
Control	CA	207	244	188	169	282	150
	CB	—	808	357	301	432	263
	CC	—	56	206	263	88	94
	CD	—	—	66	94	188	—
	CE	282	75	75	75	244	94
	CF	—	432	301	263	301	357
	CG	116	56	244	38	188	282

sometimes less, the experimental group showed the following results: 7 swayed more at the end of 24 hr.; 7 at the end of 48 hr.; 11 at 72 hr.; and 13 at the end of 96 hr. without sleep. Large irregularities occurred in the two control periods before the loss of sleep, the control after two weeks' rest and the results of the 7 control Ss who took the tests without loss of sleep. Notwithstanding these irregularities that appear for various reasons irrespective of loss of sleep, the effects of the loss of sleep seemed to be definitely apparent. Observation also indicated great effort on the part of those who had lost sleep. One S fell off the platform during one test. Careful measurement of ataxiagraph records was made with a possible error of 2%. (See Table II.)

(4) *A.C.E. psychological examination.* Some of the most important results were

obtained from the records of the American Council on Education Psychological Examination. Different editions were used each day and the results reduced to percentile ranks by means of the tables published in the Educational Records for April of the years required. Effects of loss of sleep appeared not only in the efforts of the Ss on succeeding days and the difficulty of keeping them awake but can also be seen in the following facts. After 24 hr. without sleep, 13 did better, 3 worse; after 48 hr., 6 better, 11 worse; after 72 hr., 5 better, 11 worse; after 96 hr., 2 better, 14 worse; after two weeks of rest and sleep, 16 did better and 1 worse. The one man who did worse after rest and sleep made a *PR* of 72 on the first control examination before loss of sleep; during loss of sleep he made on successive days the following *PR*: 93.8, 94.6, 93.8, 93.6; and after two weeks of rest and sleep, 84.

Twelve control Ss made very uneven records, on some days being better, on others poorer, than on the preceding day. The median *PR*s for the control group of 12 students show the following: 89, 92, 82.9, 84, 91, 87.

The median *PR*s for the 17 experimental Ss, show the following: before loss of sleep, 60.5; after loss of sleep for 24 hr., 71; for 48 hr., 66; for 72 hr., 40.5; for 96 hr., 34; and after two weeks with regular sleep, 76.

The particularly significant results appear after the loss of 72 hr. of sleep and especially after insomnia for 96 hr. At the last period, 8 Ss did less than half as well as they did at the 72-hr. period: one of these failed utterly; one had a *PR* of 4; the other, a *PR* of 6.

(5) *Memory*. Tests with the Ranschburg memory apparatus showed in general somewhat irregular results for both experimental and control groups, no striking differences appearing between the two groups until after the loss of 72 hr. of sleep. At the end of this period, 6 experimental Ss showed lower scores; at the end of 96 hr., 14 experimental Ss had scores from one-fourth to less than half the former scores for words. For nonsense syllables no significant results appeared until the 96-hr. period. Then 12 experimental Ss had lower scores, but variation in all scores limit the significance of this result. Memory for digits seemed not to be particularly affected by loss of sleep.

(6) *Physical and physiological tests*. No significant changes were found in height or weight. There were slight increases in weight for the experimental group, perhaps due to the fact they ate more than usual, and slight drop in oral temperature. The pulse was modified by the activities of the Ss prior to taking the examination, and examinations were frequently postponed for some minutes until the Ss were quiet and free from the effects of such activities as going up stairs. Only one S showed notable change in blood pressure and he was advised by the physician and the present writer to discontinue the experiment. His rise in blood pressure may have been due to previous illness.

(7) *Patellar reflex*. Careful measurements were taken during the early experiments but were later discontinued; no positive results were discovered.

(8) *Tests of learning*. Three tests of learning—telegraphy, typewriting, and learning associated pairs of words—were tried but discontinued, partly because no very definite results appeared, partly because the Ss objected to or refused to take these tests. Ten Ss took the typing practice and tests, which were standard material

and tests obtained from a business school. Taking the first record as the standard, it may be said that 3 of the Ss made fair improvement in words per minute even through the 96 hr. without sleep, but only one did as well after two weeks of rest with regular sleep. The others barely held the slight initial ability with which they started or did worse, especially after 72 and 96 hr. Although it seems to be possible to make progress in learning to type during continued loss of sleep, it is only with increasing difficulty and effort, and with increasing irritability and unwillingness.

The tests of learning telegraphy and associated words were soon discontinued; they showed no results of significance.

(9) *Vision.* Of the various senses, vision was the only one systematically studied. Visual acuity was tested with the Snellen charts and the telebinocular; color blindness by the Ishihara charts; color zones by the DeZeng perimeter; and depth perception by the Howard-Dohlman depth apparatus and the telebinocular. In agreement with earlier work the present studies show no results that can be attributed to the loss of sleep. These tests were discontinued before the end of the experiment.

(10) *Healy instruction box.* Ss acquired skill in opening the boxes and of the 8 who opened the box on successive days, 7 found it more difficult on the last day than on the trial two weeks after they had had rest and sleep. With the one exception, after two weeks' rest and normal sleep, all opened the box in about half the time it took after the 96-hr. period without sleep. Every S opened four boxes successively on each day. The improvement after two weeks' interval cannot easily be understood, even without consideration of the effects of loss of sleep. It is possible that this test should have been continued and given to all the Ss.

CLINICAL RECORDS

The clinical notes indicating the actual behavior of the Ss and remarks made by them indicate certain facts. In all cases the following manifestations appeared: dozing, falling asleep, irritability, inattention, difficulty in giving attention, greatly increased effort for anything that required sustained attention and especially the A.C.E. Psychological Examination, increasing difficulty in maintaining equilibrium including staggering while walking, increasing and continual restlessness and desire to sleep, and waves of feeling very sleepy and very wide awake. For all Ss the most difficult time to stay awake was from about 3 A.M. to 6 A.M. They were inclined to feel quite wide awake sometime after breakfast and early in the evening. Practically all found it difficult to read, especially after 48 hr. without sleep. Study became increasingly more difficult and generally practically impossible after that length of time without sleep.

In fewer cases the following appeared: 10 Ss had difficulty in remembering what they had said or done in the preceding 5-30 min. Ten had either hallucinations or pseudo-hallucinations. Seven undoubtedly had what may be called hallucinations and three questioned their experiences, a fact which probably means that the experiences were not true hallucinations. Six reported headaches, three suffering severely

and more or less continually. One *S*, who suffered severely from headache, said that he had never had a headache before.

Fatigue was definitely reported by only 5 *Ss*—this may be due to the fact that they were encouraged to rest and to keep in as good a physical condition as possible without going to sleep. Only one *S* could be said to be exhausted; he was unable to take the last A.C.E. psychological test. It was actually slightly more than 100 hr. however, before he went to bed and his recovery then was rapid. Illusory experiences, such as seeing double, were reported by 5 *Ss*. One *S* reported a person across the table as appearing and disappearing from vision several times. Despite the increasing irritability that was general, 4 *Ss* maintained surprising good humor with smiles and cheerful remarks even though irritability also appeared at times.

Other symptoms, appearing in from 2-4 *Ss* each, are in order of frequency: nonsensical or irrelevant remarks; hearing sounds seemingly at a considerable distance; becoming dizzy from smoking a cigarette; feeling dizzy at other times; straying off the walk; things appearing hazy; finding it difficult to think about some particular thing; acting in silly ways. There were 4 *Ss* who had largely increased appetites and 2 who ate less than usual. Eye strain and backache was reported by 2 *Ss*. Two *Ss* said they were unable to distinguish hot and cold when they took shower baths; 2 had definite difficulty in speaking, being unable to select the words they wanted. Two *Ss* reported no feeling in the skin.

Other manifestations definitely indicated by only one *S* include the following: feeling of a band pressing on the head; objects in eye; feeling light as though one were walking on things that were very soft or the feeling that the pavement was mushy; incoherent remarks; actually walking into a hedge; walking off with apparently no idea of location or destination; numbness across the forehead; clearness of attention for a few moments and then everything fading away; dressing incorrectly and having to dress over again; feeling for the hat when not wearing one; hay fever and sinus trouble stopped although *S* had suffered continually before the experiment began; falling off the platform of the ataxiograph apparatus; stomach-ache; no feeling in finger tips; delusional statements; mechanically repeating what someone said; periods of euphoria and depression during the last day; peculiar tingling feeling all over body; slight depersonalization; talking and failing to remember; reported dreaming; mispronouncing proper names; holding book upside down; and seeing double.

Considering the results of the objective tests and the clinical reports, together the effects of loss of sleep may be much more clearly understood as the result of greatly increased and increasing effort. Most experimental records could be maintained at a good level. The effects of the loss of sleep appeared most definitely when the effort had to be maintained for one hour as in the psychological examination, for failure in ability to attend and to work the problems was obvious both in percentile ranks and in the behavior of the *Ss*. At least twelve symptoms appeared universally for the experimental *Ss*, none of which appeared in the control *Ss* or at the control periods for the experimental *Ss*; viz., abnormal amount of dozing, falling asleep, irritability, inattention and inability to sustain attention, increasingly great effort required for various tasks, difficulty in maintaining equilibrium and staggering, recurring waves of feeling very sleepy and then feeling wide awake, increasing desire to sleep as well as increasing restlessness, general indifference and extreme desire to be let alone, and much reduced ability to read or study. Nearly as frequent were the hallucinations or pseudo-hallucinations and illusions.

Of definite significance is the number of abnormal symptoms per S. In the 17 Ss among the men, the abnormal symptoms noted above appeared in frequency from 10 to 30 times, and among the women from 13 to 16 times; that is to say, from 10 to 30 abnormal symptoms appeared in each of the experimental Ss. For the men the actual number of abnormal manifestations were as follows: 30, 25, 25, 19, 24, 14, 14, 10, 11, 11, 15, 14, 11; for the women, 16, 13, 16, 16. More complete notes would probably have increased this number of symptoms for some of the Ss but not greatly.

Some details from clinical notes are as follows:

J. Very irritable at various times. Said he felt like fighting. Objected strenuously to being kept awake. Walking towards a jonquil bed, he stopped and asked if there was a dog or cat there. Felt sick standing for measurement of static ataxia. Tried several times to get hat off head; was not wearing one. Played cards and, after hand was played, he thought he was still playing the hand. Said he felt fine while taking a walk; then felt dizzy and soon seemed to be thoroughly drunk. Continually walking off sidewalks and getting on lawns or terraces.

I. Nearly fell several times when being measured for static ataxia. Was angry at one of these times and jerked out of the apparatus. Staggered a great deal. Tried to walk through a window thinking it was a door. In the middle of street would try to step up on curb. Once reported that sides of head had no feeling and that skin felt dead.

D. Reported peculiar tingling feeling all over body. Talked only of a football team in home state. Refused to get out of car. Failed to recognize a friend. Silly giggling and laughing. Regained normal behavior after walking about.

E. Felt light as if walking on air. Voices of those present seemed to be far away. Said she felt as if she were her mother and that she thought she had heard her mother. Said that people and voices seemed to be in the distance. Saw gloves on a girl's hand when the girl had on no gloves.

G. Heard housemother call her when latter was not there. Voices sounded far away. Touched her skin and said she felt as though someone else had touched her. Her speech became slower and seemed to be much more difficult. Reported some periods when she was very sleepy and could not see at all; then suddenly became very wide awake and could see clearly.

H. Constantly counted people to see if all were there. One missing, failed to count herself. Taken in car to ride and shortly afterwards did not remember trip. Difficulty in climbing stairs; had to use banisters. Could not remember things. "When anyone asked me who was in the experiment, I couldn't think of anybody."

Several Ss took walks or rides in automobiles and, shortly afterwards, could not remember the walks or rides.

Recovery-time. An attempt was made to determine the amount of sleep necessary for full recovery after 100 hr. without sleep. The results show large individual differences. The first 2 Ss (*A* and *B*) made up their sleep with about half the sleep they had lost. Of the other 9 Ss for whom accurate information was obtained, the results indicate the following: one man reported that he did not feel recovered until he had had extra sleep for two weeks. The most powerful of the Ss, a man, required 86% of lost time for recovery. The smallest and one of the least athletic men recovered in 57% of the time lost. Another made up his lost sleep in 55%

of the time lost. One required 50% and two 29% of the lost time to recuperate, the others about one third extra time. For the other Ss for whom accurate times were not recorded, it can only be estimated that it took from about one third to one half the time lost to make recovery. Careful observation and discussion with the Ss indicated that certainly, for some of them if not for most or all, the extra sleep taken directly after the close of the experiment did not really return them to normal; that they did not fully recover until they had lived and slept normally for a week or even more.

SUMMARY

This study reports results of loss of sleep for 100 hr. for 13 men and 4 women. The controls consisted of the examination of the experimental Ss before and after losing sleep, and of 10 Ss who took the tests without any loss of sleep. Standard tests and examinations were used.

(1) To a large extent, the results corroborate the findings of other investigators but add to the information already available.

(2) No significant results appeared for the determinations of blood pressure, oral temperature, pulse rate, height, and weight.

(3) The patellar reflex seemed not to be affected.

(4) Auditory reaction was somewhat affected. The results shown in means, standard deviations, and coefficients of variability did not appear to be particularly significant, but graphs of individual reactions indicated extremely slow followed by extremely quick reactions.

(5) The hand steadiness and hand grip may improve or grow worse but not more so than with control Ss.

(6) Static ataxia increased for most Ss and showed large and definite effects of loss of sleep.

(7) Very significant effects were noted in the results of the A.C.E. Psychological Examination, especially after 72 hr. and more especially after 96 hr. Five Ss practically held their own in these tests throughout the entire experiment but obviously with only the greatest effort. Results for the experimental Ss and the controls are different. Three experimental Ss were practically unable to do anything after 96 hr.

(8) Experimental results showed deterioration of memory at the end of 72 hr. and especially at the end of 96 hr. Comparison of experimental Ss with controls and variations in all scores, limit somewhat the significance of these results.

(9) In tests of learning, which were discontinued on account of the unwillingness and irritability of some Ss, varied results appeared and it is only possible to say that a few Ss seemed to be able to make progress in learning but with the greatest difficulty, while others barely held the initial ability with which they started.

(10) Tests of vision showed no positive results.

(11) Results for the Healy instruction box indicated that, although the objective results are not indicative of clearly positive results, it appears that this kind of test, required over a longer period of time, might show definite interference from loss of sleep.

The combination of experimental results with clinical observations and records makes a different picture.

(12) Records of the experimental group on objective tests and examinations were made with increasingly greater effort, irritability, and difficulties of attention, on the part of *S*.

(13) At least 12 definitely abnormal symptoms appeared in all *Ss*.

(14) A considerable number of other abnormal symptoms were noted for these *Ss*.

(15) For the experimental group more than 50 abnormal symptoms appeared in the record.

(16) The abnormal symptoms were in number from 11 to 30 per *S*, the median for the men was 14; for the women, 16.

(17) Of the greatest significance is the fact that the following symptoms appeared in excessive degree for all the experimental *Ss*: dozing, falling asleep, irritability, inattention and inability to sustain attention, effort required for various tasks, difficulty in maintaining equilibrium walking and climbing stairs, recurrent waves of feeling very sleepy and then feeling very wide-awake, increasing desire to sleep, increasing restlessness, general indifference and the desire to be let alone, much reduced ability to read or study and the tendency to hallucinations and illusions.

(18) The women came through the experiment as well or better than the men; the smaller and lighter individuals suffered the least; the stronger and more athletic men suffered the most.

CONDITIONING UNDER ANESTHESIA

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This is a report of preliminary attempts to make careful studies of learning by unconscious Ss: an experiment in which the eyelid-closing reflex of cats under sodium evipal anesthesia was conditioned.

ANTECEDENTS

The effects of drugs upon conditioned responses already built up have been studied by various investigators. Zavadski in 1908 in Pavlov's laboratory¹ found that alcohol depresses conditioned responses already set up, and presumed that this indicates depression of the activity of higher parts of the central nervous system. He found that the conditioned responses reappeared in from 30 min. to 4 hr., and that next day all the conditioned responses were increased in strength. In 1910 Nikiforovski, working under Pavlov,² found that small doses of alcohol had a disinhibiting effect on conditioned responses. Larger doses were found to act as Zavadski reported, diminishing or entirely abolishing positive conditioned responses. This work also showed that the day after administration of the alcohol the conditioned responses had become much stronger. Andreyev, a pupil of Pavlov working at McGill, in 1934³ found that by the first action of small, single doses of alcohol positive conditioned responses were inhibited and negative conditioned responses were disinhibited. In a later stage of the effect of such doses, and soon after the administration of large, single doses, it was stated that general depression of higher forms of nervous activity was evident, and complete abolition of conditioned responses occurred. The after-effect of large, single doses for several days after administration was found to be depression of 'activity of the hemispheres,' that is, of conditioned responses.

Wolff and Gantt in 1935⁴ showed from experiments that there are two types of drugs affecting conditioned responses: (a) stimulants, like caffeine sodiobenzoate, which lower the threshold of the 'highest integrative functions,' of which the conditioned response was taken to be an expression; and (b) depressants, like sodium bromide, which raise the threshold of the highest integrative functions. Gantt in 1935,⁵ working with conditioning in dogs, found that alcohol increases the latency

* Accepted for publication June 19, 1940.

¹ I. V. Zavadski, *Trans. Soc. Russian Physicians*, 1908. This is in Russian. Andreyev (cf. note 3) reviews the work.

² P. M. Nikiforovski, Thesis, St. Petersburg, 1910, 131-169. Also in Russian. Andreyev (cf. note 3) reviews the work.

³ L. A. Andreyev, The effect of single and repeated doses of alcohol on conditioned reflexes in the dog, *Arch. Internat. de Pharmacodyn. et Therap.*, 48, 1934, 117-128.

⁴ H. G. Wolff and W. H. Gantt, Caffeine sodiobenzoate, sodium iso-amylethyl barbiturate, sodium bromide and chloral hydrate effect on the highest integrative functions. *Arch. Neurol. & Psychiat.*, 33, 1935, 1030-1057.

⁵ W. H. Gantt, Effect of alcohol on cortical and subcortical activity measured by the conditioned reflex method, *Bull. Johns Hopkins Hosp.*, 56, 1935, 61-83.

of both secretory and motor responses even in moderate doses. In larger amounts it decreases the intensity of the conditioned reactions, disturbing the balance between excitation and inhibition. In 1937 Dworkin, Bourne and Raginsky⁶ tried the effect of (a) amytal, alcohol, nembutal, paraldehyde, and avertin; (b) bulbo-capnine, carbon dioxide, nitrous oxide, and ethylene; and (c) morphia and hyoscine on a lid-lifting conditioned response in dogs. The drugs of the first group, which were found to act alike, produced symptoms such as Nikiforovski and the Russian group had reported—weakening of negative aspects of behavior, with a reappearance of previously suppressed behavior, *i.e.* disinhibition. The second group chiefly suppressed usual activity, not apparently by depressing processes of excitation, but by deepening inhibition. The third group acted in an entirely different way, seemingly by causing nausea and disturbance of appetite.

It has been frequently stated that the conditioned response is the basis of the 'highest integrative functions,' and Pavlov's school has mentioned that the cortex is necessary for conditioning. It is obvious that this question is a vital part of the problem of the effect on conditioned responses of drugs known to have some action on the cortex. Pavlov, discussing the possibility of establishing conditioned responses in decorticate dogs, said: "To present the final conclusion of these experiments with the utmost reserve, the cerebral cortex should be regarded as the essential organ for the maintenance and establishment of conditioned reflexes, possessing in this respect a function of nervous synthesis of a scope and exactness which is not found in any other part of the central nervous system."⁷ This doctrine is still maintained by some, oftentimes without 'the utmost reserve,' and has been fundamental in much of the theory of the conditioned response. Wolff and Gantt in their consideration of the effect of drugs on the rôle of the cortex in conditioning say: "For the formation of a conditioned response three factors are essential: (1) The animal must have a cerebral cortex. (2) The receptor end-organs, the afferent pathways and the efferent apparatus must be intact. (3) A hitherto inconsequential stimulus must precede and overlap in time the presentation of a second stimulus which elicits an inborn reflex."⁸ It is doubtful, in the light of recent evidence, whether any of these three statements is entirely true.

Poltyrev and Zeliony, in 1929,⁹ succeeded in developing what they considered conditioning to sound and discrimination in the dog after removing an undetermined amount of the cortex. Culler and Mettler, in 1934,¹⁰ carried this line of investigation farther by developing conditioning in a dog in which only a few shreds of cortex, most probably non-functional, were present. On the basis of their results they state: "The decorticate is just as conditionable in a random, diffuse way as the normal; the bell-impulses soon find their way into the animal's existing systems of defense and escape; but she remains fatally stereotyped, inadapttable, incomparably less capable of achieving and fixating a localized, efficient response. . . . We conclude that

⁶ S. Dworkin, W. Bourne, and B. B. Raginsky, Changes in conditioned responses brought about by anesthetics and sedatives, *Can. Med. Assoc. J.*, 37, 1937, 136-139.

⁷ I. P. Pavlov, *Conditioned Reflexes* (trans. Anrep), 1927, 330.

⁸ Wolff and Gantt, *op. cit.*, 1031.

⁹ S. S. Poltyrev and G. P. Zeliony, Der Hund ohne Grosshirn, *Amer. J. Physiol.*, 90, 1929, 475 f.

¹⁰ E. Culler and F. A. Mettler, Conditioned behavior in a decorticate dog, *J. Comp. Psychol.*, 18, 1934, 291-303.

the cortex is required, not for simple diffuse conditioning (direct discharge of substitute impulses into existing action-systems) but for localized, adaptive responses."¹¹ Marquis and Hilgard, in 1936,¹² studied conditioned responses after complete extirpation of the visual cortex in dogs. It was found that the general form of the responses developed in this state was normal, with a slight increase in latency. The conditioned responses developed in normal dogs, which then underwent occipital lobectomy, were practically normal five days after the operation, except for increased latency. After injection of 0.6 c.c. per kg. of a 10% solution of sodium amytal intraperitoneally, however, the conditioned response which disappeared returned much more rapidly, in 24 hr. So anesthetizing the animal (the surgical level was finally reached with further injection) did not have the same effects as removing the occipital cortex. These workers commented that it may be hypothesized that the function of the visual cortex with respect to the conditioned lid response is a maintaining of a facilitatory background for the responses, in other words, a lowering of the threshold of response to light.¹³ This was inferred from the longer latencies after the operation.

The antecedent work most like that reported in this article was the investigation of Settlage reported in 1936,¹⁴ which is the first published conditioning done under the influence of a general anesthetic. He conditioned flexion of the hind leg to a bell in 12 cats which were in a state of depression due to injections of sodium amytal. He found that there was a 'critical state' of depression, in which conditioned responses could be formed, but these responses could not be elicited until the effects of the drug had worn off. There were individual differences in the amount of the drug per kg. of body weight necessary to bring about this state. In one cat it was found that this critical state was deeper for visceral muscle responses than for skeletal. Since in two others this was not the case, there is certainly insufficient evidence to make this finding definite. The same critical state was found in preliminary work with nembutal and alcohol. The critical state occurred at a light level of depression (about 10-12 mg. of sodium amytal per kg.). The corneal and righting reflexes were still normal. "The animals, while under the influence of this dosage, would seek and ingest food, and they would spontaneously walk around in the experimental chamber or the living cage. The only apparent modification of normal behavior was that the animals, even after all the excitement of handling and training, seemed to lie down and assume a sleeping pose more readily than do normal cats."¹⁵ This was apparently the deepest state in which Settlage could develop conditioning.

In our work attempts were made to obtain conditioning under much deeper anesthesia than the 'critical state' of Settlage.

PROCEDURE

Thirty cats were used in the main body of this work. None of them at the beginning of the experiment would blink its eyes when the buzzer used in the conditioning

¹¹ Culler and Mettler, *op. cit.*, 300 f.

¹² D. G. Marquis and E. R. Hilgard, Conditioned lid responses to light in dogs after removal of the visual cortex, *J. Comp. Psychol.*, 22, 1936, 157-178.

¹³ Marquis and Hilgard, *op. cit.*, 175.

¹⁴ P. Settlage, The effect of sodium amytal on the formation and elicitation of conditioned reflexes, *J. Comp. Psychol.*, 22, 1936, 339-343.

¹⁵ Settlage, *op. cit.*, 342.

was sounded. Each of the cats was injected with sodium evipal (1-methyl-5 Δ -cyclohexenyl-5-methyl sodium barbiturate). This was administered in two ways, intraperitoneally and intravenously. Intraperitoneal injections were made to get the cats to the lighter levels of anesthesia, and in a few of these cases it was necessary to give additional doses to reach the desired level. Intraperitoneal injections, in sufficient quantity to reach the deepest level of anesthesia used in this experiment, were frequently fatal, so in order to assure survival slow intravenous infusion was used to reach this level.

As is well known, the dosages necessary to reach a certain level of anesthesia vary greatly between individual animals. The single intraperitoneal doses varied from 26 mg. per kg. of body weight to 55 mg. per kg. Intravenously the dosages varied from 50 mg. per kg. to 100 mg. per kg. As will be seen, in some of the cats an operation was necessary, and this was performed under ether. These animals were allowed to return practically to normal before the evipal was administered. With two cats, however, the intravenous infusions were begun before the effects of the ether had worn off, and in these cases (cats Nos. 20 and 21) much lighter doses of evipal sufficed to reach the deepest level of anesthesia. All other animals may be considered to have been under pure evipal anesthesia.

After the administration of the drug intraperitoneally, or throughout the intravenous infusion, the level of anesthesia was carefully watched until the desired state had been reached. Four levels of anesthesia (A, B, C, and D) were arbitrarily defined on the basis of the presence or absence of certain reflexes. In determining the depth of anesthesia of each cat the following indices were observed:¹⁰ (1) running; (2) posture; (3) righting reflexes; (4) correction of unusual positions, ability to return to a normal position an extremity moved by E; (5) tenseness of leg musculature; (6) tenseness of abdominal musculature; (7) twitches of the ear and face to painful stimulation of the ear; (8) nystagmus; (9) knee jerk; (10) homolateral leg-flexion to the pinch of a paw; (11) homolateral leg-flexion to stimulation of the central end of the cut sciatic nerve; (12) corneal reflex, eyelid closing to a light touch on the cornea; (13) eyelid closing to a puff of air. Table I relates these signs to the various levels of evipal anesthesia.

We have arbitrarily defined Level A as the range between the earliest observable ataxia in running and the point at which motor and balance control is lost to such a degree that the animal lies on its side, unable to lift even its head. The lower limit of Level B is the disappearance of leg-flexion to the pinch of a paw. At this level the cat tends to be hyperexcitable, the leg muscles are often tense and extended, and they frequently tremble. When lifted by the scruff, the cat commonly arches its neck to a right angle with the back. Sometimes when the cat is lowered to the ground, the hind legs make jerky, leaping motions. Commonly the tongue protrudes, and almost invariably it makes licking motions. As Level C is approached the muscles gradually relax and the cat becomes increasingly unresponsive to painful stimuli. Level C is the range between the disappearance of the leg-flexion to the pinch of a paw and the disappearance of leg-flexion to stimulation of the central end of the cut

¹⁰ Much assistance in developing these criteria was derived from the work of R. Magnus, *Körperstellung*, 1924. Especially useful was the article by the pupil of Magnus, O. Girndt, Die Ermittlung der Wirkungsstärke von Schlafmitteln mit Hilfe der Körperstell- und Labyrinthreflexe, *Arch. f. exper. Pathol. u. Pharmacol.*, 164, 1932, 118-157.

sciatic nerve.¹⁷ At this level the muscles are greatly relaxed and the cat is practically inert. The closing of the eye in response to a touch on the cornea becomes slower and less reliable and may vanish altogether, although the blink in response to a puff of air lingers and may be discernable in Level D. Somewhat deeper than the disappearance of the leg-flexion to sciatic stimulation, and the onset of Level D, respiratory paralysis and death supervene.

The definitive criteria and upper limits of our four levels of anesthesia appear in italics in Table I. They are: Level A—ataxia; Level B—recumbent position of the whole animal; Level C—loss of leg flexion to the pinch of a paw; and Level

TABLE I
INDICES USUALLY PRESENT AT THE VARIOUS LEVELS OF EVIPAL ANESTHESIA IN THE CAT

Indices	Level A	Level B	Level C	Level D
1. Running	<i>ataxic</i>	absent	absent	absent
2. Posture	from erect to lying on side with head upright	<i>whole animal recumbent</i>	whole animal recumbent	whole animal recumbent
3. Righting reflexes	normal to di- minished	absent	absent	absent
4. Correction of unusual posi- tions	normal	diminishing	absent	absent
5. Tenseness of leg musculature	normal	accentuated	diminished	slight
6. Tenseness of abdominal mus- culature	normal	normal	diminished	slight
7. Twitches of ear and face to painful stimulation of ear	normal	accentuated	normal to absent	slight to ab- sent
8. Nystagmus	normal	normal to absent	absent	absent
9. Knee jerk	normal	normal	normal to absent	absent
10. Homolateral leg-flexion to pinch of paw	normal	normal	absent	absent
11. Homolateral leg-flexion to stimulation of central end of cut sciatic nerve	normal	normal	normal	<i>absent</i>
12. Corneal reflex: eyelid closing to touch on cornea	normal	normal	diminished to absent	absent
13. Eyelid closing to puff of air	normal	normal	normal to diminished	usually ab- sent

D—disappearance of leg flexion to central sciatic stimulation.¹⁸ Despite variations among individual cats, the other indices listed in Table I usually disappeared in a regular order in relation to these four, but these are the four criteria by which the levels are defined. The exposure of the sciatic nerve was done only in the more deeply anesthetized cats when the attainment of Level D was sought.

After the cat had been anesthetized and the observations as to its level of

¹⁷ In our work the sciatic nerve was exposed and the femur relatively immobilized after the method of H. K. Beecher, F. K. McDonough, and A. Forbes, Similarity of effects of barbiturate anesthesia and spinal transection, *J. Neurophysiol.*, 2, 1939, 81-88.

¹⁸ This criterion has been used by H. K. Beecher and F. K. McDonough to indicate deep surgical anesthesia (Cortical action potentials during anesthesia, *J. Neurophysiol.*, 2, 1939, 289-307).

anesthesia had been made, it was put under restraint, so that the head and neck were allowed only limited movement.

The conditioning was accomplished by a puff of air on the cat's eye as the unconditioned stimulus and the eyelid-closing reflex as the response. The conditioned stimulus was a buzz which sounded a fraction of a second before the puff of air was given and continued for the duration of the puff. Series of conditioning trials were then given, most of the cats getting 100 trials. In every case far more trials than necessary to condition a cat in the waking state were given, except to Cat No. 1. This animal was at Level A, and showed conditioning after 3 trials. This was the only cat to give the conditioned response under anesthesia at the conditioning session.

The next day or as soon as the effects of the anesthetic had worn off, the cats were put back into the conditioning situation and tested with the buzzer alone to see if the eyelid closing could be elicited by the buzz. If it could not, an attempt was made to develop such a conditioning in the unanesthetized state.

RESULTS

Of the 30 cats, 22 could not be conditioned under anesthesia and 7 were conditioned. One cat, No. 30, will be considered separately. Table II

TABLE II
RESULTS OF CONDITIONING AT EACH LEVEL OF ANESTHESIA

	Level A	Level B	Level C	Level D
Number of cats given conditioning trials at this level	3	12	7	7
Number successfully conditioned at this level	2	3	2	0
Percentage successfully conditioned at this level	67%	25%	29%	0%

summarizes the results for 29 of these cats (all except No. 30), showing the number studied at each level of anesthesia, and the number and percentage successfully conditioned at each level.

The evidence in Table II points to Level C as the lowest at which conditioning can be accomplished. It was discovered that the eyelid-closing response disappears at the beginning of Level D, at about the same point that we found conditioning to be impossible. In no case could conditioning be achieved when the eyelid-closing response had disappeared.

Cat No. 30 was given conditioning trials at Level D, but after returning to the waking state showed no conditioning. Two days later it still showed no conditioning, but was anesthetized again, this time to Level C, and was again given conditioning trials. When it reached the waking state this time it showed conditioning. This was the only cat given more than one conditioning series, and it showed that the point at which the possibility of conditioning the eyelid-closing response disappears is between Levels C and D.

Of the 22 cats which were not successfully conditioned under anesthesia, 12 could not be conditioned even in the waking state.

DISCUSSION

(1) Our work has borne out Settlage in his basic statement that it is possible to develop conditioned responses under anesthesia. Moreover, we found, as did he, that there is a stage at which it is possible to develop conditioning but at which no conditioning is evident, and that the conditioned response can be elicited by the appropriate stimulus after the effects of the drug have worn off.

(2) An important difference between our work and Settlage's is that we found that conditioning can be developed at a much deeper level of anesthesia than he did. His critical state was probably not below Level A, while we were able to condition cats in Levels B and C. We found the point below which conditioning could not be developed was the upper limit of Level D. It is possible that this is a difference between the two barbiturates, amytal and evipal. It will be profitable to test conditioning under various drugs. We have already done preliminary work with ethyl urethane.

(3) An insistent problem of course is why some of the cats were conditioned and others were not. This is not understood now, but will require future investigation. The trauma involved in exposing and sectioning the sciatic nerve may have been an important factor. Two other hindrances to conditioning were observed. (a) Certain cats half closed their eyes under the restraint of the conditioning situation or responded to the first puffs by maintaining a motionless squint throughout the session. This precluded the developing of specific conditioning. (b) The nictitating membrane in some cats covered a large part of the eye and lessened the response to the puffs. Perhaps it was for these same reasons that some cats could not be made to give the specific conditioned response even in the waking state.

(4) As to the necessity of response in conditioning, our work has interesting implications. The response was capable of being elicited during the conditioning, because the puff of air made the eyelid wink, but the conditioned stimulus could not evoke it until the effects of the drug had worn off. (Cat No. 1 was the only exception to this, and it showed conditioning in the light Level A.) Both afferent and efferent tracts for the reflex were open, but the central mechanism was incapable of substituting one stimulus for another. It has of course already been shown that the ability to respond is not necessary for conditioning.¹⁹ This evidence, how-

¹⁹ Cf. this series of experiments: H. F. Harlow and R. Stagner, Effect of complete striate muscle paralysis upon the learning process, *J. Exper. Psychol.*, 16,

ever, has been based on experiments in which the efferent neurone or the effector was incapable of action. It is most probable that in the present experiment the interruption was central, for we were unable to condition the eyelid-closing response after it had disappeared.

(5) It is interesting to consider our results in the light of the evidence as to whether the anesthetized cat is essentially a decorticate preparation. Beecher and McDonough's study of cortical action potentials during anesthesia²⁰ is important in this connection. They observed, concerning the effects of a group of non-volatile drugs which included evipal, that the anesthesia produced results in cortical potentials more similar to those of sleep than to those produced by volatile agents like ether.²¹ Moreover they found that in anesthesia caused by any of the non-volatile, evipal group of drugs, central sciatic stimulation has no effect upon the voltage of the cortical waves, while in anesthesia caused by the ether group it does. They write: "Stimulation has no effect on the voltage of the typical cortical waves under any agent at a deep level of anesthesia. Attention is called to the fact that failure of stimulation to alter the voltages of any of the waves under the non-volatile agents is not due to failure of impulses to reach the cortex for 'secondary discharges' occur here in response to stimulation even (and usually) at deep levels of anesthesia. This indicates that pathways remain open. . . . The cortical voltages are a fundamental general characteristic of the cortex and not simply phenomena arising from isolated centers. The voltage of cortical waves under anesthesia is a labile characteristic easily affected in certain given cases by peripheral stimuli, but not affected in others, a characteristic uniformly altered by changes in depth of anesthesia."²² These findings concerning cortical potentials do not tell us whether the anesthetized cat is functionally decorticate, for while under deep surgical anesthesia the stimuli do not affect the cortical potentials, nevertheless the potentials are still there.

Moreover, if our cats were really temporarily decorticate, why was our conditioning not like that which Culler and Mettler²³ got on decorticate dogs? Most of our successfully conditioned cats developed specific eyelid reactions, which Culler and Mettler's dog did not do. This would seem to show that, though the animals were under anesthesia, a level which would

1933, 283-294; J. S. Light and W. H. Gantt, Essential part of reflex arc for establishment of conditioned reflex: formation of conditioned reflex after exclusion of motor peripheral end, *J. Comp. Psychol.*, 21, 1936, 19-36; E. Girden and E. Culler, Conditioned responses in curarized striate muscle in dogs, *ibid.*, 23, 1937, 261-274.

²⁰ H. K. Beecher and F. K. McDonough, *op. cit.*, 289-307.

²¹ Beecher and McDonough, *op. cit.*, 301.

²² Beecher and McDonough, *op. cit.*, 306.

²³ Culler and Mettler, *op. cit.*, 300 f.

be called 'unconsciousness' in man, they were not functionally decorticate. We thus confirm the finding of Marquis and Hilgard²⁴ that anesthesia is not the same as the decorticate state. What the Pavlov school referred to as 'activity of the higher parts of the central nervous system' (Zavadski) or 'activity of the hemispheres' (Andreyev)—*i.e.* conditioned responses—are present, though perhaps depressed, and the cortical potentials are present, showing some cortical activity. Still a human being in this stage would be 'unconscious,' and the animal is unable to act normally. This throws doubt on any glib equation of cortical activity and consciousness. Perhaps there are degrees of both—this is a problem for present surmise and future research.

It is at least demonstrated once more by our work combined with that of Beecher and McDonough that, as Poltyrev and Zeliony²⁵ and Culler and Mettler²⁶ showed, normal functioning of the cortex is unnecessary for conditioning.

(6) What implications may the present findings have for human learning and behavior? It has been shown in experiments by one of the authors²⁷ that it is possible for human beings to learn without awareness. In this work the Ss learned to discriminate subliminal stimuli. This indicates the desirability of future research along these lines, attempting to condition human Ss under anesthesia while employing a wide variety of stimuli including the spoken word. If such work were to yield positive results, we might speculate on the possible implications of the findings. An incident in a Boston hospital may serve as a basis for such surmises. In this case a surgeon, having performed an exploratory operation with negative findings, discussed in the presence of the lightly-anesthetized patient how he intended to alleviate the apparently 'functional' symptoms by telling the patient afterward that a successful operation had been performed. It is not entirely inconceivable that he might have done well to refrain from mentioning his notion while the patient lay on the operating table, lest his plan to trick 'the unconscious' be conveyed directly to that very 'unconscious.' This is sheer supposition but it suggests a direction for future investigation.

(7) One of the intentions of the authors in future experiments is to discover quantitatively how variations in levels of anesthesia affect the

²⁴ Marquis and Hilgard, *op. cit.*, 157-178.

²⁵ Poltyrev and Zeliony, *loc. cit.*

²⁶ Culler and Mettler, *op. cit.*, 291-303.

²⁷ J. G. Miller, The rôle of motivation in learning without awareness, this JOURNAL, 53, 1940, 229-239.

ease of learning. An adaptation of the Ebbinghaus method of savings could be used for this provided one were to select a conditioned response which, unlike eyelid closing, requires a large number of trials to establish. A constant number of conditioning trials would be given at various levels of anesthesia. Down to a certain level it would be found that the animals were completely conditioned. Animals given the same number of trials at deeper levels would not be. Then the number of trials necessary to finish conditioning these in the normal state would be determined. It is possible that this would increase as the depth of anesthesia increased, and still be less than in ordinary waking conditioning.

SUMMARY

Attempts were made to condition 30 cats under sodium evipal anesthesia. The unconditioned stimulus was a puff of air directed at the face of the cat, which elicited the eyelid-closing response. The conditioned stimulus was the buzz of an electric buzzer. Of the 30 animals, 22 could not be conditioned under anesthesia and 8 were successfully conditioned. Conditioning could not be developed at a depth of anesthesia at which the eyelid-closing reflex and the homolateral leg-flexion to sciatic nerve stimulation had disappeared.

ESTIMATES OF PAST AND OF FUTURE PERFORMANCES AS MEASURES OF ASPIRATION

By ERNEST R. HILGARD and EDWARD M. SAIT, Stanford University

Success and failure, as seen from the point of view of the experiencing individual, depend on what the individual is trying to do, and may have little relation to statistical norms of what he should be able to do. Consequently, measures of performance need to be supplemented by some sort of statement of goal if the performance is to be interpreted as successful or as unsuccessful. Attempts to compare an S's formulated goals with his performances have given rise to experiments on the level of aspiration. The process of objectifying the person's goal is not a simple one, for, as Gardner has so well pointed out,¹ there is always a certain amount of "editing" which takes place before the goals are announced or otherwise indicated to the experimenter. The most common score used in the discussion of level of aspiration is a measure of the discrepancy between the known performance score on a trial just completed and a statement respecting the score on the next trial. This statement may be in the form either of what S is going to "try for" or of what he "expects to reach."² It may be conjectured that such estimates of uncertain future performances will be distorted in accordance with the disposition of the individual to exaggerate or to depreciate what he is able to do. That such distortions occur is well testified by the evidence within the level of aspiration experiments already reported.

In order to find out something further about the basis for distortion of self-estimates, we have added another discrepancy score; namely, that between the self-estimate of the performance just completed and the true score on that performance. In this case the task set is that of describing

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¹ J. W. Gardner, The use of the term "level of aspiration," *Psychol. Rev.*, 47, 1940, 59-68.

² The form of the question upon which the statement of goal is based may make a difference, but to our knowledge no detailed comparisons are available. We have asked the Ss to estimate the future score; an alternative is provided in the study by P. S. Sears (Levels of aspiration in academically successful and unsuccessful children, Ph.D. dissertation, Yale University, 1939), in which she asked her Ss what they were going to "try for." Gould reports (An experimental analysis of level of aspiration, *Genet. Psychol. Monog.*, 21, 1939, 3-115) that with about 30 Ss she found no difference between response to "What will you do next time?" and "What do you intend to do next time?"

a past event instead of predicting a future possibility. It was felt that distortions occurring in self-estimates with respect to past performance might profitably be compared with the distortions in such estimates when the reference is to the future.

Since the true score is announced between the estimate of past performance and the estimate of future performance, every opportunity is given S to correct a trend towards over- or underestimation before the announcement of expected future performance. The possibility of over-correction must not be neglected. If, for example, S tends to estimate his past performance as too low, the announcement that his true score was much higher might lead to revised estimates of future performance erring in the opposite direction. If such were the rule, lack of correlation might be expected between the discrepancy scores based on estimates of past performance and those based on estimates of future performance. If, on the other hand, the level of self-estimate is little affected by realistic aspects of performance (hence is relatively rigid), a positive correlation may be expected between both discrepancy scores. That is, one who underestimates his own performances may do so with respect to past as well as future scores, while a person who overestimates his performances may interpret both past and future scores as too high. In view of these and other alternative possibilities, experimental demonstration of the relationship between the two scores appears desirable.³

Procedure. The two tasks used were card-sorting and manual pursuit-learning. Improvement in card-sorting, as measured by the time to sort a pack in a prescribed manner, is slow. Improvement in pursuit, on the other hand, is rapid. These differences proved to be of some importance in the self-estimates obtained.

The card-sorting was performed in the manner described by Husband and Miles.⁴ An ordinary deck of playing cards was sorted into the four compartments of a box, each compartment representing a suit. After the cards were shuffled, the top card was dealt into the upper left compartment, face up, the next into the compartment labelled with the suit of the card just dealt, and so on through the deck. That is,

³ In this study we are concerned primarily with the consistency of individual differences. Estimates for a group may be distorted in one or another direction through experimental arrangements, as by social suggestion (D. W. Chapman and J. Volkmann, A social determinant of the level of aspiration, *J. Abn. & Soc. Psychol.*, 34, 1939, 225-238) or the presence of a social group (H. H. Anderson and H. F. Brandt, Study of motivation involving self-announced goals of fifth grade children and the concept of level of aspiration, *J. Soc. Psychol.*, 10, 1939, 209-232; E. R. Hilgard, G. A. Magaret, and E. M. Sait, Level of aspiration as affected by relative standing in an experimental social group, *J. Exper. Psychol.*, 27, 1940, 411-421. In such cases the direction of distortion is a function of factors other than the personality characteristics of the group members.

⁴ R. W. Husband and W. R. Miles, On sorting packs of sixty cards with form and color as variables in two to six kinds, *J. Appl. Psychol.*, 11, 1927, 465-482.

each new card was placed in the compartment indicated by the suit of the previous card. The score for one trial consisted in the time in seconds required to complete the sorting of the deck in this manner. Practice continued for 25 trials, all within one experimental session. Following each trial, *S* made a guess as to the time he had required. This is known as the estimate of past performance. Then the correct time for this trial was announced by *E*. For convenience, the score on the performance just completed will be designated P_1 , that on the next following performance as P_2 . The first estimate secured is an estimate of P_1 , which may be designated EP_1 . Following the statement of the true P_1 by *E*, *S* estimated the score he would make on the next trial. This is known as the estimate of future performance and is designated EP_2 . The discrepancies between the actual scores and the estimates furnish the data under consideration.

The pursuit experiment was performed on another day by the same *Ss*. The task consisted in keeping the tip of a hinged pointer on a $\frac{3}{4}$ in. brass target revolving with the turntable of a phonograph, as described by Koerth.⁵ The turntable revolved once per sec. An electromagnetic device recorded 10 counts per revolution when the pointer was on the target continuously. Trials were of 1-min. length, so that the maximum possible score per trial was 600. As in the card-sorting experiment, there were 25 such trials within the single experimental period. Estimates of the just completed performance were followed in each trial by the announcement of the actual score by *E*, and then *S* estimated what he could do on the following trial. Thus the estimates, also, paralleled those in the card-sorting experiment.

Half of the 50 college students participating in the experiment performed first in the pursuit experiment, half performed first in the card-sorting experiment. Analysis of the data showed no significant trends to be associated with the order of tasks, so that all the *Ss* have been thrown together in the treatment of results.

Three different discrepancy scores have been derived from the true scores and estimates. One measure of distortion in self-estimates is the discrepancy between the estimate of past performance and the true score on that performance ($EP_1 - P_1$). Another, that more common in level of aspiration experiments, is the discrepancy between the estimated future performance and the score prior to the estimate ($EP_2 - P_1$). For some purposes, particularly in determining the degree of realism in the estimates, the estimated future performance has been compared with the actual future performance ($EP_2 - P_2$). Since it takes a trial or two in the experimental situation before *S*'s estimates can be coordinated with known scores, discrepancy scores based on estimates within the first trial have been omitted. Hence the first performance score entering into ($EP_1 - P_1$) is that of the second trial; similarly, for ($EP_2 - P_1$) the first performance score entering is also that of the second trial, while for ($EP_2 - P_2$) the first performance score is correspondingly the one for the third trial. With these exceptions, all of the available estimates and scores were used in securing an average value for each of the discrepancy measures for each of the subjects on each of the two tasks.

Results. Reliabilities of the various measures as determined by correlating odd and even trials and applying the usual correction are given in Table I. The lowest reliability is that for discrepancies based on estimates of past performance in card sorting ($EP_1 - P_1$). This may appear to be out

⁵ W. Koerth, A pursuit apparatus; eye-hand coordination, *Psychol. Monog.*, 31, 1922, (no. 140), 288-292.

of harmony with the high reliability for the card sorting discrepancy based on estimates of future performance. Estimates of past performance in card sorting were so accurate, however, that the range of discrepancy scores was greatly restricted, thus reducing reliabilities. The other reliability

TABLE I
RELIABILITY OF PERFORMANCE SCORES AND OF DISCREPANCY SCORES
($N=50$ in this and in all the following tables.)

	Odd-even reliability (r and σ_r)	
	Card sorting	Pursuit learning
Performance (P_1)	.99 \pm .003	.99 \pm .003
Discrepancy between estimate of past performance and the actual performance (EP_1-P_1)	.49 \pm .11	.71 \pm .07
Discrepancy between estimate of future performance and actual past performance (EP_2-P_1)	.86 \pm .04	.95 \pm .02
Discrepancy between estimate of future performance and actual future performance (EP_2-P_2)	.86 \pm .04	.69 \pm .07

coefficients are satisfactory for preliminary analysis of the intercorrelations between the measures.⁶

The interrelations between the performances on the two tasks are reported in Table II. The lack of correlation between performance scores,

TABLE II
INTERCORRELATIONS OF MEASURES BETWEEN THE TWO TASKS

	Correlation: card sorting and pursuit learning (r and σ_r)	
	Obtained	Corrected
Performance scores (P_1)	.12 \pm .14	.12 \pm .14
Discrepancy scores based on estimates of past performance (EP_1-P_1)	.46 \pm .11	.78 \pm .06
Discrepancy scores between estimates of future performance and past performance (EP_2-P_1)	.66 \pm .08	.73 \pm .07
Discrepancy scores between estimates of future performance and future performance (EP_2-P_2)	.60 \pm .09	.78 \pm .06

in spite of their high reliabilities, is important, since it makes all the more significant the relationships found between the discrepancy scores.⁷ There

⁶ Since there is some correlation between the size of the discrepancy score and the actual performance, it appeared that the reliability coefficients of the discrepancy scores might be spuriously high because of their dependence on performance. However, when the effect of performance was reduced by expressing the discrepancy scores as ratios of performance, reliabilities calculated from these ratio scores were equal to or higher than those given in the table.

⁷ Because card sorting was scored in seconds, yielding a falling performance curve, and pursuit learning in points, yielding a rising curve, it might be supposed that the intercorrelations would be reduced by these differences in units of measurement. When the scores were converted to a similar basis by using reciprocals of the card sorting scores, a similar insignificant correlation ($r = +0.11$) was found, so that

is an appreciable correlation between the discrepancy scores, whether estimates are of past or of future performances. These correlations agree with those commonly reported in similar experiments, showing a higher consistency in the distortions of self-estimates than in the performance scores on several tasks. From these relationships it is evident that the discrepancy scores are in this situation more closely related to personality characteristics

TABLE III
RELATIONSHIP BETWEEN DISCREPANCY SCORES BASED ON ESTIMATES OF PAST PERFORMANCE
AND ON ESTIMATES OF FUTURE PERFORMANCE

Task	Relationship	Correlation (r and σ_r)	
		Obtained	Corrected
Card sorting	$(EP_1 - P_1)$ vs. $(EP_2 - P_1)$	$.29^* \pm .13$	$.45 \pm .11$
	$(EP_1 - P_1)$ vs. $(EP_2 - P_2)$	$.45^\dagger \pm .11$	$.69 \pm .07$
Pursuit learning	$(EP_1 - P_1)$ vs. $(EP_2 - P_1)$	$.77 \pm .06$	$.94 \pm .02$
	$(EP_1 - P_1)$ vs. $(EP_2 - P_2)$	$.75 \pm .06$	$1.07 \pm -$

* The score for a single subject greatly distorts this coefficient. Omitting this one score raises the obtained correlation to $0.55 \pm .10$; corrected, $0.78 \pm .06$.

† Omitting the score for the one discordant case raises the obtained correlation to $0.62 \pm .09$; corrected, $.92 \pm .02$.

than to present motor skill, although the data are too fragmentary to determine what the influence of past experience with motor skills has been.

What the data of Table II add to the usual findings in level of aspiration experiments is that discrepancy scores based on estimates of past performance are related in a manner corresponding to the relationship found between discrepancy scores based on future performances. If this turns out to be more generally substantiated, the interpretation of a stated level of aspiration as the expression of a momentary future goal would appear too limited. Further evidence appears in Table III, which gives the relationships between the discrepancy scores based on past and on future performances within each of the tasks. Since the correlations are significant and positive, it appears that discrepancies of self-estimates based on past performances ($EP_1 - P_1$) depart from the group tendency in about the same direction and to similar proportionate amounts as estimates based on performances not yet completed ($EP_2 - P_2$).

While the distortions correlated significantly, it does not follow that the mean amount of distortion is similar in the two estimates, since all subjects are influenced by certain common considerations, such as expected improvement in a learning task. The actual discrepancies are summarized in Table

the differences in scoring are probably not responsible for the low intercorrelations between performance scores on the two tasks.

IV. Except for the discrepancy score based on the estimate of past performance in card sorting, each mean departs significantly from zero. The following interpretations may be suggested.

In card sorting, relatively realistic judgments of past performance, erring unreliably on the side of underestimation, are replaced by overestimates of

TABLE IV
DISCREPANCIES OF ESTIMATES OF PAST AND OF FUTURE PERFORMANCES FROM ACTUAL PERFORMANCE SCORES

A positive sign indicates overestimation (*i.e.* overconfidence), and a negative sign underestimation. The signs have the same meaning, therefore, whether scores are in points or in seconds.

Relationship	Mean and σ_m	
	Card sorting (seconds)	Pursuit learning (points)
Actual performance (P_1)*	50.6 ± 1.3	347 ± 11
Discrepancy between estimate of past performance and actual performance ($EP_1 - P_1$)	-0.4 ± 0.2	-11 ± 1
Discrepancy between estimate of future performance and past performance ($EP_2 - P_1$)	$+2.5 \pm 0.3$	$+7 \pm 2$
Discrepancy between estimate of future performance and future performance ($EP_2 - P_2$)	$+1.6 \pm 0.3$	-9 ± 2

* The means refer to the average score per trial per S. Performance measures as stated are for trials 2 to 25.

future performance, as indicated by the positive signs for both ($EP_2 - P_1$) and ($EP_2 - P_2$). This mean result is probably due to the expectation of gains in card sorting similar to those in other laboratory learning tasks, whereas the actual gains are slight, the mean gain per trial ($P_2 - P_1$) being only 0.9 sec.

In pursuit learning the original underestimation of past performance is continued in the estimation of future performance. The positive sign for ($EP_2 - P_1$) means that gains are anticipated, but the negative sign for ($EP_2 - P_2$) means that the gains tend to be larger than expected. This finding, contrary to that for card sorting, is probably due to the rapid and continued improvement in pursuit scores, *i.e.* to gains which turn out to be unexpectedly great. The mean gain per trial ($P_2 - P_1$) is 16 points. It is of interest that these average trends, showing overestimation in card sorting and underestimation in pursuit learning, do not greatly affect the individual differences which persist through both tasks, as shown by the correlations presented earlier in Table III.

Distortion. While subjective factors, describable as persisting idiosyncrasies, are important in determining the individual differences in level of aspiration experiments, there are many features within the experiments themselves which render objective estimates unlikely. Disinterested ob-

jective estimates of future performance, such as could be made by a third person aware of past scores, conceivably might be based upon (1) the last announced score, (2) a previous best score, (3) previously experienced increases in score, (4) previously experienced decreases in score, and (5) preconceived notions of the course of learning (accounting for overestimation in card sorting and underestimation in pursuit learning). A guess which depends on widely fluctuating earlier scores, even though it is realistic, must somehow achieve a synthesis of these factors. Because of the range of variation which must be allowed even for realistic estimates, subjective distortions based on personality characteristics may be introduced without Ss awareness that his interpretations of the probabilities of the situation are unusual.⁸ Are distortions of estimates of a performance already completed to be explained on the same basis? An affirmative answer can probably be justified because an estimate of past performance is based on all of the above-mentioned factors with the single exception of the last announced score (which, in the case of an estimate of past performance, would have to be replaced by knowledge of earlier scores). The only added information available in the estimate of past performance is the experience of the trial itself. When there is something unusual about a single trial, such as momentary confusion, clumsiness, or blocking, this may enter into the estimate of what score has been made or how long the trial took. Such fortuitous occurrences cannot be allowed for accurately in estimates of future performance. So long as there is some uncertainty, subjective preferences will lead to distortions in the direction of over- or underestimation, whether in reference to a performance completed or one yet to be made.

SUMMARY

Estimates of past and of future scores were made by 50 college students participating singly in experiments on card sorting and on pursuit learning. Discrepancy scores, whether based on estimates of past or of future performances, showed significant positive correlations within and between tasks. The positive correlations for discrepancy scores between tasks were obtained in spite of the dissimilarity of tasks as shown by the lack of correlation between the raw performance scores. The similarity of discrepancies when estimates referred to completed performances and when they referred to future performances suggests that the subjective distortion is similar whenever there is uncertainty in estimating the score on a realistic basis.

⁸ McGehee has recently reported the relative accuracy of 'bidding' with respect to what one will do, and 'estimating' what another person will do (W. McGehee, Judgment and the level of aspiration, *J. Gen. Psychol.*, 22, 1940, 3-23). While 16 of 26 Ss estimated more accurately than they did, 10 bid more accurately than they estimated. McGehee concludes that "behavior involved in erection of and subsequent relationship to the level of aspiration is psychologically different from that involved in the activity of judging."

MINIMAL AUDITORY STIMULI DURING THE ONSET OF SLEEP

By MARION R. BARTLETT, Wagner College

In this study we investigate the changes in the auditory threshold which occur during the *onset* of sleep. Earlier investigators have largely concerned themselves with changes occurring *during* sleep and a considerable amount of work, from the physiological as well as the psychological points of view, has been done on the problem.¹

A few investigators have, however, concerned themselves with changes of sensibility and reflex irritability directly preceding sleep. Miller, for example, found that shock was not so unpleasant to Ss who were relaxed, even though they were not asleep.² Bass made a study of the decrease in correct responses to an auditory stimulus of constant intensity during the process of going to sleep and found a decrease from 90% to 15%.³ Reflex irritability was shown to be decreased in the study of Jacobson and Carlson, who were able to demonstrate that the knee-jerk could be abolished through deep relaxation, even though the S was not quite asleep.⁴

In our present study we used 3 men (A, C and D) and 1 woman (B) as Ss. All were trained psychologists, each holding a doctorate in that field. They were selected on the basis of their ability to go to sleep readily in the sitting position. The experiments were performed when S thought it would be easy for him to go to sleep. Because no adequate objective criterion of sleep has as yet been formulated, we used S's reports of his condition, as our criterion.

Technique of measurement. As a sound source, we used a General Electric audio-oscillator of 1000 \sim in circuit with a 6-v. battery. The current was attenuated by means of a continuously variable potentiometer of 10,000 Ω resistance and by a stepwise shunt across the phone of 45 Ω total resistance. This allowed for a considerable range of intensity from moderately loud to threshold-level. Intensities were measured in terms of microamperes and then converted mathematically into decibels, with the zero point at $1/10^{-10}$, so that results could be expressed in psychological terms.⁵ Photographs made of the sound stimuli from this source by a cathode ray oscillograph showed no surging when the switch was turned on.

* Accepted for publication September 6, 1940.

¹E. Kohlshutter, Messungen der Festigkeit des Schlafes, *Zeit. f. rationelle Medizin*, 17, 1862, 209-253; O. Monninghof and F. Piesbergen, Messungen über die Tiefe des Schlafes, *Zsch. f. Biol.*, 19, 1883, 114-128; A. Czerny, Zur Kenntniss des physiologischen Schlafes, *ibid.*, 41, 1896, 337-342; S. DeSanctis and U. Neyroz, Experimental investigations concerning the depth of sleep, (Tr. by H. C. Warren) *Psychol. Rev.*, 9, 1902, 254-282; Henri Pieron, *Le probleme physiologique du sommeil*, 1913, 550; N. Kleitman, Sleep, *Physiol. Rev.*, 9, 1929, 624-665.

²Margaret Miller, Changes in the response to electric shock by varying muscular conditions, *J. Exper. Psychol.*, 9, 1926, 26-44.

³M. J. Bass, Differentiation of the hypnotic trance from normal sleep, *ibid.*, 14, 1931, 382-399.

⁴E. Jacobson and A. J. Carlson, The influence of relaxation on the knee-jerk, *Amer. J. Physiol.*, 73, 1925, 324-328.

⁵E. E. Free, Noise measurement, *Rev. Sci. Instr.*, 4, 1933, 368-372.

Our experiments were performed in an inner room without windows. It was relatively but not entirely soundproof. Since we desired to avoid disturbance of occasional sounds, we used an electrical fan as a noise screen. This had an over-all intensity of 46 db., and a specific intensity at our frequency of 41 db.⁶ The Ss were allowed 5 min. to become adapted to this constant noise level before starting the experiments.⁷

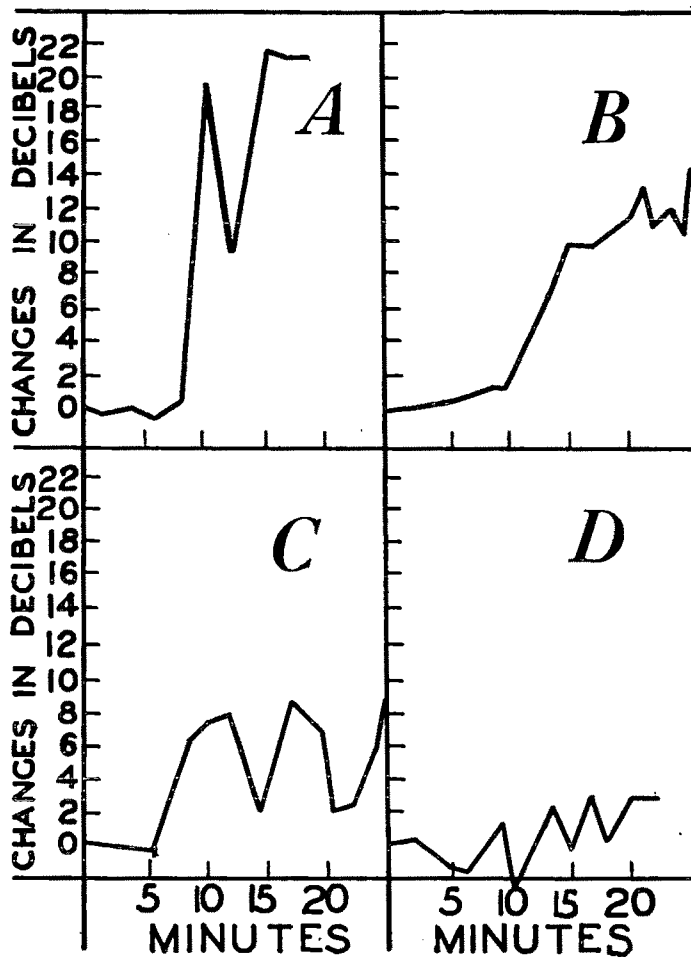


FIG. 1. INDIVIDUAL CURVES OF MINIMAL STIMULI PRECEDING SLEEP

⁶ We are indebted to Dr. E. E. Free for these measurements of our noise screen.

⁷ In an experiment of E. G. Wever and S. R. Truman (The course of the auditory threshold in the presence of a tonal background, *J. Exper. Psychol.*, 11, 1928, 98-112), it was shown that in adaptation to a tonal background a constant level was reached at the end of 2 min.

E gave the stimuli by closing a noiseless knife switch in a screened booth at irregular intervals. The tone was carried to *S* by means of an ear-phone of 300 Ω resistance. *S* responded by pressing a telegraph key placed on the right arm of his chair, thereby lighting a signal light on *E*'s table. *S* was instructed to press the key every time he heard the stimulus, but not to let the task keep him awake. Liminal determinations were made at approximately 2 min. intervals.

In ascertaining the response values we used only the ascending series of the method of limits. This was deemed necessary in view of the nature of the experiment which demanded that *S* be aroused as little as possible. Since all of the results were taken

TABLE I
MINIMAL AUDITORY STIMULI DURING THE ONSET OF SLEEP

A		B		C		D	
Time (P.M.)	Decibels	Time (A.M.)	Decibels	Time (P.M.)	Decibels	Time (P.M.)	Decibels
11.30	44.95	11.50	45.50	7.50	31.50	4.34	66.25
11.32	44.90	11.52	45.55	7.55	31.35	4.36	66.50
11.34	45.00	11.55	46.00	7.57	35.25	4.38	65.25
11.36	44.50	11.57	46.45	7.58	37.60	4.40	65.70
11.38	45.20	(moved)		8.00	38.70	4.42	68.00
11.40	64.00	11.59	46.70	8.02	39.40	(moved)	
11.42	54.55	12.00	46.75	(moved)		4.44	63.70
11.44	62.25	(talked)		8.04	33.50	4.45	65.00
11.45	66.50	12.03	51.95	8.05	35.35	4.47	68.35
11.46	66.00	12.05	55.20	8.07	40.00	(moved)	
11.48	66.00	(moved)		(moved)		4.49	65.90
		12.07	55.20	8.09	38.25	4.51	69.00
		12.09	56.00	8.10	33.50	(moved)	
		12.10	57.00	8.12	33.70	4.52	66.25
		(moved)		8.14	36.90	(moved)	
		12.11	58.80	8.16	40.00	4.54	69.00
		12.12	56.30			4.56	69.00
		12.14	57.40				
		(moved)					
		12.16	56.00				
		12.18	57.40				
		12.19	58.10				

in the same manner, it seemed probable that they should be entirely comparable one with another, although obviously there would be a *constant* error of a uni-directional nature.

Results. The results obtained from the experiment are shown in Table I in which the minimal stimuli preceding sleep are given, and in Fig. 1 in which the curves for these values are plotted. Observation of these curves would indicate that in the case of the *Ss* used in this experiment there are no typical curves, but rather are there individual ones. In one respect only are the curves in agreement, *i.e.* there is a considerable amount of rise in the threshold value from the starting point to that after which *S* aroused himself and reported that he had slept. This is the obvious result if loss of sensibility occurs in sleep. In other respects the curves differ markedly both as to slope, to amount of change, and as to variability from reading to reading.

A brief discussion of the individual Ss in respect to their curves may be helpful in interpretation. A was by far the best 'sleeper' of the group, being able to go to sleep at almost any time, *i.e.* in seminars, lectures, etc. In view of this, it is interesting to note that his auditory threshold rises sharply 10 min. after the beginning of the experiment. There was a secondary slump in the curve with a rapid subsequent rise which was maintained until he awakened.

B was of a rather placid nature, and although she aroused herself to speak aloud on one occasion during the experiment she seemed to have little difficulty in carrying out the directions of going to sleep. Her curve shows a rapid rise, although not as rapid as that of A, nor does it go so high.

C reported subsequently to awakening that he had been aware of the sound right at the start, then thought that 'he went off and suddenly came to with a start.' He reported that he 'came to' several times during the experiment and was not aware that he had been continuously responding. It is interesting in connection with his retrospective report to observe that on three occasions his curve reaches practically the same high level, and on the first two occasions temporarily comes down.

D was the most restless of our Ss and at the end of the experiment he reported that he had not fallen into a deep sleep but had dozed. His curve shows a considerable amount of fluctuation from reading to reading, and but a comparatively small rise of level of audition from the start to the end of the experiment.

From these results it would appear that those cases who found it most easy to go into a sleep-state showed a rapid rise in the curve of audition while going to sleep; those cases who found the act of going to sleep more difficult, as evidenced by restlessness, and, by subsequent report, showed fluctuations in the threshold of audition during the experiment. These results are in accord with a recent finding of Mullin, Kleitman, and Cooperman, who found the depth of sleep to be lighter immediately after a movement and then to grow deeper, thereby giving many curves of the 'depth of sleep.'⁸ They are also in accord with the results of Johnson and Swan who found that actograms of curves of sleep showed much individual difference.⁹

Summary. A study of the curves of auditory sensitivity during the onset of sleep indicated the following: (1) The only typical change in all of the curves of audition was a rise from the initial readings to the final ones. (2) The amount of the rise differed widely among the Ss. (3) There was a marked individual difference in the *slope* of the curves of audition for the Ss used.

⁸ F. J. Mullin, N. Kleitman, and N. R. Cooperman, Studies on the physiology of sleep changes in irritability to auditory stimuli during sleep, *J. Exper. Psychol.*, 21, 1937, 95.

⁹ H. M. Johnson and T. H. Swan, Sleep, *Psychol. Bull.*, 27, 1930, 1-39.

EYE MOVEMENTS IN READING A MODERN TYPE FACE AND OLD ENGLISH

By MILES A. TINKER and DONALD G. PATERSON, University of Minnesota

In five determinations of the difference in speed of reading Old English (Cloister Black) in comparison with a modern type face (Scotch Roman) we found a differential effect in favor of the Scotch Roman ranging between 11.6 and 14.2%.¹ The consistency of this striking difference naturally suggests the desirability of searching for the specific differences in the eye-movement patterns underlying this retardation.

The eye movements of each of 20 college students were photographed while reading 10 paragraphs from the Chapman-Cook Speed of Reading Test, Form A, set in Cloister Black and 10 different paragraphs from Form B of the same test set in

TABLE I
MEAN EYE-MOVEMENT MEASURES FOR 20 COLLEGE STUDENTS READING TEXT SET IN
CLOISTER BLACK AND IN SCOTCH ROMAN

Pause-duration and perception-time are reported in seconds. Ten paragraphs from the Chapman-Cook Speed of Reading Test, Form A, were set in Cloister Black and 10 paragraphs from Form B of the same test were set in Scotch Roman. All paragraphs were printed as follows: lower case, 10 point, 19 pica line-width, set solid, on egg-shell paper stock.

Type form	Fixation- frequency	Words per fixation	Pause- duration	Perception- time	Regression- frequency
Scotch Roman	210.1	1.5	0.239	50.4	28.2
Cloister Black	220.6	1.4	0.242	53.6	31.2
Diff.*	+10.5	-0.1	+0.003	+3.2	+3.0
% diff.	+5.0	-5.3	+1.3	+6.4	+10.7

* The t values are beyond the 5% level except for regression frequency (between 5% and 10% level) and for average pause-duration (at 30% level).

Scotch Roman. Practice effects were equated by systematic variation of test-forms and Ss.

The results, presented in Table 1, show that the eye-movement patterns are more efficient in reading Scotch Roman than in reading Cloister Black. The detailed findings show that the reading of Cloister Black resulted in a 5.0% increase in the number of fixations and a 5.3% decrease in the number of words per fixation. The average duration of the fixational pauses, however, was only slightly increased (1.3%). Perception-time was lengthened by 6.4% and there was a 10.7% increase in the number of regressions.

None of these differences are established statistically with a probability beyond the .1% level. Fixation-frequency, words per fixation and perception-time yield differences with t values beyond the 5% level. Regression frequency and pause duration involve differences still less significant. Nevertheless, it is important to note

* Accepted for publication June 21, 1940. The writers are grateful to the University of Minnesota Graduate School for research grant to finance this study.

¹ Detailed results of these and other studies have been published in book form, under the title, *How to Make Type Readable*, 1940.

that all of the differences are in the direction of decreased efficiency in reading Cloister Black.

We interpret the obtained differences to indicate the difficulties experienced by our readers in grasping word and phrase units. This forces the reader to a discrimination of details which ordinarily would be at a minimum. As a matter of fact, inspection of the printed copy reveals that characteristic word forms are distorted in Cloister Black printing. In addition, many of the letters are difficult to discriminate.

It is somewhat surprising to discover that the reading of Cloister Black, which is read at a strikingly slower rate than Scotch Roman (performance tests), should not produce an equally striking difference in the analytical eye-movement records. This discrepancy confirms our belief that eye-movement photography is not an adequate method for measuring the efficiency of one typographical arrangement as against another. For this reason, we would emphasize our prior findings indicating that Cloister Black retards speed of reading by about 12%. The eye-movement records, on the other hand, are of value in indicating that the chief difficulties in reading Cloister Black are confined to reduction in span of perception with a consequent increase in number of fixations, perception-time, and regressions. Of course, it is evident that both methods are in agreement in showing that Cloister Black is read more slowly than Scotch Roman.

SUMMARY

(1) Performance reading tests have uniformly shown that Old English is read about 12% more slowly than ordinary type face.

(2) Eye-movement photographs of a new group of 20 Ss indicate that the reading of Old English tends to reduce the span of perception, to increase the number of fixations, total perception-time, and the number of regressive movements. There is a suggestion that pause duration is also slightly increased.

(3) It is suggested that the difficulty encountered in reading Old English type is due to the necessity for discriminating details in the perception of word and phrase units.

APPARATUS

A CLASSROOM DEMONSTRATION OF THE CONDITIONED RESPONSE

By R. H. HENNEMAN and J. G. TALLEY, College of William and Mary

It is usual in laboratory demonstrations of the conditioned response to employ a manual reaction of avoidance to shock, using the Watson finger-withdrawal apparatus.¹ Because of marked individual differences in response to shocks applied to the fingers, satisfactory records are difficult to obtain. Furthermore, since the withdrawal response is subject to voluntary control, the attitude of an *S* may strongly influence the process of conditioning. Again, strong shocks must be applied in order to evoke the avoidance responses. After experimenting with several modifications of the Watson apparatus without much success, the writers devised a set-up which obviates most of the difficulties commonly encountered.

The set-up we use consists of three parts: (1) a device for delivering shock stimuli to *S*'s hand and receiving his responses; (2) an automatic interval-timer for controlling the presentation of the stimuli; and (3) a kymographic recording system. The manual unit, Part I, is made of a rubber hand-bulb connected by tubing to a kymographic-recording tambour. On opposite sides of the bulb are taped electrodes for delivering shocks from an inductorium. *S* grasps the bulb, maintaining a mild, continuous pressure. Shocks are received on palm and finger or on thumb and little finger. Experimentation with the apparatus has indicated three kinds of responses which may follow the shock: a sudden increase of pressure on the bulb; a sudden relaxation of pressure; a muscular tremor lasting for the duration of the shock. All three of these responses are clearly differentiated on the records from normal variations in pressure by *S*'s hand on the bulb during absence of stimulation. The continuous-feed, ink-recording kymograph indicates the duration of the conditioned stimulus (buzzer), the duration of the shock stimulus and *S*'s manual response to either or both stimuli. Both amplitude and duration of these responses, as well as their time relations to the stimuli, show up clearly on the records. As the process of conditioning sets in, the anticipatory nature of the conditioned response is well demonstrated.

The writers believe this apparatus to have several advantages: (a) Shock stimuli consistently evoke a pressure change on the hand-bulb which records clearly. (b) With the sensitivity provided by this recording system, relatively weak shocks produce adequate responses for measurement. (c) A blindfolded *S* is usually unaware of his manual reaction to the conditioned stimulus, hence attitude factors are lessened. (d) Presentation of stimuli and recording of responses are automatic. For demonstrations before large classes small lights may be substituted for the kymographic recording device.

¹ N. L. Munn, *A Laboratory Manual in General Experimental Psychology*, 1938, 93-95; J. P. Guilford, *Laboratory Studies in Psychology*, 1934, 103-108.

NOTES AND DISCUSSIONS

NOTES ON THE CONCEPT OF MENTAL DEFICIENCY

The recent literature shows an increased interest in the subject of mental deficiency as a profitable field of psychological exploration. These contributions are not confined to social welfare, but reach into the deeper problems of experimental and theoretical psychology. They are logical extensions of early work in the fields of genetic, social, and abnormal psychology which reflect an extensive interest in fact and theory as well as useful application.

In evaluating these contributions one immediately senses a confusion of stand-points in concept and definition which embarrasses the interpretation of data. The oversimplified mental-age, *IQ*, and statistical definitions of mental deficiency disturb those working more deeply in this field in close contact with the subject material. It may be something less than presumptuous to offer the following notes on this condition in the interest of clarifying the different points of view and of harmonizing the conflicting results already reported and likely in the near future to be materially augmented.

It should be clear that the scientific work on the feeble-minded must be viewed with reference to the selective influences of form, grade, and type of subject. The weighting of subjects according to one or another of these categories will materially influence the results obtained. This is clearly reflected, for example, in such careful work as Kreezer's several studies on the electro-encephalogram with feeble-minded subjects which reveal markedly different results according to their mental age, clinical type, and etiology.

General concept. Mental deficiency (feeble-mindedness, amentia, oligophrenia, mental defectiveness) is typically defined as a condition such that the person affected is (a) socially insufficient, because of (b) subnormal intelligence, (c) existing from an early age. Whatever else might be added to the description, these three elements are essential. This is the traditional concept for the standard literature and is the essential basis of most legal definitions. Even when one or another element of this definition is omitted, or when other considerations are added, these three criteria remain as indispensable. Departures from this point of view have become increasingly apparent in the recent literature, the consequences of which gravely confuse the unwary student.

There are many types and degrees of social incompetence of which mental deficiency is but one. Our society includes many persons of low estate who are indolent, illiterate, criminal, unskilled, frustrated, impoverished. To these we may add other socially handicapped persons such as the infirm, crippled, blind, deaf, and the aged. Those who are socially dependent in these groups owe their estate chiefly to physical or social circumstances or to mental attitudes which fall outside the field of feeble-mindedness because such social inadequacy is due to causes other than developmental deficiency of intelligence.

We omit children from this consideration on the ground that the child is presumed to be dependent until he reaches maturity, and this fact handicaps the recognition of mental deficiency in children during the period of genetic maturation.

Accordingly, the criterion of social competence which is essential to the recognition of feeble-mindedness in children must be thought of in different terms than those which apply to adults.

Another class of the socially dependent are incompetent because of mental infirmities. Among these we find the constitutionally defective, the psychopathic, psychoneurotic, psychotic, neuropsychiatric, and convulsive (epileptic) states commonly recognized as mental disease which so gravely impair an originally normal mental development as to produce functional inadequacy. These individuals are socially insufficient because of mental disturbances arising after the period of maturation and which generally produce a reduction of social competence from the level formerly exercised.

Finally there are the feeble-minded, who are socially inadequate *and* mentally inadequate, but whose mental and consequent social inadequacy are due to imperfect bio-social maturation rather than to age, social circumstance, or mental or physical infirmity. Among these the condition of social insufficiency has existed from birth or from an early age and there is little prospect that they will ever reach the adult level of normal human attainment. Their development is therefore said to be arrested or incomplete. Some of them may have developed within average limits during early childhood, but an arrest of development, whether gradual or sudden, has taken place prior to attaining the normal adult standard.

In short, we observe that the socially insufficient person may owe his inadequacy to (a) tender age, (b) social circumstance, (c) physical handicap, (d) mental disorder or deterioration from a once normal state, or (e) subnormal development of intelligence. It is this last group, and only this last, that we technically recognize as feeble-minded. Social insufficiency is the symptom and developmental deficiency the cause; *both* are essential to a sound concept.

This distinction is simple enough when applied to adults. In the case of the feeble-minded child, the concept of social sufficiency must be expressed according to developmental standards corresponding to his age rather than to adult standards. This presents a special difficulty since we have to discriminate between maturity and immaturity as well as between opportunity and disability. Indeed, the differentiation of the feeble-minded child from the normal child is difficult on all counts, since the diagnosis is based on a presumption or a prediction as to what his condition will be at maturity. Feeble-mindedness as a form of incompetence during the developmental period is to some degree a paradox, since all children are to a degree dependent according to their age-relation to the developmental cycle. Maturation retardation during the developmental period is therefore an uncertain criterion of mental deficiency except as it is assumed that such retardation will be permanent rather than subsequently overcome. There are many forms of developmental subnormality which are overcome with age or by treatment, and this materially complicates the diagnosis of mental deficiency during infancy, childhood, and early youth. This gives additional pause to the tendency to diagnose mental deficiency on the basis of a single criterion which may reflect only a transitory retardation. Since one criterion of feeble-mindedness is its essential incurability, errors of detection during the developmental period lead to classifications which can only be recognized as pseudo-feeble-mindedness.

Aside from the vagaries of environmental misfortunes, physical anomalies, physiological deficiencies, sensory handicaps, ill health, special disabilities, emotional

disorders, and the like which produce conditions simulating feeble-mindedness in children, we must recognize a definite group of children of constitutionally slow maturation or delayed development. These latter individuals may mature relatively late, but they do ultimately reach the normal standard. Conspicuous among these, for example, are the mentally normal birth-injured, and a not inconsiderable group of other mental, physical, and social idiosyncrasies.

On the other hand, feeble-mindedness may occur during the developmental period after some part of that period during which the child has developed within average limits of maturation. Among these we find the hereditary feeble-minded of low developmental potential, whose arrest of development may not set in until relatively late in childhood. We also observe those who through some accident, disease, or other mischance experience a frustration of an essentially normal hereditary endowment. Arrested development as a precondition of mental deficiency may therefore date from early pregnancy to early adolescence, and may be gradual or sudden, or even accompanied by deterioration as in the case of epilepsy combined with feeble-mindedness or in such deteriorating diseases as epidemic encephalitis. From the clinical point of view, therefore, the normal constancy of development may be interfered with as a result of limited endowment, intra-uterine mishap, complications of birth and the neonatal period, trauma, accident, disease, or deficiency acquired prior to normal maturity.

From whatever point of view feeble-mindedness is considered, we repeat that the essential outcome is a condition of social insufficiency which is the result of sub-normal evolution of intelligence during the maturational period. With this conclusion clearly in mind, it will be evident that no single criterion will suffice for diagnosis, and this will avoid confusing mental deficiency with other conditions which it simulates or approximates. In the more comprehensive evaluation of this condition; in the formulation of standard definitions; in the classification by degree, type, and cause; and in the implications regarding outcome, disposition, treatment and control, we are greatly assisted by other lines of evidence, but we must be careful not to substitute these for the basic standard.

Criteria: (a) Social. We observe that the criterion of social insufficiency is fundamental to the concept of mental deficiency. We may now examine how this concept is to be concretely formulated. We note at once that this insufficiency is a fundamental incompetence rather than an unwillingness. It has been variously described as incapacity to manage one's self and one's affairs with ordinary prudence, or inability to get along without the aid and supervision of others, or inaptitude for self-direction and self-support, or inability to profit from instruction in such matters. This social inadequacy of the feeble-minded is therefore reflected in such directions as conduct, education, occupation, foresight, economic independence, self-direction, self-interests, social participation. In these regards the feeble-minded seldom come within a comprehensive standard of adult proficiency and responsibility. Their social activities are typically characterized by indiscretion, poor management, limited imagination, poor judgment, whether these be expressed in terms of conventional conduct, ordinary literacy, wage-earning capacity, or the more severe expectation of contributing to the general welfare. The individual feeble-minded adult may approximate the norm in respect to one or another of these requirements of social desirability under favorable circumstances or for a limited period of time or after long-

standing habituation, but his social success is at best temporary, marginal and precarious.

In these respects the feeble-minded differ from the social ne'er-do-wells previously mentioned whose social insufficiency is the result of limited motivation rather than essential incompetence. They differ also from the social dependency of the physically handicapped and the mentally deteriorated for whom there is always some prospect of normal attainment or rehabilitation if the limiting handicap can be overcome. In the feeble-minded the handicap is a generalized incompetence which is reflected in all forms of self-expression and not in social-economic success alone.

In the case of children below the level of normal adult maturity, the criterion of social incompetence is more difficult to define. During the developmental period, therefore, the child suspected of mental deficiency must be compared with others of his own age and the standard of his attainment is consequently one of more or less in terms of the successive stages of maturation. Within the developmental period, feeble-mindedness is judged in social terms by the extent to which the child has presumably fallen so far below the level of attainment expected of one of his age that there is no hope of recovery from treatment, instruction, or as a consequence of delayed mental growth. The successive ages of childhood are characterized by successive stages of personal independence and social responsibility even though the child as a whole remains socially dependent.

Indeed, these successive stages of personal social maturation give rise to the social definitions of feeble-mindedness in terms of its subgrades. Thus, the idiot never outgrows the period of infancy in which the normal child is incapable of practicable speech, or taking care of his ordinary wants, or protecting himself from ordinary dangers. The imbecile, outgrowing the period of infancy even though at a later age than normal, succeeds in those respects in which the idiot fails, but makes no progress under academic instruction at school, learns only the simplest repetitive tasks, and requires continuous supervision of his daily activities. The moron accomplishes the imbecile limits of competence, may master the rudiments of academic instruction under persistent teaching, may master a number of unskilled industrial pursuits, but does not succeed at the apprentice level of occupational pursuits, does not attain to consistent good judgment, does not attain to independent self-support, and does not master competent direction of himself and his general affairs.

Until recently the social standard of mental deficiency has been but vaguely formulated and has not been subject to numerical appraisal. This lack of a standardized criterion susceptible to measurement is a major reason why the social criterion has not received adequate attention. The successful measurement of intelligence by means of the Binet-Simon scale and other mental tests has consequently supplanted the social criterion. This is an outgrowth of the current demand for more precise detection of mental deficiency but has paradoxically led to confusion because the intellectual criterion when employed alone leads to much uncertainty, especially since the intellectual criterion requires the social criterion as a prerequisite to its own differential standards.

The development of the Vineland Social Maturity Scale helps to overcome this difficulty. This scale provides a clear definition of social competence and a fairly adequate method for its quantitative measurement. This scale supplies a social criterion which separates the feeble-minded from the normal much more explicitly than does the Binet-Simon or any other intelligence test. It also supplies differentiat-

ing standards for idiot, imbecile, and moron in terms of their social definitions. Although this scale is a comparatively recent product and has not yet come into very wide-spread use for this purpose, a number of experimental studies indicate its value for such use. Such quantitative estimation of the social criterion will almost inevitably lead to reappraisal of other devices and will probably return the diagnosis of mental deficiency to its indispensable social foundations and relegate the intellectual criterion to its necessary but secondary relation.

The social-age scores of the feeble-minded as measured by means of this scale rarely rise above eighteen years; if they do there is a reason to question the certainty of the diagnosis. Normal adults, on the other hand, seldom score below the social age of twenty years. This scale therefore provides practically total mutual exclusion between the upper limits of feeble-mindedness and the lower limits of normality, a distinction which cannot be accorded to any other present single measuring instrument of the clinical syllabus.

In the case of children, social quotients by this scale which fall below seventy (uncomplicated by specific handicaps) suggest strong suspicion of mental deficiency, while those above eighty give a presumption of normality. As noted previously, however, diagnosis of deficiency in children during the developmental period is uncertain because of the difficulties of prediction as a result of which social quotient scores may rise or fall in individual cases during the developmental period.

Similarly, this social scale affords a more satisfactory separation of idiot, imbecile, and moron than is possible from other measurement devices. The upper limit of idiocy is at social age three years, and this is also the lower limit of imbecility without appreciable overlapping. In the case of imbeciles, the upper limit is at social age nine years, which is also the lower limit of moronity without overlapping. As previously noted, the upper limit of moronity is at eighteen years.

These social-age limits are practically self-evident from the nature of the scale, but have also been verified by consideration of scores of large numbers of individual cases. In the case of children the quotient scores for idiocy, imbecility, and moronity cannot be accurately fixed during the developmental period until development is substantially complete because of the uncertainties of prediction as noted previously.

In using the social scale for this purpose allowance must be made for the influence of special handicaps other than arrested development of intelligence which might modify the interpretation of scores. The above standards assume the absence of such handicaps. In other words, the limits of social competence must be due to lack of intelligence existing from an early age rather than to other disabilities.

(b) *Mental.* Although the primary manifestation of mental deficiency is social incompetence, the cause of this incompetence is mental subnormality. This is implicit in the terms *feeble-mindedness* and *mental* deficiency. Before Binet and Simon constructed their measuring scale for intelligence, the attempts to describe and measure this mental deficiency had not been successful. While recognizing the generalized nature of mental retardation for the feeble-minded, Binet and Simon found it difficult to isolate and classify the feeble-minded in terms of the old mental faculties. They overcame this dilemma by observing how intelligence is modified by progressive maturation with age, the lack of which was the essential ear-mark of the feeble-minded. They argued that in intelligence there was a fundamental faculty, the lack or alteration of which was the essential mental factor in mental deficiency. This central factor they identified with common sense, reasoning, rational comprehension,

and judgment. These constituted for them the mental power or mental level of the individual in respect to which other mental processes were correlative and contributory. The clearest psychological description of mental deficiency is still to be found in their writings relative to the evolution of their scale for measuring intelligence.

In this scale they introduced the method of mental-age or year-scale measurement which has since proved to be such an important practical development in psychology for the classification of individual differences. That scale yielded such a satisfactory method of measurement and grading that it came to be substituted for the social criterion. The concept of mental level as expressed in mental age further became confused with the concept of brightness as expressed in mental ratios, or *IQ*, and these with extreme statistical deviation on the normal curve of distribution, so that today in spite of the inadequacy of the mental criterion alone it is widely used as the sole or pathognomonic test of the condition. This practice is so satisfyingly simple, and the use of the clinical syllabus so technically difficult, that the former has practically replaced the latter among those not intimately familiar with the problems at issue. Even among those who rationally deplore this situation, there are many who nevertheless condone it in practice because of the time and skill required for adequate clinical appraisal.

This state of affairs is aggravated by the failure of psychology successfully to measure the more or less specific mental processes, functions, or faculties which constitute the principal chapters of standard textbooks in psychology. Few of these mental processes have been reduced to measurement norms in terms of genetic maturation and this essential principle so successfully worked out in the measurement of intelligence has yet to be exploited for genetic psychology as a whole. For the same reason the general psychology of the feeble-minded has not yet been experimentally worked out. Since that psychology must be comparative and genetic, it is obvious that little headway can be made until more progress has been accomplished with normal subjects. For the present we must be content with the general proposition that the feeble-minded are child-like in mental development and that their maturation in specific psychological activities parallels their degree of intellectual maturation. Feeble-mindedness is a generalized tendency toward infantility and mental age gives the most satisfactory central measure of developmental status.

The psychological aptitudes of the feeble-minded are generally inferred from the mental-age orientation. On the other hand, the correlation of psychological abilities is not so high that standing in one activity may be safely inferred from another. Moreover, there is reason to believe that the feeble-minded may be unevenly retarded or specifically handicapped in some directions as compared with others. Thus the feeble-minded are conspicuously lacking in initiative, imagination, sustained attention. Compared with normal persons of the same mental age-levels they are less adaptive, less original, less favorably motivated. In general motor coördination there is definite evidence of generalized reduction in skills, posture, gross and fine coördination. Their interests are meager, their learning handicapped, their dispositions naïve, their perceptions feeble. In compensation, memory is rather generally well-developed and behavior more habituated and less subject to original modification. In general, there is relative reduction in abstract as compared with concrete learning and response, more emphasis on the material as opposed to the idealistic, more learning from drill and example than from rule and precept. It is impracticable here to consider these details of the psychology of feeble-mindedness except

in such general terms. It is important in evaluating the literature to note that the significance of comparative studies is greatly limited by the failure to take account of the nature and level of the processes investigated as well as their relation to clinical type and specific etiology. The "level of mastery" and the advantage in life-age which accompany the mental-age comparisons with average normal children must also be considered.

(c) *Developmental*. The third criterion of an essential definition, namely, arrested development, is a general criterion and applies to all details of individual consideration. The feeble-minded are essentially juvenile in a generalized sense, and this juvenility is reflected in all parts of their development. In addition to the essential arrest of social maturation and the accompanying arrest in mental development there is a retardation of growth in physical development, lack of progress in educational and occupational attainment. Special attention must also be paid to those features of medical history which include children's diseases, accidents, malformations, and other morbidity. Of course not all subjects are equally or even necessarily retarded in all respects. Allowance must be made for individual differences.

(d) *Educational*. As in other respects, the feeble-minded are backward in profiting from instruction. To begin with, the imitative learning of infancy and early childhood is only feebly evident and response to parental instruction is essentially acquisition of habits through repetition. Speech is generally acquired late and is generally lower than mental-age expectation. Mastery of school subjects, especially abstract or academic subjects, also shows specific handicap in addition to that which accompanies mental-age retardation. The feeble-minded rarely exceed fourth-grade academic instruction. Indeed, retardation in profiting from academic instruction at school is one of the early and significant presumptive symptoms of the condition. Mastery of routine information is also relatively meager because of the lack of varied interest and fertile imagination. Whereas the normal child contributes to his learning in the instructional situation by pooling knowledge gleaned from other sources, therefore reënforging his learning through transfer, this seldom occurs among the feeble-minded. In manual and occupational pursuits, on the other hand, the feeble-minded are relatively proficient as compared with normal children of the same mental ages because of their advantage in life experience, physical size, and repetition of instruction. These educational symptoms must be related to school opportunity and relevant disabilities for learning.

(e) *Constitutional*. As already suggested, mental deficiency is essentially a developmental retardation in maturation of the entire organism, structurally, functionally, and expressively. In physical growth and development there is generalized retardation, commonly termed hypoplasia. This somatic deficiency is not so conspicuous as the expressive deficiency in social competence, mental competence and educational attainment. It is found, however, in skeletal form, cranial content, endocrine metabolism, circulatory functioning, neuromuscular coördination and sensory acuity. It is impracticable here to review the wealth of literature on these several departments of physical growth.

Obviously the various aspects of mental deficiency cannot be divorced from their organic background. These psycho-biological preconditions of feeble-mindedness constitute the principal medical interest in the subject. Indeed, the phrase "of constitutional origin" might well be added as a fourth criterion to an essential definition. It is of course true that extreme environmental deprivation and physical and phys-

iological accidents of the environment may produce feeble-mindedness. Instances of the former are perhaps more obscure than rare, but at least this possibility must be reckoned with. In general, feeble-mindedness due to absence of environmental stimulation favoring normal development produces retardation which apparently may be either temporary or long-standing in its effects and may to some extent be overcome by favorable environmental stimulation. Instances of accidental feeble-mindedness produced by trauma and disease are more dramatic than frequent and the results are relatively more permanent.

(f) *Etiological.* The point may be argued as to whether mental deficiency is a disease or a condition and the argument will depend rather heavily on the definition of these terms. In the majority of instances mental deficiency reflects a congenitally low potential for development not obviously related to disease processes. The study of feeble-mindedness from this point of view is perhaps ultimately to be found in the field of auxology which deals with those processes both normal and abnormal which affect growth and development of the organism as well as the expression of these in behavior. The disease processes reflected in mental deficiency are largely those which produce the clinical varieties of feeble-mindedness such as mongolism, intracranial birth lesions, and the relatively infrequent clinical types such as microcephaly, hydrocephalus, epiloia, phenylketonuria, amaurotic family idiocy, and so on.

The accurate diagnosis of mental deficiency is reinforced when a fairly definite etiology can be demonstrated or plausibly assumed. Such an explanation of the condition lends weight to the presumptive fact. The major cause of feeble-mindedness as a condition is to be found in simple ancestral heredity; the major causes as a disease are spread over a wide field of influences some of which are rather clearly understood and others only hypothecated. The exploration of these disease processes has proved promising in recent additions to the literature of neurology, psychiatry, and endocrinology.

However viewed, mental deficiency as previously noted is a condition which obtains at maturity. The condition has long been considered as incurable. The exceptions to this rule are so few as only to confirm the generalizations presented above. The amelioration of mental deficiency by medical means offers only little promise, and reduction in the severity of the social symptoms is to be expected only within a relatively narrow range of improvement through training and environmental opportunity.

The condition of pseudo feeble-mindedness has been recognized as applying to those individuals who for the time being appear to be mentally deficient, but in whom the causes operating are relatively temporary, or the symptoms misleading.

Theoretical implications. Improved concepts of mental deficiency may be sought in the realm of rational speculation designed to relate the condition to knowledge in other fields of investigation. Such philosophical reasoning on the nature of feeble-mindedness produces at least five rather plausible possibilities.

(a) Feeble-mindedness is most generally conceived as a simple quantitative deviation from normal developmental attainment. From this point of view mental deficiency is a condition of uncomplicated human subnormality without serious qualitative variation. This point of view ignores the contrary observations of those intimately familiar with the feeble-minded, neglects the problem of clinical varieties, and gives less than adequate attention to the correlation of symptoms with etiology.

It is, however, supported by mental-age comparisons of feeble-minded and normal for the majority of cases.

(b) Feeble-mindedness may be thought of as a pathological condition arising from constitutional injury, disease, or anomaly as a result of unforeseen vagaries of development or accidents of reproduction and social experience. From this point of view mental deficiency reveals a pathological morphology involving qualitative variation from the normal as well as quantitative subdeviation. This point of view may be used as a supplement to the preceding rather than as a contradiction of it. It applies with special weight to the secondary forms among which the mental-age comparisons are relatively less satisfactory than in the primary form.

(c) Feeble-mindedness as a deficiency of human evolution may be conceived as an ontogenetic idiosyncrasy. From this point of view mental deficiency represents a tendency toward permanent infantility of greater or less degree, or an arrested development at one or another of the preadolescent stages of individual evolution in a standard environment. This point of view is essentially that of the mental-age concept as a result of which the behavior of the feeble-minded of any life age is compared with that of normal children of standard ages.

(d) Feeble-mindedness may be conceived as a phylogenetic atavism. From this point of view mental deficiency is considered as related to some simpler stage of social evolution or level of culture than that in which the individual lives his life. The level of cultural attainment of the feeble-minded person may thus be compared with those of so-called primitive, savage, or preliterate stages or forms of anthropological progress.

(e) Finally, the feeble-minded may be compared with infra-human levels of development, especially those represented by the higher anthropoids. From this point of view mental deficiency may be studied in relation to prehuman stages of phylogenetic origin in which the level of non-linguistic adaptive behavior becomes the critical criterion.

These various theoretical speculations are not mutually exclusive. Each of them is susceptible to profitable experimental exploration, the pursuit of which may well enough further elucidate our knowledge of the condition as a whole.

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GERTRUDE STEIN, WILLIAM JAMES, AND GRAMMAR

Readers of Miss Stein's more difficult productions, from *Tender Buttons* to *Operas and Plays* and beyond, have had thrust upon their attention numberless grammatical peculiarities, ranging from the violation of the conventional rules of punctuation to the complete structural disintegration of the traditional form of the English sentence. Examples abound. We may take as typical of Miss Stein's triumphs over grammar the following congeries of vocables—I dare not say sentences—from "Lynn and the College de France."¹

Mary River. Pleases. In. Harvest. It. Or rather. He arranges. With it.
By. Then. Or whether. There is an interruption.
In hurriedly. Looking. For. Their door. To them.
The college of France. Has learned.

¹ Gertrude Stein, *Operas and Plays*, 1932, 260.

And will. All. Seats of learning.
Which they do. Having. Been fought.

It is not our problem to determine the precise meaning, if any, of the preceding cryptogram. Ours is the far easier objective of suggesting, from the point of view of Miss Stein's interest in language, and her theory of its use, a definite purpose underlying her radical breach with the grammar of her native tongue.

A few years ago it was skillfully argued by Professor B. F. Skinner, in an *Atlantic Monthly* article entitled "Has Gertrude Stein a Secret?";² that nothing rising to the dignity of a purpose is involved in these esoteric utterances of Miss Stein's. On Mr. Skinner's view (and the reader will find the hypothesis by no means lacking in the necessary documentation) the true and only begetter of these verbal patterns is Miss Stein's right arm, operating automatically, as it had learned to do in the Harvard psychology laboratory in the middle nineties, when its owner was a student of psychology at neighboring Radcliffe. Mr. Skinner does not believe that *Three Lives* or *The Autobiography of Alice B. Toklas* was written in this automatic fashion. These he would allow to be the products of the integral personality of Miss Stein; but such books as *Portraits and Prayers* and *Operas and Plays* he regards as in great part the cold and unmeaning products of Miss Stein's unhappy faculty of disengaging from her central self an "elbow" with nothing significant to say and with no power to import interest into the saying of it.

It is farthest from my mind to attempt a refutation of Mr. Skinner's interesting and valuable thesis. I think, however, that his view may require modification in such a way as to take into the reckoning certain quite definite stylistic doctrines of Miss Stein's which are on the conscious level of her mind, and which she has perhaps attempted to exemplify in the kind of writing that is in question. Some of these doctrines, dealing with the subtler issues of literary composition, are expressed with a degree of clarity that is not altogether usual in her writing, in her *Narration*, and related pronouncements appear quite explicitly in the slightly earlier *Lectures in America*.

It is now my turn to offer a hypothesis, which we briefly state before supplying any of the evidence available for its support. My suggestion is that most if not all of Miss Stein's writing which resembles in form and content the early automatic writing, is the attempt to put into practice some notions of the ideal function of language, notions which were in all probability derived from the distinguished teacher of her Radcliffe days, William James.

James' interest in language was, naturally, subservient to an overarching interest in the larger problems of psychology and philosophy. One finds, scattered here and there throughout his writings, many pregnant comments upon the nature of language, and the inadequacy with which it performs its important function in human life. We shall quote some of the more memorable of these, particularly those occurring in his *Psychology*, published shortly before Miss Stein's student days. In the famous chapter, "The Stream of Thought," we hear him voicing a complaint against the fixed and unchangeable character of names, particularly as they occur in our English speech, and against the resulting injury to the adequacy of our vision of the world: "What, after all, is so natural as to assume that one object, called by one name, should be known by one affection of the mind? But if language must thus influence

² B. F. Skinner, *op. cit.*, 153, 1934, (January), 50-57.

us, the agglutinative languages, and even Greek and Latin with their declensions, would be the better guides. Names did not appear in them inalterable, but changed their shape to suit the context in which they lay." (P. 236.) A little later we are further warned against the over-simplification in which the verbal christening of our thoughts is so eminently apt to land us: "Here again language works against our perception of the truth. We name our thoughts simply, each after its thing, as if each knew its own thing and nothing else. What each really knows is clearly the thing it is named for, with dimly perhaps a thousand other things. It ought to be named after all of them, but it never is." (P. 241.)

But the climactic passage, marked by James' full genius for expression at once precise and picturesque, arrives on page 243, with its memorable metaphor of "flights and perchings": "As we take . . . a general view of the wonderful stream of our consciousness, what strikes us first is this different pace of its parts. Like a bird's life it seems to be made of an alternation of flights and perchings. The rhythm of language expresses this, where every thought is expressed in a sentence, and every sentence closed by a period. The resting-places are usually occupied by sensorial imaginations of some sort . . . the places of flight are filled with thoughts of relations, static or dynamic, that for the most part obtain between the matters contemplated in the periods of comparative rest." We are presently told that the "substantive parts" of the stream of our consciousness, *i.e.* the *perches*, lord it over the "transitive" flights, a tyranny which language assists in making possible. "We ought to say a feeling of *and*, a feeling of *if*, a feeling of *but*, and a feeling of *by*, quite as readily as we say a feeling of *blue* or a feeling of *cold*." The result, James tells us, is a psychic impoverishment in which "all *dumb* or anonymous psychic states have . . . been coolly suppressed; or, if recognized at all, have been named after the substantive perception they led to, as thoughts 'about' this object or 'about' that, the stolid word *about* engulfing all their idiosyncrasies in its monotonous sound. Thus the greater and greater accentuation and isolation of the substantive parts have continually gone on."

This same standpoint James restated and reënfirmed in some of his very latest writings, those collected under the title *Essays in Radical Empiricism*. It is here that he expounded his distinctive doctrine that relations "conjunctive as well as the disjunctive" are just as much "matters of experience as the things themselves." On that hypothesis it is obvious that conjunctions, prepositions, *et id genus omne* need no longer feel any ontological inferiority to substances and adjectives, their traditional overlords. It is a corollary to James' position here, though he does not explicitly draw it, that a sentence without conjunctions and so forth to indicate its relations to the rest of reality is as metaphysically deficient as one which, while quite explicit in its relationships, is lacking a definite *something* to be 'about.'

All of the criticisms of language which we have cited from James can be matched by explicit pronouncements of Miss Stein's. To these let us now turn, restricting attention to the close parallels in one of the earlier-mentioned *Lectures in America*, viz. "Poetry and Grammar." As we read along, the first arrow to strike the target of our interest is the question on page 209 (punctuation and spelling hers): "Do you always have the same kind of feeling in relation to the sounds as the words come out of you or do you not. All this has so much to do with grammar and with poetry and with prose." This query leads directly into a discussion of those parts of speech favored by some other authors and those favored by Miss Stein. She is rather

sniffy about nouns. "A noun," she tells us, "is a name of anything, why after a thing is named write about it. A name is adequate or it is not. If it is adequate then why go on calling it, if it is not then calling it by its name does no good. . . . Nouns are the names of anything and just naming names is alright when you want to call a roll but is it any good for anything else. . . . Slowly if you feel what is inside that thing you do not call it by the name by which it is known. Everybody knows that by the way they do when they are in love and a writer should always have that intensity of emotion about whatever is the object about which he writes. And therefore and I say it again more and more one does not use nouns."

As a corollary to this proposition about nouns follows the condemnation of adjectives. These are condemned for the company that they keep: "adjectives are not really and truly interesting . . . because after all adjectives effect (sic) nouns and . . . the thing that effects a not too interesting thing is of necessity not interesting." (P. 211.)

Now for the first time we get into grammatical good company,—we are introduced to verbs and adverbs! Among the virtues attributed to them is the truly Jamesian excellence that they move and change, thus manifesting their relative adequacy to the changeful "stream of consciousness." We read on page 212: "verbs can change to look like . . . something else, they are, so to speak on the move and adverbs move with them and each of them find themselves not at all annoying but very often very much mistaken. That is the reason anyone can like what verbs can do."

We need not pursue Miss Stein through the whole of her whimsical revaluation of the parts of speech. It is necessary for our purpose, however, to note her curious exaltation of prepositions and conjunctions. Let us hear her speak: "Prepositions can live one long life being really being nothing but absolutely nothing but mistaken . . . I like prepositions the best of all" (p. 212). . . . "Beside that there are conjunctions, and a conjunction is not varied but it has a force that need not make anyone feel that they are dull. Conjunctions have made themselves live by their work. They work and as they work they live. . . . So you see why I like to write with prepositions and conjunctions and articles and verbs and adverbs but not with nouns and adjectives. If you read my writing you will you do see what I mean." (P. 213.)

It remains to ask, what is the general purport of these grammatical doctrines of Miss Stein's, and how closely do they conform to the earlier-presented gospel of grammar according to William James? In general it would seem safe to say that Miss Stein is urging a program of linguistic usage which, though definitely out-running James' demands—one suspects that James would have found some of its features not a little extravagant—is nevertheless founded upon a conception of the "stream of consciousness" quite similar to that of James. Her accent on the more fluid and moving elements in language (the verbs and adverbs), her corresponding depreciation of the static moveless noun, what is this but the counterpart of James' plea in behalf of the "flights" as against the linguistic predominance of the "perchings?" And perhaps the most striking, though by no means the most fundamental, point of parallelism between them is the prominence they both accord to conjunctions and prepositions, the often unappreciated parts of speech. It is perhaps not wholly fanciful to discern in this doctrine of verbal equality a remote variation upon the theme of sturdy American democracy which is characteristic alike of James' *A Pluralistic Universe* and of Miss Stein's *The Making of Americans*.

In conclusion, I would not insist upon the literal derivation by Miss Stein of her grammatical predilections from William James. On the other hand it is difficult not to suppose at least an initial arousal and some permanent direction of interest toward the philosophy of grammar, as having passed from the persuasive teacher to the girl whom he evaluated as the most brilliant of all his feminine students. The substantial result of our inquiry, however, remains independent of the question of source. From whatever quarter the wind of doctrine blew, it propelled Miss Stein toward an appreciation of the potential interest to the literary craftsman of the subtle issues, half psychologic, half philosophic, which turn upon the finer categorical analysis of the creative word. It is this intellectual concern with linguistic experimentation, though one may quite deny the success of the experiments, which may supply a clue for distinguishing the products of Miss Stein's literary workshop from those early automatic fruits of the Harvard laboratory of Psychology.

University of Maine

RONALD B. LEVINSON

WHOLE AND PART RELATIONSHIP IN CREATIVE THOUGHT

Wertheimer, in his exposition of Gestalt theory, states that "there are wholes, the behavior of which is not determined by that of their individual elements, but where the part-processes are themselves determined by the intrinsic nature of the whole."¹ "The individual parts ('elements') are not primary, not pieces to be combined in and-summations, but are 'parts of wholes'."² Köhler also declares that "the whole configuration determines what the relatively separate natural parts of a figure shall be."³ Other writers have made similar statements. Wheeler asserts that "the wholes are primary and the parts are derived from them by means of individuation."⁴

The primacy of the whole over the part in creative thought has been investigated in a study of poets⁵ and one of artists.⁶ In the first investigation 55 poets and 58 non-poets wrote poems after looking at a picture which was presented to them. They talked aloud while looking at the picture and composing the poem, which verbal reports were recorded in shorthand. Four stages of creative thought were found to be present. (1) There first appears preparation, when the writer is assembling or receiving new ideas and associations are shifting rapidly. His ideas are not yet dominated by any coherent theme or formulation. (2) Incubation follows preparation, although it may accompany it. A mood or idea is being incubated when it involuntarily repeats itself with more or less modification during a period when the subject is also thinking of other topics. It recurs from time to time. (3) In the third stage of illumination the idea, which has been incubating, becomes crystallized into definite lines. (4) In the final stage of verification or revision the idea is elaborated and revised.

These four developmental stages were also found in a similar study of artists, during which 50 artists and 50 non-artists sketched pictures after reading a given

¹ M. Wertheimer, Gestalt theory, in W. D. Ellis' *Source Book of Gestalt Psychology*, 1939, 2.

² *Op. cit.*, 88.

³ W. Köhler, Reply to G. E. Müller, in W. D. Ellis' *Source Book of Gestalt Psychology*, 1939, 380.

⁴ R. H. Wheeler, *Science of Psychology*, 1940, 20.

⁵ C. Patrick, Creative thought in poets, *Arch. Psychol.*, 26, 1935, (no. 178), 1-74.

⁶ Patrick, Creative thought in artists, *J. Psychol.*, 4, 1937, 35-73.

poem. Their verbal reports, made while sketching the pictures, were likewise recorded in shorthand. Since no attempt was made in either of the studies at that time to ascertain whether the idea of the whole preceded the idea of the parts, it was later decided to make such an analysis and the results are here presented.

During the first two stages, preparation and incubation, the idea, which later becomes expressed in a picture or a poem, does not yet have definite form. In preparation, ideas often occur without much relation to each other, for they are shifting rapidly. As one poet states, "various ideas are pressing in upon the mind." During incubation an idea recurs from time to time in different mental sets and is modified as it recurs. During preparation and incubation there may either be a general idea or mood from the first time that the poet or artist considers making a picture or poem; or there may at first be a small detail which expands into a general idea during the modification which occurs in incubation. In both cases, however, the idea which emerges in illumination (when it is first written or sketched in definite form) is a general one, as shown below.

An example of incubation, where a general idea exists from the first time that the poet thinks about writing a poem, is given in each of the following quotations from two authors: "A poem is a spiritual irritation. It annoys me till it breaks out. I sometimes incubate a mood for years. I do not incubate the lines. The idea lies fallow and comes back when conditions are favorable." "If I feel blue or excited, then I sit down and try to get it out of my system. Sometimes it grows for days and days, and then I sit down and write it off. The mood comes first and then the phrase." On the other hand, an illustration of a detail which expands into a general idea during the modification, that occurs in incubation, is seen in the following statement from another poet, "I get a word and carry it around in my head. Then other words come. Pretty soon I get a phrase which is the nut of the poem, then I write the poem and modify that phrase."

Besides such comments, the protocols obtained during the experimental situations were analyzed to see whether an idea began with a detail and developed to a

TABLE I
PERCENTAGE OF Ss SHOWING NATURE OF IDEA DURING INCUBATION

Group	Detail to general	General throughout	Uncertain
Poets	18	46	36
Non-Poets	36	45	19
Artists	24	60	16
Non-Artists	30	56	14

general form during the incubation stage, or whether it was general throughout this period. The percentages are given in Table I.

The majority of the Ss had a general idea from the beginning of incubation, which was somewhat modified during that stage until it appeared in definite form in illumination. A large proportion, however, showed that the idea started as a detail and was modified to general form during incubation. These data indicate that in incubation either a detail may develop into a general idea, or the general idea may exist from the time S first thought about writing a poem or drawing a picture.

¹ Patrick, *op. cit.*, *Arch. Psychol.*, 30.

As mentioned before, during incubation the idea has not yet become definite. It is in the third stage of illumination that the idea first crystallizes into a definite form. The general outline of the picture or poem appears then, as the general shapes are drawn for the first time or the majority of lines are being drafted. The fourth and last stage of revision consists in developing details. Some details are omitted and others added. The attention is focused on certain words of a poem or parts of a picture. The thought shifts from a survey of the whole to a survey of the parts. As the data show, these stages may overlap. There may thus be several shifts of attention from the whole to the parts, as illumination overlaps with revision.

TABLE II
SPECIFIC COMMENTS CONCERNING GENERAL IDEA IN ILLUMINATION STAGE

Group	% Ss showing	
	Comments	No comments
Poets	66	34
Non-Poets	83	17
Artists	84	16
Non-Artists	72	28

Part of revision may be a critical reviewing of the poem or picture as a whole, but it usually consists of examination of specific parts or details.

Most of the Ss made comments showing the general idea, which they had in mind, just as they started to write or sketch. Table II shows those instances in which specific comments were made to show the nature of this idea of the whole, which the subject had as he started to write or sketch. The absence of such a remark does not necessarily indicate that such a general idea did not exist, for it might not

TABLE III
AVERAGE PERCENTAGE OF COMMENTS CONCERNED WITH GENERAL IDEA AND DETAILS IN REVISION STAGE

Group	Average percentage of comments concerned with	
	General idea	Details
Poets	8	92
Non-Poets	7	93
Artists	3	97
Non-Artists	4	96

have been mentioned in a definite phrase. It was made evident only as the writing or drawing proceeded. Such specific comments were made more frequently by the non-poets. This may be due to the fact that they are more apt to make a prose summary beforehand, because of the difficulty which they have in using the metrical form.

Table III shows that most of the instances of revision, the last stage, are concerned with details or parts of the picture or poem. A small percentage deal with a survey of the picture or poem as a whole. The percentage of such instances concerned with the whole is only a small fraction of the number of instances

dealing with details. In the final stage of revision, the attention is chiefly focused on details.

The primacy of the whole over the parts is apparent in the last two stages. When the idea becomes definite for the first time, which is characteristic of the third stage of illumination, it is a general idea, and we can say that the whole precedes the parts, for the details are added later in the final stage of revision. During the first two stages of preparation and incubation, when the idea is gradually being modified into definite form, there may either be a mood or general idea of the whole from the very beginning when the poet starts to think about writing a poem, or a detail at the beginning which grows into a general idea of the whole as a result of this modification. In either case, the idea which is first written or sketched in illumination is a general one. In the last stage of revision the attention is chiefly focused on details or parts.

In many cases the primacy of the whole over the part is apparent from the beginning of the task to write a poem or draw a picture. In other cases, however, the idea of the whole develops from a detail or part during incubation; which idea of the whole in turn precedes the parts or details brought out in revision.

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CATHARINE PATRICK

APPLICATION OF HUTT'S REVISED SCORING OF THE KOHS BLOCK DESIGNS TEST TO THE PERFORMANCES OF ADULT SUBJECTS

Some years ago Hutt introduced a revised scoring system for the Kohs Block Designs test which was based solely on time taken to complete the designs as contrasted with Kohs' original scoring based on time taken and moves made in completing the designs.¹ This revised scoring system, which eliminates the necessity for counting moves, has significant advantages. It is more objective than the original scoring system, there is less chance for errors of observation, and it allows the examiner the opportunity of observing qualitative aspects of S's behavior which are clinically important—an opportunity which is precluded by the necessity for counting moves in the original scoring system.

Hutt demonstrated the practical equivalence of the two scoring systems with respect to the performances of children on the test. The coefficient of correlation between *MA* scores derived from the two scoring systems in a group of 100 children, ranging in age from 6 to 15 yr., was $+0.992 \pm 0.001$. In no case was the discrepancy in *MA* scores derived from the two methods greater than 6 mo. It was found, however, that the revised scoring tended to raise slightly the *MA* and *IQ* scores. The most probable change in mental age score (middle 50 per cent of the changes), using the original scoring as a base, fell within the limits of 0 and $+2$ mo.

The question arises whether the revised scoring applies equally well to the performances of adult Ss on the test. The question is of interest in view of the recent applications of the Kohs test to the study of the intellectual functioning of neurological and psychiatric patients and its general use as a non-verbal test of adult

¹ M. L. Hutt, A simplified scoring method for the Kohs Block-Designs tests, this JOURNAL, 42, 1930, 450-452; The Kohs Block-Designs tests. A revision for clinical practice, *J. Appl. Psychol.*, 16, 1932, 298-307.

intelligence.² In order to answer it, the performances of 35 adults, ranging in age from 18 to 57 yr., were scored according to both methods. The range in point scores, computed according to the original method, was 19 to 133. The range in point scores, computed according to the revised method, was 19.5 to 133. The mean point score, computed according to the original method, was 92.1. The mean point score, computed according to the revised method, was 91.5. The coefficient of correlation between the point scores derived from the two methods was $+ 0.996 \pm 0.001$. Differences in point score derived from the two methods, using the score derived from the original method as a base, varied from +3.5 points to -4.5 points. The most probable change in point score (middle 50% of the changes), using the score derived from the original method as a base, was within the limits of -2 points to + 0.5 points.

The findings indicate that scores computed according to the two scoring methods are practically equivalent when applied to the performances of adult Ss. However, it is found, contrary to Hutt's findings with children, that the revised scoring system tends to *lower* slightly the scores obtained by adults. This difference may perhaps be interpreted to indicate that in children there is a greater tendency to indulge in overt trial-and-error behavior which leads to the making of more moves (for which the child is penalized) while adults tend to restrain this overt trial-and-error behavior in favor of implicit responses.

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ARTHUR L. BENTON

MENTAL DYNAMICS SHOWN BY THE ABBREVIATION AND AMELIORATION OF WORDS IN HEARING AND REMEMBERING

In an experiment in learning the English equivalents of French words there were many errors caused by mishearing, phonetic distortion in memory, or a combination of these. Such, for example, are book for *brook*, movie for *booby*, billows for *bellow*, vowel for *bowel*, grew for *brew*, bruise for *brew*, and brute for *brew*, in all probability.

If the occurrence of a mental connection strengthens it these errors should tend in the direction of commoner words; that is, words more often heard, seen, spoken, or written. In a sample of 100 they do, 55 being commoner, and 42 less common in printed matter (according to the Thorndike count of 10 million words) than the originals. For frequency of occurrence in speech and hearing this difference would almost certainly be accentuated, because cases like movie and whoopee for *booby* would then be transferred from the less common to the commoner side.

If the satisfyingness of the consequences of a mental connection strengthens it these errors should also tend in the direction of words easier to pronounce or write and pleasanter to think of. In our sample they do: 27 have fewer phonemes and 23 have more phonemes than the originals. (In the number of letters there is no appreciable difference.) *Bowel* changes to the pleasanter *vowel* 16 times. The only

² A. B. Nadel, A qualitative analysis of behavior following cerebral lesions, *Arch. Psychol.*, 32, 1938, (no. 224), 1-56; M. M. Bolles, The basis of pertinence, *ibid.*, 30, 1938, (no. 212), 1-51; M. M. Bolles and K. Goldstein, A study of the impairment of "abstract behavior" in schizophrenic patients, *Psychiat. Quart.*, 12, 1938, 42-65.

probable changes in the opposite direction (*bug* to *hog*, *brew* to *brute*, *latch* to *lash*, and *file* to *vile*) are more than counterbalanced by *booby* to *movie*, *bellow* to *billows*, *jolt* to *joke*, and *sallow* to *shallow*.

I regret the smallness of the sample, and have waited 10 years in the hope of increasing it, but it now seems unlikely that I shall be able to do so.

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E. L. THORNDIKE

RESEARCH ON THE PROBLEMS OF AGING

The National Institute of Health of the United States Public Health Service is organizing a new unit for research in gerontology, the problem of aging. Dr. Edward J. Stieglitz, National Institute of Health, U. S. Public Health Service, Bethesda, Maryland, is in charge of the work. With the present shift toward greater age in the population of the United States, senescent individuals are becoming more important in the national economy and defense. Aging is a continuous biological phenomenon which starts upon the creation of a new individual and continues at various rates until his death. While gerontological research is directed upon this entire process and includes the study of the diseases of the aged, the present undertaking directs attention particularly upon the normal processes of late maturity, approximately the period between 40 and 60 years of age.

To advise this new unit there has been formed a National Committee on Gerontology, consisting of L. R. Thompson, Director (National Institute of Health), A. J. Carlson (physiologist, University of Chicago), C. L. Christiernin (Association of Life Insurance Medical Directors of America), R. E. Coker (zoölogist, University of North Carolina), William Crocker (botanist, Boyce Thompson Institute of Plant Research), L. K. Frank (sociologist, Josiah Macy, Jr. Foundation), A. B. Hastings (biochemist, Harvard University), Ludvig Hektoen (pathologist, Consultant, U. S. Public Health Service), Winfred Overholser (psychiatrist, St. Elizabeth's Hospital), Clarence Selby (physician, General Motors Corporation), W. D. Stroud (clinician, Philadelphia). The committee lacks a psychologist, and it is to be hoped that this omission will presently be corrected.

The new unit is beginning its work with a survey of present trends in investigations and contemplated investigations into the problems of aging. The Unit solicits information about projects in gerontology, not the actual findings, which may well wait upon publication, but the problems and methods which are being used now or are planned. Inquiries are being sent to scientists in the biological sciences and to clinical investigators asking about the fundamental work upon the processes, mechanisms and consequences of senescence, but it is particularly important for Dr. Stieglitz to receive information from persons who do not receive these inquiries and who are working, either on problems of gerontology or on other problems which have a definite gerontological bearing. The editors join with him in urging psychologists to make the survey complete, at least as regards their own relevant research in progress or in contemplation. The survey should result in the discovery of important untouched fields for which investigation should be begun and for which support might be found.

E. G. B.
K. M. D.

THE FORTY-EIGHTH ANNUAL MEETING OF THE
AMERICAN PSYCHOLOGICAL ASSOCIATION

The Forty-Eighth Annual Meeting of the American Psychological Association was held at Pennsylvania State College, State College, Pennsylvania, from Wednesday to Saturday, September 4-7, 1940. A total of 1237 persons registered; 251 being members, 490 being associate members, 58 being newly elected associates, 18 newly elected and transferred members, and 420 being persons not affiliated with the Association.

Newly elected officers are: President, Herbert Woodrow, University of Illinois; Directors, Edwin R. Guthrie, University of Washington, and Joy Paul Guilford, University of Southern California; Division of Anthropology and Psychology of the National Research Council, Joy Paul Guilford, Harold Ellis Jones, University of California, and Norman R. F. Maier, University of Michigan; Social Science Research Council, A. T. Poffenberger, Columbia University. Willard C. Olson, University of Michigan, was reelected as Secretary for a three-year period. In conformity with an earlier vote it was announced that the Forty-Ninth Annual Meeting would be held on the campus of Northwestern University, from Wednesday to Saturday, September 3-6, 1941. The Fiftieth Anniversary Meeting is to be held at Harvard University on September 2-5, 1942. The Association voted to create a Committee on Observance of the Fiftieth Anniversary of the American Psychological Association and the Centennial of William James. Edwin G. Boring, Harvard University, was appointed as Chairman of this committee, with S. W. Fernberger, University of Pennsylvania, as Historian, and J. G. Beebe-Center, Harvard University, as an added member.

The program presented was drawn from a total of 260 abstracts, the largest number submitted in the history of the Association. From these, 190 abstracts, distributed in 24 section meetings, were incorporated in the program. While it is difficult to make any technical statement about program trends, it is of interest to note the appearance of sufficient papers to constitute a section on Air and Highway Traffic. A group of papers on projective techniques, while not sufficient for a separate section, was incorporated in one of the meetings on Personality. There were also a sufficient number of abstracts to schedule a section on Musical Aptitudes. A somewhat typical distribution of papers continued in the fields of clinical, animal, industrial, abnormal, vocational, educational, and social and political psychology, and in the specialized areas of human learning, conditioning, conflict and frustration, brain functions, vision, growth and development, and auditory and cutaneous functions.

On Friday evening the presidential address, "The Experimental Embryology of Mind," was given by Leonard Carmichael, Tufts College. Following the address, Pennsylvania State College entertained members and guests in the Old Main building.

There was intense interest at the meeting in the part the Association should play in plans for morale and the national defense. An Emergency Committee of Council had been created following the 1939 meeting. Walter R. Miles, Yale University, reported as chairman of its activities during the past year. At its own recommendation, this Committee was discharged and Walter R. Miles was made the Association's representative to an Emergency Committee on Psychology of the National Research

Council to be composed of representatives of various psychological and related groups. It has recently been announced by the National Research Council that Karl M. Dallenbach, Cornell University, will serve as chairman of the new Committee.

The question of the amount and nature of service the Association should render to the field of psychology and its Associates and Members has come up from time to time in Council and on the floor at annual meetings. These suggestions have usually been concerned with problems of public relations, promotion of demands for psychological services, and the placement of psychologists. As the result of recommendations of the Committee on Personnel, Promotion, and Public Relations and the Committee on Displaced Foreign Psychologists these matters came to a head at the 1940 meeting in a recommendation for a full-time secretary. Because of the complicated problems of policy and finance involved, a Committee on Extension of Duties of the Secretary's Office was formed consisting of the President, Treasurer, and Secretary. This Committee was charged with the responsibility of studying the problem and reporting at the next meeting. An appropriation of \$300.00 was also voted by the Association for setting up a duplicate roster of psychologists in connection with the Secretary's Office, should it prove possible to develop a plan in co-operation with the National Roster of Scientific and Specialized Personnel, of which Leonard Carmichael is Director.

The Committee on Scientific and Professional Ethics, Robert S. Woodworth, Columbia University, chairman, presented an extensive report on its activities during the past year with a series of recommendations. A standing committee with the same name was created by modification of the Constitution. A further change gives this Committee power to act in disciplinary cases. A. T. Poffenberger, Columbia University, was made chairman of the new Committee with a group of past presidents as members.

A joint meeting of the Council of Directors and Board of Editors was held for the discussion of policies and business matters. Consideration was given to the effect of the war on subscriptions to the Association's periodicals and the volume of abstracts for the *Psychological Abstracts*. The reelection of Herbert S. Langfeld, Princeton University, as editor of the *Psychological Review*, the resignation of Raymond R. Willoughby, Brown University, as assistant editor of the *Psychological Abstracts*, and the appointment of H. L. Ansbacher, Brown University, as his successor were announced.

The Business Manager of the Association's publications, Willard L. Valentine, Northwestern University, reported that the club subscription rate of \$10.00 for Members and Associates had received such a generous response that it was possible to offer an \$8.00 club rate for 1941.

The American Association for Speech Correction, registering 46 persons, met on September 2 and 3 prior to the opening of the American Psychological Association meetings. The American Association for Applied Psychology, the Psychometric Society, and the Society for the Psychological Study of Social Issues, affiliated societies, held meetings prior to and during meetings of the American Psychological Association.

The dinner meeting of the Psychometric Society on Thursday was followed by the address of the President, Karl J. Holzinger, University of Chicago, on "A Synthetic Approach to Factor Analysis." Jack W. Dunlap, University of Rochester, was elected president for the coming year.

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

Industrial Conflict: 1939 Yearbook of the S.P.S.S.I. Edited by George W. Hartmann and Theodore Newcomb, New York, Cordon Co., 1940. Pp. xi, 583.

This book cannot be summarized and criticized briefly. It contains 22 articles and offers nearly an equivalent number of different points of view and approaches to the understanding and misunderstanding of the problem of industrial conflict. The editors assist the reader by grouping the papers into four parts. The fifth part is reserved for editorial summary. Each part is preceded by a foreword by the editors explaining the logic responsible for the inclusion of the papers in that section. Each part is also followed by comments by the editors in the form of wider generalizations to be drawn from the papers. Part I provides the reader with background and orientation. Part II views industrial conflict from the point of view of individual needs, tensions, and satisfactions. Part III is more difficult to identify; it contains papers dealing primarily with "social" factors relating to industrial conflict. Part IV considers procedures for eliminating industrial conflict.

Part I sets the stage—so the editors tell us—"in order that the contemporary phenomena of industrial conflict may be viewed in the most advantageous perspective," but to the reviewer after reading this section the stage remained dimly lit with some essential "props" missing. Although most of the papers which follow are in a large part concerned with the interpretation of verbal behavior, there is no introductory chapter dealing in general with this important problem for social science. Chapter 1, consisting of two papers, is essentially concerned with establishing the proposition that psychologists have a right to study social issues and that in so doing they will not lose their "objectivity." Although the preceding paper maintains that "by common consent most of the ills of the world today are agreed to be psychological," the second chapter defends the thesis that "the current conflict situation has its roots in the economic structure of our society." After introducing this "economic" interpretation of industrial conflict, no more explicitly stated conceptual schemes are offered.

The last two papers in Part I describe and interpret two conflict situations—the automobile strikes of 1937 in Detroit and the Johnstown strike of 1937. Why these attempts—no matter how crude one may think they are—to study and diagnose concrete human situations should be introduced as background material rather than as samples of the application of psychological skill to the understanding of concrete, and admittedly complex, human situations is a mystery to the reviewer. From the remaining contributions, it seemed to the reviewer that talking about human behavior in general was considered more "scientific" than trying to describe and to discover uniformities in the behavior of people in a particular situation. For most of the subsequent papers discuss employer or employee behavior, motives, feelings, and attitudes apart from their particular organizational or community contexts, and sometimes even apart from their personal histories. Cataloguing the verbal responses of people to certain questions and performing certain statistical operations upon those responses seemed to be the only method recognized as "scientific."

One example is perhaps needed to illustrate what the reviewer regards as the major weakness of the book. In one paper extensive statistical operations are performed on the verbal responses of a group of college students and a group of foremen in various industrial organizations. No information is given about the particular college or colleges from which the students came nor about the particular industrial organizations from which the foremen came. Nothing is known about the "social conditioning" of these persons prior to their participation in these particular social organizations. All the variables which may influence their attitudes are ignored. Statistically significant differences in verbal responses in the two "groups" to certain given questions are the only findings reported. Yet conclusions are drawn about "college students" and "foremen" and to top it all off the paper is entitled "Attitudes of Prospective and Actual *Executives* on Social Issues in Personnel Policies."

Over and over again the contributors seem to be surprised that people with different backgrounds and from different social places in particular formal organizations and in the community see "the world" differently. Such a lack of intimate familiarity with their data and such semantic naïveté, as some papers show, seem unpardonable on the part of psychologists. If "science" consists in dealing with "items" torn from their contexts and in dealing with words apart from their referents, if it consists of a "method" which can be applied by people who have no intimate, intuitive familiarity with their data, then, in the opinion of the reviewer, let us have less "science" and more researchers who are less afraid to lose their "objectivity" and more eager to face the complex material with which social science—willy-nilly—has to deal.

With the aim of the *Yearbook* and its editors the reviewer is highly in accord. It is an undertaking in a much needed direction, and it is hoped that it will continue and improve with each issue. It is profitable reading for all psychologists who want to know where they stand when they enter the "practical" arena in the year 1939.

Harvard University

F. J. ROETHLISBERGER

Psychology. By L. F. SHAFFER, B. v. H. GILMER, and M. SCHOEN. New York, Harper & Bros., 1940. Pp. xii, 531.

The authors of this general textbook in psychology have succeeded admirably in attaining the objective set forth in their preface of making "a balanced presentation of psychological attitudes, researches, and practical applications within the limited scope imposed by the introductory course." Happily the book is written for the college student and not for the entertainment of the authors nor for the enlightenment of other psychologists. It is mechanistic in approach and classes psychology with the biological rather than with the social sciences. It emphasizes stimulus-response relationships perhaps even more than the originators of the S-R formula. In the reviewer's opinion, its most important assets consist of its eclectic choice of material, its original and refreshing arrangement of topics, and its direct and straightforward style of writing. No doubt some of these virtues are to be traced in part to the judgment of the ten psychologists, all in different universities, who read and criticized the manuscript before it was published. The authors are to be congratulated upon such an extensive attempt to obtain a fair and unbiased content.

Shaffer, Gilmer, and Schoen's *Psychology* contains 473 pages of text material, divided into 16 chapters which average slightly less than 30 pages each. A bibliography of selected references, grouped according to the chapter headings, is given in the back of the book. Combined with the index is a glossary of the principal psychological terms which are used. There are 117 figures including reproductions in full color of the spectrum and of color mixing charts. To go with the textbook the authors have prepared a study guide containing questions and exercises. A note by the publishers informs the reader that there are also a syllabus, a teacher's guide, and objective tests.

The book conveniently divides itself into five major sections, each of which has a unity and organization of its own. The first three chapters, entitled, "The Aim and Scope of Psychology," "The Nature of Human Adjustments," and "The Bodily Basis of Behavior" (nervous system and effectors), introduce the student to the subject matter. The next four chapters, called, "The Beginnings of Behavior," "Learned Behavior," "Emotional Behavior," and "Motivated and Voluntary Behavior," present the activity of the organism from a genetic viewpoint. Although this section tends to give the impression that the authors are strictly behavioristic, the material which follows quickly dispels such a view. Chapters VIII to XI take up the traditional topics of sensation and perception under the general heading of experience. Chapter XII, "Remembering," and Chapter XIII, "Thinking," stand as a unit by themselves, while the last three chapters discuss "Abilities and their Measurement," "Personality and Character," and "Social Adjustments."

This order of topics seems to work out as a naturally progressive sequence. It has the advantage of acquainting the student with the basic material without the necessity of burdening the introductory chapters with an abstract presentation of psychological method which must be meaningless to many beginners. The whole arrangement is sensible and direct, and is especially well adapted to the introductory course. The authors seem to be especially adept in dispelling common misconceptions and superstitions—an activity in which they indulge regularly throughout the book. An example taken from Chapter XII is as follows: "An incorrect popular interpretation holds that remembered events are 'stored away' ready to come forth when needed. This notion ignores the fact, amply supported by experimental findings, that *remembering is a stimulus-response process*" (p. 319).

In view of the careful elimination of superfluous details which characterizes the first part of the book, the four chapters on "Experience" (sensation and perception) seem by contrast to be exceptionally "heavy" for an elementary text. Three theories of color vision are discussed, along with three theories of audition. Beats and combination tones are given space and the vibration ratios of the notes in the diatonic scale come in for discussion. The theories of Helmholtz and Stumpf on consonance and dissonance are also treated, and the vibratory sense receives discussion, together with two theories of cutaneous sensitivity. One wonders if this emphasis upon the stimulus and physiological side of psychology is an aspect of the general atmosphere of the institution in which the authors have been teaching.

In contrast to these inclusions, which will seem to many psychologists to be too advanced for the introductory course, there are some omissions of other material which is often considered to be fundamental. We are told in a footnote on page 213 that "Fechner's Law is derived from Weber's Law by a simple operation of the integral calculus." Yet, despite the mathematical promise which such a statement

in one part of the book seems to hold, there is practically no treatment of ordinary psychological statistics. Correlation is unmentioned, as are the standard deviation and other measures of dispersion. Any discussion of the statistical concepts of validity and reliability has also been omitted. Another strange omission, which has somewhat puzzled the reviewer, is the failure to include any discussion of the process of recognition, which is disposed of in a single sentence as "a variation of the more fundamental process of recalling" (p. 319).

Such criticisms are, after all, of a minor nature. Taken as a whole the book is a stimulating contribution to the problem of the elementary course. It is certainly a teachable volume. It subscribes to no particular school and should find wide acceptance as an introductory text.

Indiana University

W. N. KELLOGG

The Psychology of Normal People. By JOSEPH TIFFIN, FREDERIC B. KNIGHT, and CHARLES C. JOSEY. Boston, D. C. Heath & Co., 1940. Pp. xv, 512.

The authors of this recent introductory text state that the book was written for students who expect to enter "business, industry and the professions." They also state that its content was determined not only by "fellow psychologists," but also "by what alumni now meeting the problems of business and industry wish they had studied while they were in college," and by the results of "deliberate counsel with superintendents of steel mills, . . . personnel managers of large department stores, . . . resident physicians in large hospitals, . . . superintendents of schools" (p.v.). These affirmations, together with a perusal of the various chapters of the book, make it clear that the primary purpose of the authors is to present a compendium of psychological material which will be intrinsically useful and interesting. In this connection the comment that "it is often stupid to be too tolerant of that which is boring" (p. 251) is probably illuminating.

In pursuit of this aim the authors have seen to it that almost every paragraph deals with some practical and interesting topic, or leads directly to such discussion. Examples of topics which are treated in detail are paramnesia, dreams, the control of emotions in ourselves and others, reading disabilities, illusions, and popular and scientific methods of judging and measuring personality. The authors write extremely well; they frequently inject sharp and apt comments. They completely avoid writing down to the student; and the mild academic humor characteristic of some texts is happily absent. Almost any college student of average caliber should find the book practical, interesting, and easy throughout.

Considering the book, however, as a text for the average course in introductory psychology, the reviewer is inclined to be rather critical of it. For one thing, the authors' viewpoint is of the common-sense, teleological variety. For example, they write: "All behavior is an attempt to reach some goal." "The chief trait of a human being is wanting" (p. 37). "Only when we know his *purpose* do we know what he is really doing" (p. 10).

Secondly, the book is briefer than most introductory texts, since it has only 14 chapters and 512 pages, and although brevity in itself is not undesirable, in this case it has been achieved by the omission, or the very sketchy treatment, of all topics which might seem to be impractical or uninteresting to certain students. For

example, there is little or nothing about animal psychology in any of its phases, the nervous system, physiological reactions in emotion, the causes of forgetting, etc., nor is any theory of learning or of thinking presented.

Furthermore, the book has only 56 figures—compared with 152 in a widely used text of 655 pages—20 tables, and 16 photographic plates. The small number of figures and tables, together with the lack of foot-notes and the use of italics instead of black caps for topic headings, give the pages a monotonous and even confusing appearance. As to the plates, the reviewer confesses to a marked prejudice against the intrusion into college texts of photographs of the *Life* or *Look* variety. Why should the elementary fact that "mental attitudes are strongly influenced by the radio" have to be illustrated by photographs of different listeners (to say nothing of the question whether a "mental" attitude can be so depicted)? Why must the importance of attention be exemplified by a picture of two smashed automobiles and a corpse beside them? And what place does the "sex appeal" type of picture (see Plates X and XII) have in a material which after all is not advertiser's copy? Any student could see through such transparent attention-getting devices as these.

Finally, although this text may be well adapted to the immediate interests of the average college student, it does not necessarily follow that it is suited to students who are of superior abilities, or who expect to take advanced courses in psychology, or who for any reason might become interested in problems of an intellectual nature. The fact is that this book—and many others like it—fails to present any systematic or coherent account of human nature, to relate psychology to any other field of knowledge (as biology), or to present the fundamental laws and principles of psychology and of scientific methods. The authors probably are trying to "vitalize" the introductory course. They are mistaken, however, if they believe that the achievement of this end requires the abandonment of any effort to present psychology as an organized and synthesized body of knowledge.

Some may argue that students and laymen are the best judges of what is useful in psychology; that for alumni to determine the content of a college course is a praiseworthy extension of democratic principles; that items which they rank as useful can be adequately mastered without understanding of psychological fundamentals; and that the greatest of educational sins is to bore some of the students some of the time. The reviewer, however, disagrees with every one of the above contentions. He suspects that these doctrines, especially the belief that everything in education must be entertaining, exemplify a way of teaching and of being taught which is easy-going and self-indulgent, and lacking in individual and social discipline. He ventures to predict, furthermore, that within a short time these agreeable notions will be regarded as luxuries which even our American economy can no longer afford.

New York University

L. W. CRAFTS

Psychological Development: An Introduction to Genetic Psychology. By N. L. MUNN. Boston, Houghton Mifflin Co., 1938. Pp. xx, 582.

"Mind" or behavior is by its very nature spatio-temporal in character, and nowhere are the historical factors more clearly apparent than in the field conventionally known as "genetic psychology." Darwin, Romanes, Preyer, G. Stanley Hall, and a number of other early investigators were deeply concerned with "mental

development," both in its ontogenetic and phylogenetic or evolutionary aspects. The present volume well merits such a rich tradition, and aptly illustrates the tremendous strides made by psychologists within comparatively recent years.

Defining genetic psychology as "the scientific investigation of behavioral development," the author devotes the introductory chapter to a consideration of historical, taxonomic, and methodological problems. Mentalistic concepts are rejected, and the affiliations of an objective genetic psychology to comparative, child, abnormal, and social psychology, as well as to the physical, other biological, and social sciences, are emphasized. Chief emphasis is placed upon the problems of the phylogenesis and ontogenesis of behavior.

The biological foundations of behavior are discussed in Chapter 2. Not only are the usual morphological mechanisms of hereditary transmission considered, but the author also calls attention to the nature and significance of both intra-cellular and inter-cellular environmental conditions. The usual studies on heredity and environment are reported here and in Chapter 13 on the "growth of intelligence," the author adopting a conservative and eclectic attitude.

The following three chapters are concerned with the phyletic aspects of behavioral evolution. The phylogenesis of "unlearned behavior," including tropisms, reflexes, and instinctive behavior patterns, is discussed in Chapter 3. The adaptive character of such reactions in their relation to organic drives is emphasized. The author follows the current view in experimental genetics in accepting the mutational origin of new structures and consequent "unlearned behavior," although the operation of natural selection and isolation is not overlooked. Chapters 4 and 5 are devoted to a survey of the evolution of intelligent behavior in different animal forms, the former dealing with basic sensory, sensorimotor, and perceptual processes and the latter with learning and other complex processes such as those involved in concept formation, imitation, multiple choice, delayed reaction, double alternation, and coöperative behavior situations.

The ontogeny of human behavior is treated in the remaining eleven chapters. After an initial consideration of behavioral development from conception until birth and a discussion of the basic factors in infant behavioral development, the following topics are presented in their genetic aspects: sensory processes, spatially coördinated behavior, motor development, basic symbolic processes, language, intelligence, emotional behavior, and social behavior and personality. The final chapter considers the changes in personality from adolescence to senescence, an all too frequently overlooked stage of the developmental cycle.

The merits of the present volume are so many as to obscure almost completely any defects. The emphasis throughout is upon experimental material, and the experiments chosen are representative and clearly reported. The reviewer was also pleased to see the treatment of the initial and terminal stages of personality development, future work will doubtless place increasing emphasis upon the rôle of prenatal factors. It is interesting to note in this connection that in the discussion of prenatal conditioning (pp. 208-209) the author anticipates subsequent results, such as those of Spelt. The broad comparative approach which characterizes the volume is also commendable, as is the lucid style of presentation. Additional clarity for textbook use is achieved by the 109 illustrations and by the footnote documentation. Suggested readings are appended at the end of each chapter, and there is a comprehensive bibliography and a good index. Typography and layout are excellent.

There are few specific statements in the volume which the reviewer would seriously question, and these are of a relatively minor nature. The "Yerkes sensory discrimination apparatus" (p. 105), for example, is usually referred to in the psychological literature as the "Yerkes-Watson apparatus." What about the statement (p. 119) that "no careful experiments on tonal responses in infra-human primates have been reported" in the light of the experiments of Elder and of Wendt? What can be operationally meant by the distinction between "description" and "explanation" in the statement (p. 257) that "the various theories of learning . . . merely *describe the conditions* under which learning occurs; no adequate 'explanation' of learning (or description in neural terms) has yet been offered?" A more general shortcoming of the volume lies in its lack of integration; this is usually found in the case of purely eclectic presentations. In the opinion of the reviewer, additional light might have been shed upon the heredity-environment problem had the author taken a more critical attitude toward the use of terms as well as toward the reported methodologies of such experiments. Similarly, the psychoanalytic material (pp. 475 ff., 497 ff.) might have been treated more critically. The "criticisms" of the conditioned response theory of learning which the author cites (p. 256), furthermore, could have been readily answered both methodologically and in terms of empirical results. Lastly, the reviewer laments the failure to include comparative anthropological material throughout the volume. Surely, the widely differing stimulation imposed upon the developing organism in various cultures offers an excellent source of genetic material.

Notwithstanding such limitations, which can be found in almost any book, the present volume represents the most satisfactory textbook available in genetic psychology. In contrast to the recent trend toward popularized "give-the-student-what-he-thinks-he-wants" textbooks, the present volume represents a scholarly and interesting treatment of an important subject. The science of psychology needs more books like this one.

George Washington University

JOHN P. FOLEY, JR.

Modern Clinical Psychiatry. By ARTHUR P. NOYES. Second edition. Philadelphia, W. B. Saunders Co., 1939. Pp. 570.

This is the second edition of a book first published in 1934. The first eight of the 30 chapters deal with general considerations—nature of the mind and personality, mental mechanisms, general causes and symptoms of mental disease, and general examination of the individual from the standpoint of mental deviations. These chapters are essentially the same as in the old edition. They present on the whole a motivistic view of the problem of mental disease, though the viewpoint is not the strictly motivistic one fostered by the psychoanalytic school of thought.

Noyes does not neglect the importance of physiological factors. In fact his paragraph summarizing a discussion of the causes and nature of mental disease might suggest that his is a thoroughly eclectic viewpoint. "Psychiatry, because of the intangible material with which it deals, has been divided into various schools which have tended to a plausible oversimplicity in their consideration of the causes and nature of mental diseases. In all schools naïve reasoning has flourished. It must not be forgotten that as yet no one formulation, whether it be chemical, physiological, neurological or psychological, can explain all the phenomena observed in those personality disorders that we call mental disease. We must remember that much

of our talk about the unconscious, about organic lesions, and about disturbed chemistry is as yet hypothetical. In psychiatry we deal with the socially conditioned biopsychic life of the individual. And the biopsychic life, it is obvious, is subject to an incalculable variety of influences."

Following the general subjects treated in the first part of the book there are twenty-one chapters dealing in a systematic way with the various mental diseases. The chapter titles and the order of presentation follow the classification of mental diseases adopted for official use by the American Psychiatric Association in 1934. In his earlier edition, Noyes reversed this order of presentation on the assumption that a broader view of mental disease would be secured if a consideration of the disorders associated with structural and toxic diseases of the brain and with somatic diseases were deferred until after a consideration of the other psychoses. Noyes still believes that the American Psychiatric Association classification and order lack a dynamic approach to personality disturbances and fail to stress the important relationship between psychological and psychobiological factors common to the behavior and personality reactions of everyday life and to what we know as mental disease.

The new edition contains some desired changes in chapter titles, as dropping of the designation "General Paralysis" for the major neurosyphilitic conditions, and use of "Psychoneuroses" instead of "Minor Psychoses" to designate a recognized group of disorders. The content has been brought up to date, especially with regard to some of the newer treatments in the field of psychiatry. For example, the chapter on dementia praecox has an added several pages presenting an excellent summary of "shock" treatment of that disorder, a development largely of the past few years. Added references make the bibliography more up to date. The new edition also has a new chapter on "Psychology and General Medicine"—a feature which should increase the usefulness of the book to those physicians outside of the specialty of mental disease.

As was true of the earlier edition, the book is fluently written, well organized, and arranged from the standpoint of textbook standards. It is generally accurate with regard to facts, shows good selection of material on the topics considered, and is interesting to read. The presentation of illustrative hospital cases from the author's experience adds to its interest.

From the standpoint of the psychologist the book is of interest for its general and theoretical discussion of the nature of mental disease and for its systematic presentation of the features of the major psychoses. From the standpoint of the psychologist and possibly also from that of the general medical person it is notably lacking in presentation of the less severe aberrations and maladjustments which form so important a part of the field of interest. Many psychologists may find the book too medical in flavor and manner of presentation, but it can well be recommended as a valuable reference book to anyone interested in the field of abnormal or clinical psychology.

George Washington University

THELMA HUNT

The Plans of Men. By LEONARD W. DOOB. New Haven, Yale University Press, 1940. Pp. xiii, 411.

There was a time, and that not long ago, when each discipline jealously guarded its frontiers. This was especially true of the social sciences. But now the sociologist

is turning psychologist and the psychologist, sociologist. True to this trend *The Plans of Men* is written by a psychologist who treats of an extremely important sociological problem, planning.

Doob accepts a relativistic view of ethics. He holds to this with reasonable strength although occasionally value judgments creep in. Words such as "enlightened," "not desirable," and "good" appear as well as condemnations of fascism and of "Mister" Coughlin. Yet the reader must not become too disturbed as no readable and vital book can be written without at least a few value judgments.

Six chapters are devoted to the four phases of human activity—the biological, the social, the political, and the economic. The first two phases are excellently handled; the others are less so. In his treatment of the economic Doob felt it necessary to restrict himself "to the schools of thought known as the classical, neo-classical, marginal utility, and Cambridge variety" (p. 96). This limitation was most unfortunate as the adherents to at least one other school have had a great interest in social planning.

Following the section on activity comes one on individual, social, economic, political, and regional planning. The thesis is expressed that "it is human nature . . . to seek the maximum of gratification and the minimum of frustration; a desirable value should be directed toward these interrelated ends" (p. 156). Doob clearly sees the weaknesses in the hedonistic hypothesis however it is phrased and realizes that accurate hedonistic calculations of gratifications and frustrations are impossible. Yet he can find nothing to replace the philosophy of hedonism.

Taking Walter Lippmann as his adversary, the author makes it clear that we always have had and certainly will have in the future planning of some sort. Planning overcomes certain frustrations, but necessarily induces others. No completely unfrustrated man has ever existed; and it is very doubtful that a complete absence of frustration would give the type of behavior the American people want.

The impression is given that the author is favorable in the main to the New Deal planning. Numerous bouquets are tossed its way. With somewhat doubtful logic Doob maintains that: "The fact that these [New Deal] measures have met with strong approval and disapproval demonstrates that they have solved some economic and social problems while creating others" (p. 275). One wonders whether or not the author would now modify his sections on fascism and communism (the preface is dated October 30, 1939). The habit of price fixing is seen as essentially the only similarity between these ideologies. His mention of Russia's Finnish land grab and sly questions now and then make it appear that he is probably stressing the theoretical and not the practical differences. Yet he is much gentler with the obvious disadvantages of Stalin's planning than with those of Hitler's.

Doob ends his book with three chapters of "Perplexing Problems." Here, among other matters, he discusses the ability to plan, techniques, trends and planning personalities. He gives almost no answers except a few which touch regional planning. In fact he could hardly be expected to offer answers and yet remain scientific. Where, then, lies the utility of the book? Its function, as the reviewer sees it, is to combat absolute standards and to demonstrate to the intelligent but relatively unread person (if such an individual can be persuaded to read almost four hundred pages) the limitations inherent in any social plan.

While the style of *The Plans of Men* is a little repetitious and possibly not as readable as that of Doob's other works, the book should be read by all social

scientists. It is without question a credit to the Institute of Human Relations from which it was published.

Stanford University

PAUL R. FARNSWORTH

The New England Mind. By PERRY MILLER. New York. Macmillan Co., 1939. Pp. xi, 528.

This is an excellent book, but not for what the psychologist wants of it: Although the author should not be criticized for failing to write the reviewer's book, here, at least, it must be treated from the viewpoint of serviceability to social psychology and to cultural anthropology.

This infinitely piled fabric is the first "volume in a projected series upon the intellectual history of New England." Although the title speaks of "mind" it would have done better to have spoken of "soul;" for in the main the work is a prodigious piece of theological archaeology on 17th century New England. Miller likes his subject and pushes on implacably to provide the ideological superstructure of its justification. His overpowering erudition produces awe and respect for itself, not for the dismal cosmology and ethics which he traces affectionately through all their refinements and permutations. From his saturation with this theological lore, he has created a legend which may be as misleading as Mencken's was in the opposite direction. The work is carried to such a conspicuous pitch of virtuosity as to leave the impression of a scholarly mash note to Puritanism. It is still to be doubted that the sullen vehemence of Puritan theology or the dreary monologue of a Bunyan, whom for some droll reason Miller regards as a norm of greatness in literature, was more significant to life than, say, bundling. With a notable determination to delude himself, the scholar fails to see that the impossible contradictions and absurdities of this theology, far from being essential to the ordinary man, had to be discarded and ignored if he were to live at all.

What would be significant to the psychologist is not the theology of a Puritan divine, which Miller gives with the utmost generosity, but what it meant to the ordinary man and to what extent he took it seriously. It must have been very hard, on week-days, for the Puritan farmer to see an arresting problem in the contradiction between Calvinistic predestination and the necessity of "works," for example. Yet Miller's colossal work exhumes the bones of this troglodyte and painstakingly fits them together to simulate a viable organism. This is symptomatic of the intellectual exercise which has produced a Puritan Ruritania rather than a scheme of life which could have had actual functional significance to living people. On a priori grounds it is permissible to doubt, as Miller does not doubt, that theology could have been the blueprint of life in 17th century New England; for had it been otherwise, the true believer would have been hung up in a state of permanent abulia, which was certainly not the case with the energetic exponents of "works" that inhabited New England. It is a libel against the solid good sense of the New England Puritan that Miller does nothing to emphasize the practical non-existence of the contradiction between predestination and "works" anywhere but on paper. This kind of symptomatic theological flight from reality could have been the stuff of mind of none but morbid theologians who influenced people with human juices but little. None but schizophrenics, which the New Englanders were admittedly not, could have lived and worked in the nightmare of these theological torments. Hence, the mind, the operational motives, which Miller does not even guess at,

still remain to be disentangled. The psychologist must refuse to believe that these exhumed fossils of the fluctuations of human absurdities, these swollen theological contradictions, were the actual motive forces by which a capable people had lived.

In his relentless pursuit of these theological nightmares, Miller challenges nothing, imposes no new values in the light of our own significant experiences on the old material, and ends nowhere, failing to suggest that the ordinary Puritan, who was canny enough, saw through and circumvented the morbid swindle of his theologians.

University of Tampa

ELLIS FREEMAN

Comparative Psychology of Mental Development. By HEINZ WERNER. Translated by E. B. Garside. New York, Harper & Bros., 1940. Pp. xii, 510.

This is a reworked first English edition of a volume which appeared in two German editions, while the author was still permitted his professorship of psychology at the University of Hamburg. Since coming to America, he has lectured at Harvard and at the University of Michigan and has conducted researches at the Wayne County Training School (a Michigan school for the feeble-minded). E. B. Garside has made a readable translation. G. W. Allport supplies a foreword, which classifies the book as part and parcel of the *Gestalt* revolution.

The approach is definitely organic rather than mechanistic or reductionistic. For the author the essence of development is "a steadily increasing differentiation and centralization or hierarchic integration, within the genetic totality." He envisages this essence as a single general law which is framed by five paired concepts; namely, (1) syncretic-discrete, (2) diffuse-articulated, (3) indefinite-definite, (4) rigid-flexible, (5) labile-stable. In an orderly, almost regimented manner, these concepts are applied to the manifestations of the child mind, the primitive adult mind, and to psychopathology, particularly schizophrenia. Major attention is paid to the first two concepts. The syncretic function is adduced whenever "several functions or phenomena which would appear in a mature state of consciousness are merged without differentiation into one activity or into one phenomenon." The concept of diffusion is referred to the formal structure of the mental content.

The text is almost encyclopedic in range as well as arrangement. There is a wealth of scholarly detail; 751 references are cited, many repeatedly. Anthropological literature was finely combed for illustrational material. The author's approach is freshest when he applies the results of his own experimental studies in perception and learning, and those of his colleague, Martha Muchow, who productively investigated the mentality of children in the Hamburg laboratory before her untimely death.

Having premised the universality of the syncretic, diffusional, and other principles of mental organization, the author systematically points out parallelisms and identities in animal and child, primitive and aberrant mental processes—in perceptions, notions of time and space, motivation, conception, reasoning, magic, spheres of reality, and structure of personality. This comparative method imparts an exhilarating sense of the presence of all-pervading natural law, and it certainly suggests the ultimate possibility of a comparative science of mental development. The method, however, also has its hazards. It frequently leads to an almost anecdotal citation of children's sayings, and an illustrational rather than inductive utilization of concrete observation. The analogies between schizophrenic and child thinking, for example,

may have a precarious foundation, as recently revealed by Dr. J. Louise Despert who found that experiences which show the closest resemblance are dependent upon emotional factors rather than upon characteristics inherent in child thinking.

This book may be read for its varied suggestiveness. One will not find a forward moving ontogenetic account of the developmental process. The chapters deal topically with the dynamic organization of mental states rather than with the substantive progressions of morphogenesis. The author relies heavily on the powerful generality of a few explanatory principles, and does not undertake to picture the life cycle sequences of infancy, childhood, adolescence, maturity and senescence. Professor Werner, however, provides an excellent *Gestalt* outlook on an expansive domain—an outlook which will prove stimulating to the cultural anthropologist, and to the student who is too narrowly preoccupied with the laboratory analysis of restricted areas of human behavior.

Yale University

ARNOLD GESELL

The Startle Pattern. By CARNEY LANDIS and WILLIAM A. HUNT. With a chapter by Hans Strauss. New York, Farrar & Rinehart, 1939. Pp. xiii, 168.

In writing *The Startle Pattern*, the authors have gathered together the material which they had previously published (with various collaborators) and added new material to give a comprehensive account of the phenomenon which they have been investigating for some years. The book gives them an opportunity to provide a synthesis and interpretation of their numerous experiments which should prevent their becoming lost in the outpouring stream of disconnected research reports.

Not the least of the authors' contributions is their successful use of high speed motion picture recording in a movement study. Their camera was capable of running at the rate of 3000 frames per sec., though illumination problems restricted most of their work to speeds under 500 per sec. The study, however, also makes clear that motion pictures do not solve all the technical problems of movement-study, for though photography records, it does not qualify behavior. For this purpose Landis and Hunt made careful use of a rating scale, which, they would no doubt agree, is too subjective to be ideal.

They found the startle response to be a highly stable pattern reaction which can be evoked by strong, sudden, stimuli of various sense modalities. It is present in adults, infants of six weeks and in all the mammals tested. The response begins with the eyelids after a latency of 40 ms. and thereafter progresses downward through the organism as a flexion movement. Fairly wide individual differences in completeness nevertheless exist. The response is modified by habituation, facilitation, expectation, posture, conditioning, hypnosis, epilepsy, catatonia, and metrazol. There are apparently no effects resulting from voluntary attempts at facilitation and inhibition, voluntary attempts at controlling muscular tension, adrenalin, and abnormal states other than those just mentioned.

A point of considerable theoretical interest developed from the study of the "secondary behavior" which may follow the startle pattern. This subsequent reaction is found to be no modification of the original reflex, but a rather variable addition to it.

The chapter contributed by Strauss is a neuro-anatomical theory of the reflex. Strauss concludes that the behavior results from two reflex mechanisms, one governing the eyelid, and the other the body flexions. Since the flexion pattern is very

much like the "pallidum syndrome," he infers that the same mechanism, *i.e.* hyperactivity of the nucleus ruber, must be responsible.

For some purposes one might wish for more formal presentation and analysis of data. Probably this was omitted partly because of the nature of the data. But where there is a question of more or less of the response, it is less assuring to rely on the authors' observation of trends than on tables and statistical comparisons. The book leaves one with the impression that such detailed study would be fruitful of results if applied to other emotional responses and "expressive movements."

Indiana University

R. C. DAVIS

New Ways in Psychoanalysis. By K. HORNEY. New York, W. W. Norton Co., 1939. Pp. 313.

Much has been written recently in academic journals in an attempt to evaluate Freud's work. The judgments are not at all in complete agreement, but this was to be expected, for Freud's theories are still in the process of experimental examination. It is, however, clear that psychoanalysis is here to stay. This does not mean, of course, that psychoanalysis will continue to exist in the narrow boundaries of Freud's own conceptions. Modifications and elaborations of his theories were in process while he was still alive, and this tendency towards change is going on. Dr. Karen Horney is one of the latest researchers to break away from the narrow concepts of Freud and set out on an elaboration of psychoanalysis based on a thorough knowledge of the subject backed up by 15 years of actual experience in the field.

Dr. Horney takes exception to Freud's conception of psychoanalysis as "an instinctive and a genetic psychology" (p. 8). She thinks that psychoanalysis is unnecessarily bound by this narrow conception, and that as dogmatism is removed new vistas will open to the discerning psychoanalyst. Dr. Horney, proceeding on this assumption, sketches the fundamental principles of psychoanalysis, gives a short account of the more important premises of Freud's thinking, and then gets down to the job of analysing and revising the now classical concepts of psychoanalysis, including the libido theory, the oedipus complex, the concept of narcissism, feminine psychology, the death instinct, childhood, the concept of transference, culture and neuroses, the "ego" and the "id," anxiety, the concept of the "super-ego," neurotic guilt feelings, masochistic phenomena, and finally psychoanalytic therapy.

In her analysis of these concepts, Dr. Horney stresses the hasty generalizations made by Freud. These generalizations, Dr. Horney points out, are frequently based on good observations but false logic. This sort of thing is unavoidable when dealing with psychopathic subjects, and Dr. Horney herself is not free from it. The mistake that is made time and again by psychoanalysts is that human nature is regarded as a thing about which arbitrary laws can be made. The fact that certain actions follow as the result of certain human conditions at one time, does not, however, in the least assure their following at some other time, because these conditions cannot be kept constant.

Dr. Horney is to be congratulated on her courage in differing with Freud. For the psychoanalyst who is deeply imbued with Freudian dogma, this book can be a revelation; for the general psychologist it pleasantly enhances what he already knows about psychoanalysis; for the student it is a wonderful example of the continuity of knowledge. It is a human failing that once we have established what we think are the ramifications of a fact, we want to leave it alone and question it no more. But

the future of truth demands a questioning mind, and it is to such a mind that Dr. Horney's work makes a particular appeal. Some books ask more questions than they answer. Dr. Horney's is such a one. Thus, anyone who is at all interested in opportunities for research is certain to be delighted with *New Ways in Psychoanalysis*.

University of Manitoba

PETER HAMPTON

A Textbook of Psychiatry. By ARTHUR P. NOYES and EDITH M. HAYDON. Third edition. New York, Macmillan Co., 1940. Pp. x, 315.

This is the third edition of a standard text for psychiatric nurses, rewritten and brought up to date by including, for example, discussion of the various forms of shock therapy. Though the psychiatric nurse is associated in our thinking with mental hospitals and outpatient psychiatric clinics, the authors hold that "in many respects the greatest field for psychiatric nursing is not the mental but the general hospital."

Chapters on the motivating sources of human behavior, conscious and unconscious mental processes, mental mechanisms and motives, and personality types lay the foundation for understanding the behavior of the psychotic patient. The authors stress that this behavior is no more a random causeless affair than the behavior of the person we call normal. In some cases the cause is obvious, and the patient is quite conscious of it. For example, he refuses to eat because he knows that the food is poisoned. In other cases the cause may be known to the physician, but the patient is unconscious of it. In many cases no one knows the cause. This is, however, true of many reactions of non-psychotic persons. Many a person has a strong dislike for his given name, but why he dislikes it he does not know. Though we do not doubt that there is a cause for this, as for other personal antipathies, we cannot tell him. "The more fully the nurse can appreciate the complex influences and forces that produce, now obvious mental disorder, now less dramatic results in the form of vague physical invalidism, embittered social attitudes, moods, dissatisfactions, and feelings of inferiority, the less she will be a mere mechanical technician attending to the physical wants and comforts of her patient and the more fully and intelligently will she realize the total needs of the individual and be able to minister to them."

The discussions of the different psychoses which follow are systematic and practical. "Nursing management" is a topic in each chapter. Some well chosen illustrative case histories are given briefly, but often a terse sentence tells the story. For example, in a discussion of the manic-depressive, we read: "Ideas of reference and illusionary falsifications are common—a pounding in the basement is that of workmen constructing the patient's coffin." There is an excellent glossary of nearly 200 definitions. These are brief and to the point; about as non-technical in their wording as is possible in defining technical terms.

The authors' style is clear throughout and the book should be read widely by teachers, social workers, and graduate nurses, as well as by student nurses. Most criticism will be commendatory. Psychoanalysts will probably find the discussion of their therapeutic principles and practice inadequate and will resent the faint praise accorded them. Someone will smile at the authors' naïve acceptance of Archbishop Usher's date for the quotation from Deuteronomy or their reference to the "English-speaking language," but will render a favorable opinion of the book just the same.

Ohio State University

FRANCIS N. MAXFIELD

An Orientation in Science. By C. W. WATKEYS and associates. New York, McGraw-Hill Book Co., 1938. Pp. x, 560.

This is a textbook for a survey course in science. Such a course, the authors hopefully aver, "acquaints the student first of all with the scientific method of thought and procedure; secondly, it gives him a bird's-eye view of the principles and problems underlying various branches of science; thirdly, it permits him to see the relation between one field of science and others." While all of us would regard as praiseworthy an attempt to provide the student with these things, there are many of us who would reserve doubts concerning its success. Your reviewer had such doubts. A reading of this volume has not allayed them.

The major difficulty concerns the student's understanding of the scientific method of thought and procedure. The authors have attempted to convey this in a final chapter on scientific method. This chapter is well-written and well-conceived. Unfortunately, however, the other chapters are crammed with so many "scientific facts" that there is little room for an examination of the basic principles of procedure in the various fields. A list of these chapters is impressive, if somewhat staggering. We find a discussion of physics through the cosmic ray; of chemistry through the architecture of covalent molecules; of mathematics through the calculus; of physiology; astronomy; geology; biology; bacteriology; paleontology; and psychology. Most of the chapters are clearly and interestingly written, but the employment of different authors and the insistence upon up-to-date fact have resulted in a discouraging lack of coordination.

An examination of the chapter on psychology, particularly in comparison with the thoroughness of the others, is likely to give the psychologist some unhappy moments. Experiments discussed are those on reaction-time, Weber's Law, memory and forgetting, the conditioned response, and the measurement of intelligence. Nothing is said of the changing concepts of perception, of the many facts and theories arising from the study of skills and habits, of the concepts of drive and motive, of the effect of Freudian theory upon classical psychology, nor of the entrance of psychology into industrial fields. The matter included seems not always well-chosen nor even thoroughly understood. For example, the author devotes 2 pages out of 35 to the use of reaction-time studies in the development of tests of driving skill, but neglects to mention that evidence for a relationship between such tests and highway performance is lacking.

One is inclined to suspect that this book will "sell" students on the sciences but will leave them woefully naïve with respect to the constructs which the scientist employs and the limitations of those constructs.

Tulane University

S. RAINS WALLACE

Human Development and Learning. By FRANK S. SALISBURY. New York, McGraw-Hill Book Co., 1939. Pp. xvii, 513.

This is "a beginner's book for students who wish an understanding of psychology as it applies in the school and the home." The author has made a courageous, if often futile, attempt to interpret the data of educational and child psychology in terms of everyday life. He is aided by an ability to write simply and clearly, and by an unusual willingness to state a general theoretical viewpoint (the organismic) and to cling to it throughout the book. There results a consistency and continuity which is often lacking in our elementary texts.

The outstanding chapters of this work are those which emphasize the importance of the physiological mechanisms underlying behavior. Here Salisbury has produced a remarkably good exposition concerning the development of structures and functions within the neuro-muscular system. When he begins to apply these principles, however, and to bring them into the home and school, the results are not so successful.

One is led to suspect that the students who read this book are likely to be imbued with some grandiose notions concerning their own knowledge of the field. The author's desire to maintain his theoretical position while striving to arrive at the happy hunting ground of everyday life, leads him to make bald statements which seem meaningless at best and which often indicate an alarming naïveté with respect to the concepts which he employs. To quote a few examples: "The babe sees with little purpose but even the babe sees purposively;" "his vital thalamic-visceral organization protests against subservience;" "The very complexity of modern life tends to excessive new-brain dominance."

The reader often gains the impression that the facts are being trimmed to meet the demands of applicability and consistency. The numbers confined in institutions for the mentally unbalanced are said to be rising, but no indication is given that the incidence rates have remained relatively constant. Köhler's relational choice is upheld with bland failure to mention experiments which have indicated that the concept is not quite so simple as some have supposed. Finally, the methods of so-called progressive and modern education are treated with something akin to awe; the critical reader finds them not justified on the basis of evidence given by the author, but accepted simply as a *fait accompli*.

In short, this book includes the many defects and the few advantages which are found in modern texts of the "popular" type. For a group of elementary school teachers who "need three hours" and who may be expected to learn more from candid snapshots of children at work and play than from graphs and tables, it might prove adequate.

Tulane University

S. RAINS WALLACE

Knowledge and Character. By MAXWELL GARNETT. New York, Macmillan Co., 1939. Pp. xii, 358.

The author, who identifies himself as a barrister-at-law, has attempted to give thinking a neurological setting. He frankly adopts interactionism, following McDougall, but it is doubtful that his position will appear convincing to many American psychologists. Nor are they likely to become greatly excited about "neurograms," "neurographies," "realms of fact," "endarchies of knowledge," "interest systems," "work systems," and "master-purposes." It is questionable whether anything is to be gained by a transfer from one set of descriptions to another. For example, it has undoubtedly been suspected for some time that "disputes between two or more people are often due to ignorance" but does it add much to our understanding to say that it is due "to the imperfect correspondence of their neurographies with the realm of facts and therefore with one another" (p. 228)? Again, the principle that we "learn by doing" has had wide acceptance in educational circles, but is anything gained by saying the value lies "in the deepening of a neurogram that results from its excitement spreading to the motor nerves" (p. 263)?

The statement, referred to as a law, that "Will can reënforce the excitement in any active neurogram" (p. 123), and again that "will" by "its intervention makes

new paths in the thinker's brain" illustrates the fact that "will" has been raised to the status of an entity. Or when we read that "curiosity urges men to discover the endarchy of knowledge" (p. 247), we wonder whether curiosity has not become a thing in itself rather than a way of behaving. One is inclined to believe that the author has a vein of mysticism in his thinking when statements are encountered such as "the mind is neither the soul nor the body but the meeting place of both" (p. 120), "there is no reason why divine inspiration should not operate through 'curiosity-wonder'" (p. 251), and "the master-purpose in a single wide interest must concern what lies beyond our present bourn of time or space" (p. 281).

The book is well documented—only 24 pages in 337 are without footnotes! Several chapters contain a very acceptable summary of modern neurological research. Those well trained in psychology and education will not find much new material but rather should look upon the book as a record of the viewpoints of this particular author. It does not seem that restatement of education and psychology in terms of hypothetical "neurograms" is likely to "bring order in educational chaos."

Iowa State College

MARTIN F. FRITZ

Personal Aggressiveness and War. By E. F. M. DURBIN and JOHN BOWLBY. New York, Columbia Univ. Press, 1939. Pp. viii, 154.

With the hideousness of war so chronically present in recent years, and with its control so apparently impossible, it was inevitable that the problem would eventually be examined by psychoanalytically oriented social psychologists. It could hardly have been hoped, however, that the job would be done as well as Durbin and Bowlby have done it. The basic hypothesis of the book is that personal aggressiveness is a product of frustration and that war as a social movement is dependent on the dammed up aggressions resulting from childhood punishments (frustrations) and the exigencies of competitive social living. Two cures are suggested: the reduction of childhood frustrations and the prevention of war by international policing. The former is admittedly a slower and more tedious therapy than the latter, according to the authors, but offers the only permanent solution.

Such a brief summary does obvious injustice to the authors' careful analysis of such mechanisms as identification, displacement, and projection which are used as theoretical bridges from the individual to the societal behavior. It neglects also the neat and accurate logic by which significant social conclusions are derived from simpler psychological assumptions. These assumptions are very similar to those presented in *Frustration and Aggression* (Yale University Press, 1939) and even the most casual reader would have little difficulty in translating the reasoning of *Personal Aggressiveness and War* into the somewhat more academic terminology of *Frustration and Aggression*.

Students of learning and conditioning will be interested, further, to discover that the principle of generalization of conditioning is adopted as a necessary basic assumption ("It is just a fact of human behaviour that cannot really be deduced from any general principle of reason." p. 10). The term *generalization* is not used, to be sure, but the concept here makes its first clear-cut entry into the writings of a psychoanalyst.

An unsympathetic critic might point out that sharp and rigid definitions are lacking and that the hundred pages of documentation of the frustration-aggression relationship include but two main sources, Zuckerman's apes and Susan Isaac's school

children. The definitions are adequate for their purpose, however, and the importance of further documentation is a matter for argument. The chief virtue of the book is its success in breaking down the superficially obvious connection between personal aggressiveness and war and showing the exact mechanics of that relationship.

Yale University

ROBERT R. SEARS

The Measurement of Adult Intelligence. By DAVID WECHSLER. Baltimore, Williams & Wilkins Co., 1939. Pp. ix, 229.

This book describes the construction and standardization of the Wechsler-Bellevue Intelligence Tests, and incorporates the manual of instructions. There are 10 regular tests and an alternate: information, comprehension, digit span, similarities, arithmetical reasoning, vocabulary (the alternate), picture arrangement, missing parts, block design, object assembly, and digit-symbol. Each test has items running from easy to hard, and the series is arranged as a point-scale. The first five tests and the alternate constitute a verbal scale; the last five a non-verbal scale. The tests appear to have been reasonably well selected, and the directions are clear. The scale yields standard scores in arbitrary units roughly comparable to *IQs* ($M = 100$, $PE = 10$, $\sigma = 14.8$, at each age). Nearly all the cases used in standardizing the scale were obtained in and near New York City. The method of selection probably gives approximately correct *averages* at the various ages, but there is some evidence that the *variabilities* were not well controlled. As a result the interpretation of the *IQs* is somewhat doubtful except where they are close to 100. On the other hand, adjustments for changes in average intelligence from 16 to 60 remove one major cause of error in previous scales.

The work is marred by numerous statistical errors. In attempting to insert a theoretical foundation under what is obviously an empirical structure the author bogs down badly in factor theory. He confuses the standard error of the mean with the standard error of estimate (p. 135), proposes to measure the mental deterioration (with age) of individuals by a method that seems logical (p. 65) but ignores the fact that the tests would need to be much more reliable than any now in existence, computes coefficients of variation from scores based on an arbitrary zero-point (p. 123), and assuming that standard scores are scaled scores, presents a skew distribution of *IQs* (p. 129) which probably proves only that the tests possess inadequate numbers of hard items.

Since the empirical work is apparently good and the scale fills a real need, it will probably be used widely during the next few years in spite of its technical shortcomings.

Alabama Polytechnic Institute

EDWARD E. CURETON

The Measurement of Abilities. By P. E. VERNON. London, Univ. London Press, 1940. Pp. xii, 308.

This is a fairly elementary book written primarily for teachers, but also directed, according to the Preface, to examiners, clinical workers, school medical officers, and psychology students. Assuming no previous knowledge of statistics or psychology, it gives the reader a general survey of the most common statistical measures, with many hints to facilitate computation. The book also contains an annotated list of the chief intelligence and educational tests published in Great Britain, together

with a more general summary of sensory-motor, special aptitude, and personality tests. The construction and use of tests, as well as the critical interpretation of test scores and statistical data are given detailed consideration. Approximately one-third of the book is devoted to an intensive discussion of school marks and examinations, treating such topics as the equating and combining of marks, the relative merits of "essay-type" and "new-type" examinations, the different forms of "new-type" questions, the principles of construction of "new-type" examinations, and practical suggestions for improving examinations. Thus the book overlaps the fields of statistics, mental testing, and educational psychology, its boundaries being dictated by the needs of a particular group of individuals rather than by traditional compartmentalization of facts.

The author has succeeded admirably in combining simplicity of expression and a completely practical point of view with scientific accuracy and a sound conceptual basis. The book is up-to-date in its factual background, and critical and cautious in pointing out implications. In discussing factor analysis, for example, the author writes: "Factors are not entities in the mind whose nature or constitution, and whose strength or weakness, are immutably fixed (p. 160) . . . factorial investigations are *not* revealing the elements out of which the mind is compounded, but are classifying test performances with a view to predicting more effectively other performances, e.g. in the educational or vocational sphere" (p. 169). If the data of psychology are to find immediate practical application, it would seem that this type of book is a much more effective means of accomplishing this end than those books which rely on extraneous and spurious motivation to attract the largest number of readers, by arousing grandiose expectations or by appealing to the desire to look at pictures and read jokes.

Queens College

ANNE ANASTASI

Principles of Psychiatric Nursing. By MADELENE ELLIOTT INGRAM. Philadelphia, W. B. Saunders Co., 1939. Pp. xviii, 428.

Not many psychologists will read this book, but more of us who are teaching abnormal psychology should do so. It teems with the life and reality of the mental hospital, and gives an intimate picture whose value is second only to that of direct practical experience. We learn what to do when a patient tries to hang himself, and read the specific intricacies of therapy all the way from sitz baths to metrazol convulsions. There is much to enrich the teacher's often too abstract acquaintance with abnormal psychology.

For its original purpose, the book seems to be a well conceived and organized text, based on a job-analysis of psychiatric nursing that is both painstaking and intelligent. Unlike some of its contemporaries, this text does not attempt to be a miniature treatise on psychiatry. Very little space is devoted to theory, for which the student is quite properly referred to other sources. The book's viewpoint seems to be that of psychobiology, although free from Dr. Meyer's esoteric terminology.

Ingram opens with a brief section on the history of the care of the mentally ill. This unit is not profound, but is interesting, and is well illustrated with reproductions of old engravings. Part II deals with practical problems of psychiatric nursing, including care, management, hazards, feeding, and the more routine forms of therapy. Part III briefly describes the chief reaction-types met in mental hospitals, and illustrates them with case studies. In this section are described the newer forms of

therapy. Part IV, concluding the book, is concerned with the care of mental patients outside of the hospital, including problems of travel, and a note on mental hygiene. The entire book is clearly and interestingly written, and is sufficiently detailed and specific to serve as a reference work. Teaching aids include chapter summaries, questions, references, a glossary, and an adequate index.

Carnegie Institute of Technology

LAURANCE F. SHAFFER

Selected Writings in Philosophy. Compiled by G. P. ADAMS, W. R. DENNES, J. LOWENBERG, D. S. MACKAY, P. MARKENKE, S. C. PEPPER, and E. W. STRONG. New York and London, D. Appleton-Century Co., 1939. Pp. xvii, 355.

The purpose of this book is not "to provide a comprehensive anthology but rather to illustrate the various themes discussed in the companion volume, *Knowledge and Society*." The latter volume, by the same authors as the present volume, was designed as "a philosophical approach to modern civilization." Readers of *Knowledge and Society* will know how very successful the authors were in demonstrating the relevancy of philosophy "to the major tasks of civilization" and how effectively the approach to contemporary cultural problems can be used as an introduction to philosophy.

The present volume is divided into two parts. Book I, Knowledge, contains selections concerned with the development of science out of common sense thinking and of metaphysics and religion out of science, with the problem of the freedom of the will, and with the part that skepticism plays in the search for certainty. Book II, Society, deals with ends and values, the selections being intended to illustrate the theoretical as well as the practical approach to the basic problems of individual and social morality and social organization. To this reviewer, the selection seems to have been expertly made and the book is well suited for the purpose the authors have in mind. In addition, the selections constitute a valuable anthology for the student or layman who wishes to begin a course of reading in philosophy.

Tulane University

MARTEN TEN HOOR

Art's Endurance. By THEODORE L. SHAW. Boston, Bruce Humphries, 1939. Pp. 249.

"Everything in life is art," and pleasure "is the measure of art." Pleasure has two "directions," simplicity and complexity. "Are there some other words or phrases," asks the author, "which will do more for us than 'pleasure' does? I allege that there are; and that they include the following: 1. That which tends to put man's nervous system into balance; 2. Rareness; 3. Untiredness-of; 4. Differenceness" (p. 15).

With such "words and phrases," together with a number of graphs, unintelligible to the reviewer, the author discusses what he chooses to call "Art's Endurance." Whatever Art's Endurance may be, the endurance of one reader has not sufficed to gain an understanding of it.

Cornell University

R. M. OGDEN

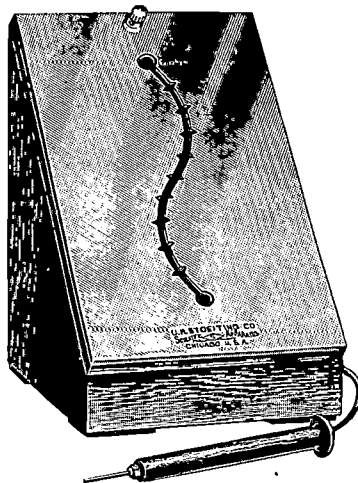
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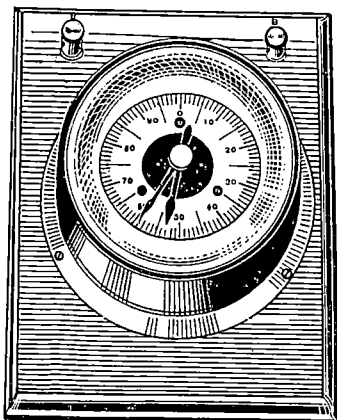
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No. 2

RETROACTIVE AND PROACTIVE INHIBITION IN RETENTION:
EVIDENCE FOR A TWO-FACTOR THEORY OF
RETROACTIVE INHIBITION

By ARTHUR W. MELTON, University of Missouri, and W. J. VON LACKUM,
Stephens College

In a recent study of the relationship between the amount of retroactive inhibition (*RI*) and the degree of learning of the interpolated activity, Melton and Irwin¹ found reason to question the theory of *RI* which attributes all of the inhibition to a competition of the original and interpolated responses at the time of attempted recall and relearning of the original responses.² It was shown that the degree of learning of the interpolated activity was differently related to the amount of *RI*, as measured by recall and relearning scores, and to the frequency of overt intrusions of interpolated responses during the recall and relearning of the original responses. On the assumption that the frequency of such overt intrusions serves as a measure of the amount of competition between the original and interpolated responses during recall and relearning, it follows that

* Accepted for publication September 25, 1940.

¹ A. W. Melton and J. McQ. Irwin, The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses, this JOURNAL, 53, 1940, 173-203.

² Cf. J. A. McGeoch, Studies in retroactive inhibition. I. The temporal course of the inhibitory effects of interpolated learning, *J. Gen. Psychol.*, 9, 1933, 24-43; Studies in retroactive inhibition. II. Relationships between temporal point of interpolation, length of interval, and amount of retroactive inhibition, *ibid.*, 9, 1933, 44-55; J. A. McGeoch and G. O. McGeoch, Studies in retroactive inhibition. VI. The influence of relative serial positions of interpolated synonyms, *J. Exper. Psychol.*, 19, 1936, 1-23; and Fred McKinney and J. A. McGeoch, The character and extent of transfer in retroactive inhibition: Disparate serial lists, this JOURNAL, 47, 1935, 409-423.

one or more factors other than the competition of these specific learned responses must be determiners of the measured *RI*. Evidence for the overt intrusion of original responses *during the learning of the interpolated responses* suggested that an unlearning of the original responses occurs at the time of the interpolated learning, and that this is one determiner of the amount of *RI*. Since the competition of responses at the time of attempted recall cannot be gainsaid, the acceptance of this hypothesis of active unlearning implies a two-factor theory of *RI*. One factor has its locus in the process of learning the interpolated activity (the 'unlearning' factor), and the other factor has its locus in the recall and relearning processes (the 'competition' factor).

The present study was designed to provide a more decisive test of the value of this two-factor theory for the explanation of obtained amounts of *RI*. A specific deduction from the two-factor theory, and one which runs counter to the expectation based on the single-factor 'competition' theory, concerns the relative retention of equally learned original and interpolated activities after an interval of rest which is approximately the same for both. If the competition of responses at the time of recall and relearning were the only determiner of the *RI*, the recall and relearning of the interpolated (second) activity should be impaired by the competition of the original responses as much as the recall and relearning of the original responses is impaired by the competition of the interpolated responses. That is, the proactive inhibition (*PI*) of the retention of the interpolated activity should equal the retroactive inhibition (*RI*) of the original activity, because the temporal order of learning of the original and interpolated activities is given no significance in the strict reproductive inhibition, or competition, theory of *RI*.

In contrast, the two-factor theory assumes an active unlearning of the original responses at the time the interpolated responses are being learned, and no such unlearning is supposed to occur in the case of the interpolated responses. As a consequence, the recall of the first of two learned activities should be less than the recall of the second, even though the competition of the two sets of responses at the time of attempted recall were unaffected by this pre-rest weakening of the first set of responses. Any initial weakening of the first set of responses should, however, decrease their competitive strength during recall, thus accentuating the difference between the recall of the first and second sets of responses. The prediction from the two-factor theory is, in short, that the amount of *RI* will be greater than the amount of *PI*, the difference to be explained by the active unlearning of

the original responses, which has the direct effect of permitting more frequent successful intrusion of the interpolated responses during the recall and relearning of the interpolated responses, and the indirect effect of decreasing the possibility of successful interference by the original responses during the recall and relearning of the interpolated responses.

No direct comparisons of the retention of the original and interpolated activities in the *RI* experiment have been made. In fact, conclusive evidence that the recall of serial verbal material, such as commonly employed in studies of *RI*, is inhibited by the prior learning of a similar activity, is found only in the recent study by Whitely and Blankenship.³ Their finding clearly demonstrates that *some* of the inhibition of recall obtained in the *RI* experiment is independent of the order of learning of the activities, and supports the interpretation of *RI* as at least in part the result of the competition of responses at the time of attempted recall. Their results need, however, verification and extension before the relationship between *RI* and *PI* can be clarified. For example, Whitely and Blankenship found no correlation between the similarity of the prior learning and the *PI* of the recall of the second list of words or stanzas of poetry, and this is at variance with the findings regarding the similarity factor and *RI*.⁴ Also, their results were limited to the inhibition as measured in written recall after a 48-hr. interval when the learning of both materials was by the method of complete presentation. The present experiment is designed to provide a comparison of *RI* and *PI* when the inhibition is measured by both recall and relearning scores and the formal similarity of the original and interpolated verbal materials is very high and very low.

METHOD

The *Ss*, 24 college men, learned lists of 10 three-consonant syllables under each of the six conditions outlined in Table I. The first three conditions follow the usual paradigm of the *RI* experiment, and the second three conditions follow the paradigm of the *PI* experiment as described by Whitely and Blankenship.⁵ All lists, whether first to be learned or second to be learned, were presented for five trials before the major rest-period. Conditions II and V provide the comparison of the retention of the first and second lists when both lists were learned and they were very dissimilar. Conditions III and VI provide the same comparison when the lists were very similar. Since 23½ min. elapsed between the fifth learning trial on the first list and the first relearning trial in the *RI* work-conditions; whereas, only 20 min. elapsed between the fifth learning trial on the second list and the first relearning trial in the *PI*

³ P. L. Whitely and A. B. Blankenship, The influence of certain conditions prior to learning upon subsequent recall, *J. Exper. Psychol.*, 19, 1936, 496-504.

⁴ Cf. S. H. Britt, Retroactive inhibition: A review of the literature, *Psychol. Bull.*, 32, 1935, 381-440.

⁵ *Op. cit.*

work-conditions, it was necessary to include separate rest-conditions for the measurement of *RI* and *PI*. These are Conditions I and IV. All major rest-periods were filled with the reading of jokes.

The units of material were presented at a 2-sec. rate on a pin-drive memory drum,⁶ and the anticipation method was used throughout. A 6-sec. rest occurred between trials, exclusive of the 2-sec. period during which the cue for the anticipation of the first syllable in the list, the symbol O-O-O, was being presented. The Ss spelled the units. The instructions regarding the anticipation method and the tasks on each experimental day were those reported in detail by Melton and Irwin.⁷ The Ss were not told which of the two lists learned during the work-conditions was to be

TABLE I
SEQUENCE OF EVENTS IN EXPERIMENTAL CONDITIONS DESIGNED TO MEASURE *RI* AND *PI* WITH
LISTS OF HIGH AND LOW DEGREES OF FORMAL SIMILARITY

Condition	Original learning	Rest	Interpolated learning	Rest	Relearning
I. <i>RI</i> rest (control)	5 trials List A	23½ min.	Relearn List A
II. <i>RI</i> work: dissimilar lists	5 trials List A	1 min.	5 trials List B	20 min.	Relearn List A
III. <i>RI</i> work: similar lists	5 trials List A	1 min.	5 trials List B	20 min.	Relearn List A
IV. <i>PI</i> rest (control)	5 trials List B	20 min.	Relearn List B
V. <i>PI</i> work: dissimilar lists	5 trials List A	1 min.	5 trials List B	20 min.	Relearn List B
VI. <i>PI</i> work: similar lists	5 trials List A	1 min.	5 trials List B	20 min.	Relearn List B

recalled and relearned until the end of the major rest-period. As a further precaution against differential motivation in the learning of the two lists, the extra list was always relearned for five trials at the conclusion of the relearning of the primary list. When only one list was learned before the major rest-period, another list was learned for 10 trials after the relearning of the primary list. The major list was always relearned to a criterion of two successive errorless trials.

The 24 Ss went through two complete cycles of the six conditions after three days of practice in learning the consonant units under *RI* and *PI* conditions.⁸ Since all Ss had served for 28 days in another study in which lists of nonsense syllables, words, and numbers were learned and relearned by the anticipation method, the results of this study are specific to well-practiced Ss. Nevertheless, there occurred an improvement in rate of learning during the experiment. Residual practice effects were equated for the six conditions during each cycle by the method of simple rotation in identical order,⁹ the basic order of conditions used in this rotation being I, V, III, IV, II, VI.

⁶ L. B. Ward, Reminiscence and rote learning, *Psychol. Monog.*, 49, 1937, (no. 220), 1-64.

⁷ *Op. cit.*, 176-177.

⁸ We are greatly indebted to the National Youth Administration at the University of Missouri for the funds used to pay these Ss.

⁹ For an example of the method of identical order, cf. Melton and Irwin, *op. cit.*

Since the prior experiment did not involve *RI* or *PI* conditions, the *Ss* were not familiar with the fundamental design of the present experiment.

The formal similarity and dissimilarity of the two lists learned in the work-conditions were achieved by constructing one set of 12 lists from Witmer's three-

TABLE II
LISTS OF THREE-CONSONANT NONSENSE UNITS FOR USE IN DETERMINING THE EFFECT OF
INTER-SERIAL FORMAL SIMILARITY

(The average association-value of the units in each list, as computed from Witmer's association-values, is given in parentheses.)

Group A: Lists which include only the consonants C, F, H, K, M, Q, R, T, and X											
1 (33.4)	2 (47.5)	3 (34.2)	4 (32.0)	5 (33.4)	6 (34.5)	7 (28.2)	8 (43.1)	9 (34.4)	10 (33.5)	11 (30.4)	12 (37.3)
XCM	FTM	HMQ	XFQ	CRX	CFH	MEK	HXM	HCX	FRX	CTM	HTF
KTQ	HCR	XFK	MHC	KTM	MKR	TXQ	KTC	MFT	QTC	RKH	XKQ
HFX	XQT	RTM	RXT	FHR	QTF	HFC	RFX	KQH	KXH	QFT	FRH
CKM	KRM	COX	KQM	TCX	HXK	KMX	QHM	CTX	CFM	XHC	COM
XRF	CTF	MHK	TFH	MKF	MQC	QCF	TKR	RHF	HKR	MTK	KFX
QHC	QXK	TFC	QXC	XRH	RKX	XHM	CMX	QKT	XQF	FXQ	TMR
KXT	MHT	KXQ	FKR	QFK	TMH	FTR	FQT	FXM	RCM	HMR	QHK
MOF	RKF	MCR	CTQ	HMC	XCQ	HKQ	MHR	HQC	TXK	TCF	RXC
CTK	CMQ	QKF	HMF	TKQ	KHF	RXF	XTF	XKR	FHQ	KQX	MEQ
RMH	TXH	RHC	KCR	XHF	QXT	CMH	QCH	TMQ	KMC	MEH	KCT
Group B: Lists which include only the consonants B, D, G, J, L, N, P, S, and Z											
1 (38.6)	2 (39.5)	3 (41.7)	4 (35.8)	5 (36.0)	6 (38.7)	7 (35.3)	8 (43.0)	9 (33.1)	10 (34.6)	11 (46.3)	12 (38.3)
JSN	BPS	BPL	LSG	BPJ	JLZ	BJS	PBN	PJL	ZSJ	NBP	BSG
GBD	GDL	GZN	PBJ	DLG	PSG	NZG	LZG	NDZ	BDP	DJG	LDP
LNJ	NZP	LSB	ZDL	SBZ	DJB	LSJ	DJP	LGB	JNL	ZSN	ZBJ
ZPB	BGJ	DPG	GJN	NDJ	NLP	PGJ	BSZ	JSD	ZBG	PGB	SLN
SDG	ZLS	JNZ	BZS	GZP	ZBD	ZBS	GPL	ZNL	PJS	LDZ	PZG
LJP	DJN	BSD	NDP	LSN	JGN	DJL	DNB	PBS	NGZ	NPJ	JDS
ZGN	PGZ	ZLP	LGZ	JGB	SDL	GPN	JLS	GZJ	LPB	SGL	ZNB
BSJ	SBD	NBD	SBN	ZNP	BNP	SZD	PDZ	LPN	SZN	JZD	GJL
PZL	LNP	SZG	JPL	SJL	GZS	JNB	SGB	SBG	DLJ	LBN	DZS
NDB	JSG	PDN	DZB	GDB	LBJ	DPZ	NLD	DLZ	GSB	GPS	NGP

consonant units¹⁰ which included only the consonants C, K, R, F, M, Q, H, T, and X, and another set of 12 lists from those units which included only the consonants B, D, G, J, L, N, P, S, and Z. Two lists from the same set of lists were used in the work-conditions with similar lists, and two lists from different sets of lists were used in the work-conditions which required dissimilar lists. The lists used are given in Table II. All lists were constructed according to the following rules:

(1) All units have association-values, as determined by Witmer,¹¹ which are less than 71%. Only two units have association-values higher than 58%, and the average association-values of the completed lists range between 28.2% and 46.3%.

(2) No consonant was repeated in successive units of a list.

(3) No two units in the same list have two consonants in common and in the same order.

(4) The frequency of occurrence of the various consonants as the first, second, and third letter in a unit has been equalized in so far as possible for each list.

The sets of lists made from the two groups of nine consonants were used equally

¹⁰ L. R. Witmer, The association value of three-place consonant syllables, *Ped. Sem.*, 47, 1935, 337-360.

¹¹ *Op. cit.*

often in the six conditions of the experiment. Lists 1 to 6 in both sets were learned first in all work-conditions and in the *RI* rest-conditions; Lists 7 to 12 in both sets were learned second in all work-conditions and in the *PI* rest-condition. The average difficulty of the lists in the two sets is approximately the same, as revealed by the learning scores during the first five trials in the *RI* and *PI* rest-conditions. In the *RI* rest-condition the mean correct anticipations during the five learning trials in both cycles of the experiment was $9.00 \pm .73$, and the mean correct anticipations on the fifth trial was $3.13 \pm .22$. The corresponding means for the *PI* rest-condition were $8.52 \pm .57$ and $3.08 \pm .24$. The differences between these two pairs of means are only 1.04 and .17 times their respective sigma.¹²

During the course of the experiment a continuous record was obtained of the time-interval between the presentation of one syllable and the vocal anticipation of the next syllable, if such occurred, by means of a Stoelting voice-key and a constant-speed polygraph. Measurements were also obtained of the muscular tension in the right hand by means of a pneumatic device on the handle of a stylus which the S

TABLE III
MEASURES OF THE DEGREE OF LEARNING OF THE FIRST AND SECOND LISTS
(Number of correct anticipations)

Trial	List	Retroactive inhibition			Proactive inhibition		
		Rest 23½ min.	Dissimilar lists	Similar lists	Rest 20 min.	Dissimilar lists	Similar lists
Fifth	1	3.13±.22	3.31±.27	2.98±.27	3.08±.24	3.23±.28	2.90±.28
	2		3.23±.29	2.54±.24		3.06±.27	2.73±.21
All	1	9.04±.75	8.85±.69	8.25±.77	8.52±.57	9.08±.75	8.54±.77
	2		8.73±.71	6.83±.60		8.47±.67	6.73±.52

moved in a downward direction whenever he made a vocal response. These reaction-time and muscular tension data will not be considered in this report.

RESULTS

Learning first and second lists. In this study the degree of learning of the first and second lists has been controlled in terms of frequency of repetition. Since the total time per repetition was 20 sec., the total time spent in learning the first and second lists was constant. The intention to compare the recall and relearning of the first and second lists requires, however, evidence on the frequency of correct anticipations during the learning of the units in those lists. In Table III we have presented the mean correct anticipations on the fifth learning trial on the first and second

¹² All measures of reliability are sigma. All sigma of the mean differences have been corrected for the correlation between the arrays. For this experimental design there are 23 degrees of freedom, and a *t* value of 2.807 is taken as an indication of a very significant mean difference (*P* = 0.01). Cf. R. A. Fisher, *Statistical Methods for Research Workers*, 1934 (5th ed.), 158.

lists and on all five learning trials for those lists.¹³ It should be noted that the scores on the first list are remarkably uniform from condition to condition. The maximum difference between the scores on the fifth trial is $0.41 \pm .28$, with a t of 1.46. The maximum difference between the scores on all five learning trials is $0.73 \pm .77$, with a t of 0.95.

As would be expected, the scores on the first list are not different from those on the second when the second is the only one to be learned as in the *PI* rest-condition. It is to be noted also that the scores on the second list do not differ significantly from those on the first when both lists are learned and are made from different sets of nine consonants. When the scores on the first and the second lists respectively are combined for the *RI* and *PI* dissimilar work-conditions, the mean difference between the scores on the fifth learning trial is $0.12 \pm .16$ ($t = 0.75$), and the mean difference between the scores on all five trials is $0.36 \pm .39$ ($t = 0.92$). It may be concluded that no *PI* of the learning of the second list occurred when the two lists were dissimilar.

There is, however, a marked *PI* of the learning of the second list when both lists were constructed from the same set of nine consonants. When the scores on the fifth trial are combined for the *RI* and *PI* similar work-conditions, the mean correct anticipations on the fifth trial is 0.30 less on the second list than on the first list. This difference is only 1.88 times its sigma of 0.16, but the corresponding difference between the frequencies of correct anticipations on all five learning trials is $1.67 \pm .49$, with a t of 3.41.

The presence of a significant *PI* in the learning of the second similar list when all five learning trials are considered, and the absence of a significant difference when only the fifth learning trial is considered, suggests that the *PI* decreased in amount as the learning of the second list proceeded. This is verified when the correct anticipations on each of the five learning trials on the first and second lists are compared. These scores for the first and second dissimilar list and the first and second similar lists are shown in Fig. 1. The curves show that there was no *PI* at any time in the learning of the second dissimilar list, and that the *PI* in the learning of the second similar list became relatively smaller as the number of trials on the second list increased. The percentage of *PI* on Trials 2 to 5 was 36.2, 28.5, 17.6,

¹³ All means throughout this paper include the scores for the 24 Ss in both cycles of the experiment. The scores of every S in Cycle I and Cycle II were averaged before the means and S.D.s were computed, and the N was, of course, considered to be 24, rather than 48. The results of the first and second cycles are consistent, the only difference being the faster learning and better retention during the second cycle.

and 10.3, respectively. The presence of a transitory *PI* in the learning of a second formally similar nonsense series verifies earlier results with standard nonsense syllables.¹⁴

Relative retention of the original and interpolated lists. The basic data of the experiment indicate the relative retention of the first and second lists after an interval of rest. The comparison of the retention of the first list

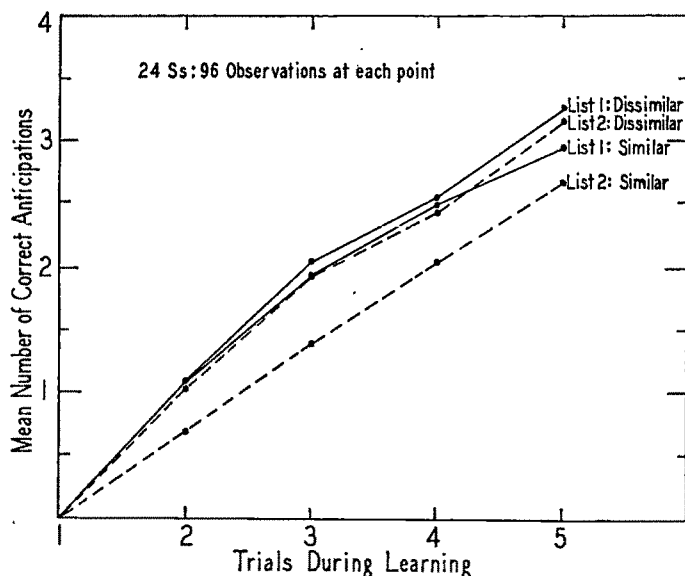


FIG. 1. MEAN CORRECT ANTICIPATIONS ON THE FIVE TRIALS OF ORIGINAL LEARNING OF THE FIRST AND SECOND LISTS WHEN THEY WERE SIMILAR AND DISSIMILAR

Comparison of the curves for the first and second lists shows *PI* in the learning of the second list when the lists were similar, but no *PI* when the lists were dissimilar.

under the rest-condition and under the similar and dissimilar work-conditions provides the usual measure of *RI*. The comparison of the retention of the second list with and without the prior learning of a similar or dissimilar list provides a measure of *PI* comparable to the

¹⁴ Melton and Irwin, *op. cit.* The absolute *PI* in the learning of the second similar list was 0.43, 0.55, 0.44, and 0.30 on Trials 2 through 5, respectively. There is, therefore, a trend toward the dissipation of the *PI*, whether it is measured in absolute or relative terms. The complete dissipation of the absolute *PI* is shown in the study by Melton and Irwin, and it may be assumed that the complete dissipation of the *PI* would have been obtained in the present study if the learning of the second list had been extended beyond five trials.

measure of *RI*. The deduction from the two-factor theory which led to this experiment was that $RI - PI > 0$ under the arranged conditions.

In Table IV are presented the mean correct anticipations on the first, second, third, and fourth relearning trials for all conditions, and the percentage of *RI* and *PI*. The percentages of inhibition have been computed by the formula: $100 (\text{rest} - \text{work}) / \text{rest}$. Of first importance in Table IV are the recall scores under the *RI* and *PI* rest-conditions, since the recalls were necessarily obtained after different rest-intervals ($23\frac{1}{3}$ min. and 20 min., respectively). If the effective degree of learning of the

TABLE IV
AMOUNTS OF PROACTIVE AND RETROACTIVE INHIBITION AS MEASURED BY THE MEAN NUMBER OF CORRECT ANTICIPATIONS IN THE FIRST FOUR RELEARNING TRIALS

Condition	Retroactive inhibition			Proactive inhibition		
	Rest $23\frac{1}{3}$ min.	Dissimilar lists	Similar lists	Rest 20 min.	Dissimilar lists	Similar lists
Recall Trial 1	$2.21 \pm .20$	$0.94 \pm .14$	$0.46 \pm .11$	$2.08 \pm .25$	$1.56 \pm .28$	$1.02 \pm .18$
% <i>RI</i> or <i>PI</i>		57.5	79.2		25.0	51.0
Recall Trial 2	$3.33 \pm .29$	$2.65 \pm .23$	$1.67 \pm .23$	$3.19 \pm .24$	$2.77 \pm .29$	$2.46 \pm .21$
% <i>RI</i> or <i>PI</i>		20.4	49.9		13.2	22.9
Recall Trial 3	$3.73 \pm .30$	$3.54 \pm .25$	$2.35 \pm .29$	$3.64 \pm .27$	$3.71 \pm .35$	$2.98 \pm .30$
% <i>RI</i> or <i>PI</i>		5.1	37.0		+1.9	24.2
Recall Trial 4	$4.35 \pm .28$	$4.17 \pm .30$	$3.37 \pm .39$	$4.10 \pm .31$	$4.17 \pm .32$	$3.81 \pm .34$
% <i>RI</i> or <i>PI</i>		4.1	22.5		+1.7	7.1

first list were less than that of the second list at the time of attempted recall and relearning, the *RI* might for this reason be expected to exceed the amount of *PI* because the amount of interference in recall is known to increase with a decrease in the degree of learning of the list subject to the interference.¹⁵ There was, however, no significant effect of the difference of $3\frac{1}{3}$ min. in the rest-interval. No difference between the recall scores of the *RI* and *PI* rest-conditions on the four relearning trials is more than 1.14 times its sigma, and all differences are opposed to the expectation based on the curve of retention for nonsense syllables under our conditions.¹⁶

In the first relearning (recall) trial the relationship between *RI* and *PI* is clearly revealed. Highly significant amounts of both *RI* and *PI* were obtained when the lists were similar, and the amount of *RI* was significantly greater than the amount of *PI*. Thus, the differences between

¹⁵ McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

¹⁶ C. W. Luh, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.

the scores on the *RI* and *PI* similar work-conditions and their respective rest-conditions were $1.75 \pm .20$ ($t = 8.75$) and $1.06 \pm .24$ ($t = 4.42$), and the difference between the scores on the *RI* and *PI* similar work-conditions was $0.56 \pm .17$ ($t = 3.29$). When the two lists were dissimilar, there was a marked reduction in the amounts of *RI* and *PI*. The difference between the *RI* similar work-condition and the *RI* dissimilar work-condition was $0.48 \pm .17$ ($t = 2.82$), and the corresponding difference in *PI* was $0.54 \pm .27$ ($t = 2.00$). But the amount of *RI* with dissimilar lists again exceeds the amount of *PI* by $0.62 \pm .27$ ($t = 2.30$). In fact, the only work-condition which failed to yield a statistically significant interference effect was the *PI* work-condition with dissimilar lists. The differences between the *RI* and *PI* work-conditions with dissimilar lists and their respective rest-conditions were $1.27 \pm .20$ ($t = 6.35$) and $0.52 \pm .32$ ($t = 1.63$).

The *RI* and *PI* with both the dissimilar and similar lists shows a percentage decrease as the relearning proceeds, which is in conformity with earlier results with nonsense syllables.¹⁷ In the case of the similar lists, the amount of *RI* continues greater than the amount of *PI* through the fourth relearning trial, although the differences cease to be significant after the second relearning trial. The differences between the scores in the *RI* and *PI* similar work-conditions are $0.79 \pm .25$, $0.63 \pm .29$, and $0.44 \pm .29$ on the second, third, and fourth relearning trials. In the case of the dissimilar lists, the difference between the amounts of *RI* and *PI* all but vanishes on the second relearning trial and thereafter. This does not seem to be attributable to the disappearance of all inhibition with dissimilar lists, because the difference between the *RI* dissimilar work-score and the *RI* rest-score on the second relearning trial is $0.68 \pm .23$ ($t = 2.96$), and the difference between the *PI* dissimilar work-score and the *PI* rest-score on that trial is $0.42 \pm .27$ ($t = 1.56$). It would appear that the difference between the amounts of *RI* and *PI* in the trials following the first relearning trial depends upon the presence of a high degree of similarity of the two lists. This difference between the relative amounts of *RI* and *PI* with similar lists and with dissimilar lists is best shown by the ratios of the scores under the *RI* and *PI* conditions during the four relearning trials. In the case of the similar lists these ratios (100 Recall *RI* condition/Recall *PI* condition) are 45.1%, 67.9%, 78.9%, and 88.5%; in the case of the dissimilar lists these ratios are 60.3%, 95.7%, 95.4%, and 100.0%.

The mean trials required by the *Ss* to complete the mastery of the first

¹⁷ Melton and Irwin, *op. cit.*

or second lists to one errorless trial or two successive errorless trials add very little to the results obtained with the more sensitive recall scores. These relearning scores are presented in Table V. The differences between the scores obtained after the rest-interval of $23\frac{1}{3}$ min. in the control for the *RI* conditions and after the rest-interval of 20 min. in the control for the *PI* conditions are again of no significance. The differences between the trials required to relearn to one errorless trial and to two successive errorless trials are 0.94 ± 1.21 ($t = 0.78$) and 2.11 ± 1.45 ($t = 1.46$), respectively.

None of the comparisons of the *RI* and *PI* conditions yield a difference

TABLE V

AMOUNTS OF PROACTIVE AND RETROACTIVE INHIBITION AS MEASURED BY TRIALS TO RELEARN TO ONE ERRORLESS AND TWO SUCCESSIVE ERRORLESS TRIALS

Condition	Retroactive inhibition			Proactive inhibition		
	Rest $23\frac{1}{3}$ min.	Dissimilar lists	Similar lists	Rest 20 min.	Dissimilar lists	Similar lists
<i>RI</i> trials to one errorless % <i>RI</i> or <i>PI</i>	18.8 ± 1.6	18.7 ± 1.3	21.7 ± 2.0	19.8 ± 1.9	18.6 ± 1.8	21.5 ± 1.7
		+0.5	15.4		+6.1	8.6
<i>RI</i> trials to two errorless % <i>RI</i> or <i>PI</i>	25.2 ± 2.4	23.2 ± 1.6	26.0 ± 2.4	23.0 ± 1.9	22.3 ± 2.1	25.7 ± 2.1
		+7.9	3.2		+3.0	11.7

which is greater than 1.94 times its sigma. It is, however, of some interest that the *RI* and *PI* conditions with dissimilar lists show consistently fewer trials for relearning to mastery, as compared with the corresponding rest conditions, and the *RI* and *PI* conditions with similar lists show a consistently greater number of trials for relearning, as compared with the same rest-conditions. Thus, it appears that the learning of a second list, either as a prior list or as an interpolated list, aided the relearning to a criterion of mastery when the lists were dissimilar; whereas, the learning of a similar second list produced the expected inhibition of the relearning to a criterion. It is probable that the learning of the additional list for five trials produced a general positive transfer (practice) effect in all work-conditions which was not present in the rest-conditions, and that the negative transfer (*RI* or *PI*) more than counteracted the positive transfer effect only when the two lists were similar. The most significant differences between the means shown in Table V are obtained in the comparison of the relearning scores for dissimilar and similar lists. Thus, the difference between the mean trials to relearn to one errorless trial and two successive errorless trials in the *RI* dissimilar work-condition and the *RI* similar work-condition are 3.04 ± 1.30 ($t = 2.34$) and 2.83 ± 2.09 ($t = 1.14$), and the cor-

responding differences in the case of the *PI* work-conditions are 2.81 ± 1.26 ($t = 2.23$) and 3.38 ± 1.44 ($t = 2.35$).

The relearning records may be employed to give additional confirmation of the different rates of disappearance of *RI* and *PI* when similar and dissimilar lists are learned, by applying a modification of the Vincent-Kjerstad technique to the relearning records.¹⁸ The method has the ad-

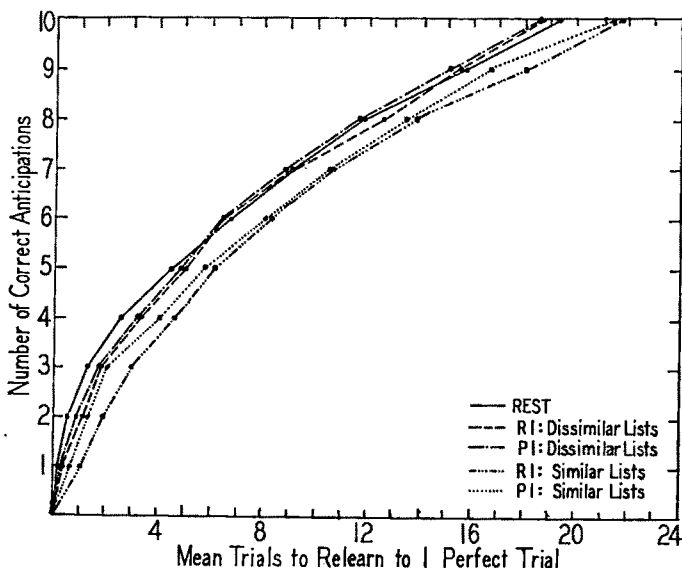


FIG. 2. MEAN TRIALS TO RELEARN THE LISTS TO VARIOUS LEVELS OF MASTERY UNDER THE *RI*, *PI*, AND REST-CONDITIONS

The *RI* and *PI* is shown to be transitory when dissimilar lists were learned, but persistent when similar lists were learned.

vantage that it combines the entire relearning curves of different *Ss* even though they reached the criterion of mastery in different numbers of trials. Thus, for the rest- and work-conditions we have computed the mean relearning trials required by the *Ss* before they could correctly anticipate 1, 2, 3, 4 . . . 10 units within a single trial. In Fig. 2, the curves for the four work-conditions are shown separately, but the curves for the two rest-conditions have been combined. It is apparent that the *RI* and *PI* persist

¹⁸ Cf. A. W. Melton, The end-spurt in memorization curves as an artifact of the averaging of individual curves, *Psychol. Monog.*, 47, 1936, (no. 212), 119-133; Melton and Irwin, *op. cit.*

throughout the relearning to one errorless trial when the lists are similar, but that when the lists are dissimilar the *RI* and *PI* disappear after the relearning of the list has reached the stage where 5 of the 10 units are correctly anticipated.

Specific inter-list interference effects. Attention has been called recently to the importance for theory of the overt inter-list intrusions of responses during the learning and relearning of lists under *RI* and *PI* conditions.¹⁹ Such intrusions during the learning of the interpolated list in the *RI* experiment have been used by Melton and Irwin²⁰ as an indication of the 'unlearning' of the original list which the two-factor theory of *RI* postulates during the learning of the interpolated list. Such inter-list intrusions during recall and relearning are considered to be direct evidence of the competition of the original and interpolated responses as a factor in *RI*.

Fewer inter-list intrusions occurred in the present experiment than in the comparable conditions of the Melton and Irwin study with nonsense syllables. Nevertheless, those whole-syllable intrusions which occurred—and none other than whole-syllable intrusions could be properly identified—tended to confirm the other evidence for the conclusion that the amount of *RI* in recall and relearning is greater than the amount of *PI*. In the relearning under the *RI* similar work-condition there were 37 intrusions from the interpolated list. Of these 26 were different syllables, of which 10 occurred on the first relearning trial. In the comparable *PI* work-condition there were 26 intrusions from the prior list, of which 18 were different syllables and 5 occurred on the first relearning trial. At least one intrusion occurred during 23 of the 48 relearning records under the *RI* similar work-condition; whereas, only 15 of the 48 relearning records under the *PI* similar work-condition showed intrusions. The frequencies of repetition of particular intrusions during the *RI* and *PI* relearnings were 1.42 and 1.44.²¹ In the dissimilar work-conditions the intrusions during relearning were very rare. Under the *RI* condition the total number of intrusions was two, and the number of different intrusion syllables was two; under the *PI* condition the number of intrusions was nine, but the number of different syllables was again two.

¹⁹ McKinney and McGeoch, *op. cit.*; Melton and Irwin, *op. cit.*

²⁰ *Op. cit.*

²¹ The frequency of repetition of intrusions during relearning seems to be very stable. Thus, in the Melton and Irwin study, *op. cit.*, the average number of repetitions of particular intrusions in the four *RI* work-conditions was 1.44, 1.44, 1.43, and 1.35. The degree of learning of the interpolated material increased in the order in which the averages are presented.

The frequencies of intrusions of syllables from the first list during the five trials of learning of the second list were low. No intrusions occurred in either the *RI* or *PI* work-conditions with dissimilar lists, and the total number of intrusions in the *RI* and *PI* work-conditions with similar lists was nine and eight. The number of different intrusion syllables under the latter conditions was five and seven.

It is clear that the frequency of inter-list intrusions, both in the learning of the second list and in the relearning of the first or second list, increases with an increase in the formal similarity of the lists. Precise determination of the similarity factor, whether formal similarity or serial position similarity, responsible for specific intrusions is impossible with the consonant units and lists employed in this study.²² It is worthy of note, however, that all four different intruding syllables during relearning under the *RI* and *PI* work-conditions with dissimilar lists displaced syllables which were not more than one serial position removed from the serial position of the intruding syllable in its own list. In the *RI* and *PI* work-conditions with similar lists the number of such intrusions apparently based on serial position similarity was 11 (42%) and 11 (61%), respectively. During the learning of the second list under the *RI* and *PI* work-conditions with similar lists, these frequencies were 5 (71%) and 4 (80%). Apparently, the factor of serial position similarity is an important determiner of inter-list intrusions even when the similarity of the lists is very great. In fact, it is suggested that the serial position factor is not sufficient to produce a significant number of intrusions unless there is also a formal similarity of the materials learned.

As in the earlier study with nonsense syllables, the intrusions which seem to be based, at least in part, on serial position similarity occur earlier during the relearning process than those which are not so based. The average trial of occurrence during relearning of the 11 intrusions in the *RI* work-condition with similar lists which seem to be based on serial position similarity was 1.7; whereas, the average trial for the 15 intrusions not so based was 7.5. The corresponding averages for the *PI* work-condition with similar lists were 6.3 and 10.9, and the average trial of occurrence of the four intrusions during the *RI* and *PI* work-conditions with dissimilar lists was 1.3. All these averages relate to the trial of first occurrence of an intrusion.

²² When dissimilar lists were learned there was no possibility of intrusions based on formal similarity of the lists; when similar lists were learned it was impossible to identify intrusions based on formal similarity because all the units in both lists were made from only nine consonants and there was overlapping formal identity of all the units.

THEORETICAL CONCLUSIONS

The results confirm the expectation based on a two-factor theory of *RI* which specifies that a portion of the *RI* obtained in the conventional *RI* experiment is attributable to something that happens to the original activity during the learning of the interpolated activity (Factor X), and that the remainder of the *RI* is attributable to the competition of the original and interpolated responses at the time of attempted recall and relearning of the original responses. The expectation was that the recall and relearning of the first of two activities would be subjected to both decremental influences; whereas, the recall and relearning of the second activity would be subjected only to the competition of the responses involved in the first activity. The total inhibition of the recall and relearning of the second activity (*PI*) would, therefore, be less than the total inhibition of the first activity (*RI*). Although no differences between the amounts of *RI* and *PI* were obtained when retention was measured in terms of the trials required for relearning of the lists to mastery, the expected differences were found in the recall scores on the first few relearning trials. The *RI* was significantly greater than the *PI* on the first relearning trial whether the lists learned were very similar or maximally dissimilar, and the difference persisted through the next three relearning trials when the lists were very similar, although it disappeared on the second relearning trial when the lists were dissimilar. As additional evidence of the greater effective strength of the interpolated activity after an interval of rest, it was found that the frequency of overt intrusions of interpolated responses during the relearning of the original list was greater than the frequency of overt intrusions of the original responses during the relearning of the interpolated list.

The results also permit a further characterization of the two factors in *RI* and their interaction. Of considerable significance is the fact that the recall of the second list was greater than the recall of the first list when the lists were similar, even though the second list was less well learned than the first list after the five original learning trials. That is, the 'unlearning' of the first list during the learning of the second list delayed the learning of the second list, but the delay in the learning of the second list did not affect the recall of that list as much as the unlearning of the first list affected its recall. It may, therefore, be suggested that inhibition of the learning of the second list has no permanent deleterious effect on the strength of the associations involved in the practice of the second list, but that the effect of whatever is happening to the first list during this proactive inhibition of the learning of the second list is permanent. If it is as-

sumed that the intrusion of units from the first list, whether partial or complete, overt or covert, during the learning of the second list results in their 'unlearning' and at the same time blocks the correct recall of the units of the second list, the conclusion is that this inappropriate transfer (generalization) of original responses permanently lessens their associative strength without permanently lessening the associative strengths of the new and different responses which are being practiced. This assumed impermanence of the inhibition of the second activity is congruent with the accumulating evidence for the transitoriness of the proactive inhibition of a second activity by the prior learning of a similar activity.²³

The results with the dissimilar lists have especial significance. In the work-conditions with dissimilar lists, the difference between the amounts of *RI* and *PI* appeared in a significant amount only on the first relearning trial. Furthermore, there were no overt intrusions of original syllables during the learning of the second list, and there was no measurable retardation of the learning of the second list. If, therefore, the frequency of overt intrusions during the learning of the second list is an index of the 'unlearning' to which the first list is subjected, and this 'unlearning' is the reason for the difference between the amounts of *RI* and *PI*, the difference between the *RI* and *PI* with dissimilar lists should be less than the difference between the *RI* and *PI* with similar lists. This result was obtained. In addition, the greater transitoriness of both *RI* and *PI* during the relearning process when the lists were dissimilar may be related to the finding of Melton and Irwin,²⁴ corroborated here in so far as the data permitted, that relearning intrusions based on serial position similarity occur much earlier during the relearning process than the intrusions based on the formal similarity of the units being learned. In the work-conditions with dissimilar lists the similarity factors operative in producing confusion of the lists must have been serial position similarities or the as yet indeterminate similarities of general situation and task set.

The interpretation of these results as favoring a form of the transfer theory of *RI* which specifies an 'unlearning' factor and a 'competition' factor does not deny the possibility that the 'unlearning' factor may be interpreted in terms of competition of response. It does insist that if the 'unlearning' factor is so interpreted, the competing responses established when an intrusion from the original list occurs without reinforcement during the learning of the interpolated list are not only the specific responses

²³ Melton and Irwin, *op. cit.*

²⁴ *Idem.*

involved in the recitation of the interpolated list. As previously indicated,²⁵ diverse lines of evidence favor the interpretation of such an 'unlearning' process in terms of the establishment of an incompatible competing response. For this reason, the two-factor interpretation of *RI* is in no final sense a contradiction of the single-factor reproductive inhibition or 'competition' theory which has been favorably considered by McGeoch.²⁶

SUMMARY

(1) In the experiment, 24 Ss learned serial lists of 10 three-consonant units for five trials with and without the prior or subsequent learning of another maximally dissimilar or very similar list of consonant units for five trials, and then relearned the list to a criterion of two successive errorless trials after approximately 20 min. of rest. The primary purpose was to compare the inhibition of the recall and relearning of the first of two lists (*RI*) and the inhibition of the recall and relearning of the second of two lists (*PI*). The anticipation method was used throughout.

(2) The results support the following generalizations: (a) The inhibition of the recall of the first of two lists is greater than the inhibition of the recall of the second, whether the lists are made from two different sets of nine consonants or are made from the same set of nine consonants. This difference persists throughout at least the first four relearning trials when the lists are similar, but disappears after the first relearning trial when the lists are dissimilar. The trials required for relearning to mastery the first and second lists are not significantly different.

(b) The frequency of overt inter-list response intrusions is greater during the recall and relearning of the first list than during the recall and relearning of the second list.

(c) The amount of inhibition of both lists is greater when the two lists are similar than when the two lists are dissimilar. The difference is reflected in the recall scores, the trials required for relearning to mastery, and in the frequency of overt inter-list response intrusions. When the lists are dissimilar, the inhibition disappears very early during the relearning process.

(3) The following interpretations are ventured: (a) The results favor a transfer theory of retroactive inhibition, and in particular a transfer theory which attributes the inhibition of recall and relearning to two factors; namely, the 'unlearning' of the original responses at the time the interpolated responses are being learned, and the 'competition' of the original and interpolated responses at the time of attempted recall of the former.

(b) This interpretation of retroactive inhibition implies that the proactive inhibition of the recall and relearning of the interpolated list is the result only of the competition of the original responses at the time of attempted recall and relearning of the interpolated responses. The proactive inhibition of the *learning* of the interpolated list, when present, is assumed to have no permanent effect on the strength of the associations involved in the interpolated list.

²⁵ *Idem.*

²⁶ Cf. footnote 2.

RELATION OF DECISION-TIME TO THE CATEGORIES OF RESPONSE

By DORWIN CARTWRIGHT, State University of Iowa

There is general agreement among investigators that decision-time varies directly with the similarity of the alternatives. Extensive experimental work by Henmon,¹ Lemmon,² and Kellogg³ has provided empirical confirmation of this relationship, and Woodworth⁴ establishes it as a general principle. The present writer proposes elsewhere a theory with which to account for the variations in decision-time which accompany variations in similarity.⁵ In this theory it is held that increases in the length of decision-time are produced by a conflict between different responses and that such a conflict arises when a stimulus falls upon the border between two ranges of equivalent stimuli. In the usual psychophysical experiment there corresponds to each category of response a range of equivalence, so that stimuli which lie at the edge of the categories are associated with maximal decision-times. Since almost all of the relevant experiments have employed two categories, the test-stimulus equal to the standard has tended to fall on the border between the ranges of stimuli yielding judgments of *greater* and *less*. According to the theory, therefore, equality should give the longest decision-time and the usual relationship should be obtained under these conditions. An entirely different relationship may be expected, however, when three categories are employed. Then three ranges of equivalence and two borders are operative, and the test-stimulus equal to the standard falls in the middle of the central range. Under these conditions decision-time would be maximal on either side of equality and, therefore, would vary *inversely* with similarity over part of the continuum and *directly* over the rest.

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¹ V. A. C. Henmon, The time of perception as a measure of differences in sensations, *Arch. Phil., Psychol., & Sci. Methods*, 8, 1906, 76.

² V. W. Lemmon, The relation of reaction-time to measures of intelligence, memory, and learning, *Arch. Psychol.*, 15, 1927, (no. 94), 1-38.

³ W. N. Kellogg, The time of judgment in psychometric measures, this JOURNAL, 43, 1931, 65-86.

⁴ R. S. Woodworth, *Experimental Psychology*, 1938, 333 and 426.

⁵ Dorwin Cartwright, Decision-time in relation to the differentiation of the phenomenal field, *Psychol. Rev.*, 48, 1941 (in press).

In the present investigation seven experimental variations were constructed so that the theory could be tested under a variety of conditions. Since the border of a range is defined as that stimulus which leads equally often to two responses, the basic hypothesis is tested by comparing in each case the peaks of the curve of decision-time with the 50% points on the curve of relative frequency.

METHOD

Procedure. But one basic procedure was employed throughout the experiments: *a series of stimuli was differentiated into several ranges of equivalence and for each of these ranges a category-name was established.* After the differentiation had been accomplished, the series was presented to the Ss with instructions for them to decide to which of the categories each stimulus belonged. The outcome of the decision and the decision-time were recorded in each case.

Throughout, the usual experimental precautions were employed. The stimuli of the test series were presented in a random order, and, when the same Ss were used for two different conditions, the order of the conditions was varied so that extraneous effects would be minimized.

Apparatus. Except in Experiment VI, the stimuli were presented in a modified Dodge tachistoscope. In Experiment VI the stimuli were presented behind an opening in an upright wooden board. Each stimulus was visible 0.5 sec. The decisions were uttered orally into a voice-key, except in Experiments IV and VI, when they were indicated by the manipulation of a lever. The decision-times were registered accurately to 0.01 sec. by an electric clock which began running simultaneously with the presentation of the stimuli and stopped when S expressed his decision.

Subjects. A total of 41 Ss was employed throughout the experiments. Seven Ss were graduate students of psychology at the State University of Iowa and the remainder were students, primarily of psychology, at Harvard and Radcliffe Colleges. Except in a few instances, enumerated in connection with the discussion of the experiments, all Ss were naïve to the purpose of the experiments.

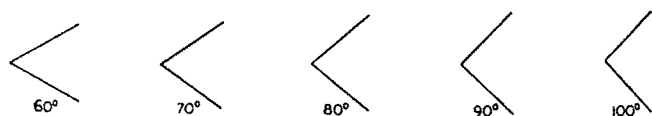
Treatment of the scores. A total of 4188 decisions was made in the seven experiments. After recording the nature of each decision and the decision-time for each stimulus, curves of the relative frequency of response and of decision-time were plotted along the range of stimuli. For simplicity only the frequency of decisions corresponding to the central category is given.

Since the *shapes* of the curves are the significant data and since all of the curves, obtained under a variety of conditions, exhibit shapes which are consistent with one general method of interpretation, no measure of reliability or significance has been computed. Abnormally long decision-times, which occurred occasionally, weight the average scores unduly in one direction, since variations from the normal range can occur radically only in this one direction. An arbitrary ceiling was established, therefore, for each S and scores passing this limit were given the value of the ceiling. About 3% of the scores were treated in this manner.

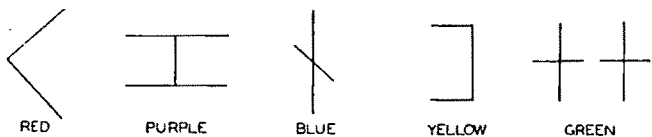
EXPERIMENT I: WIDE VS. NARROW RANGE

A simple test of the basic hypothesis can be made by arbitrarily including different ranges of stimuli within a category and determining where the

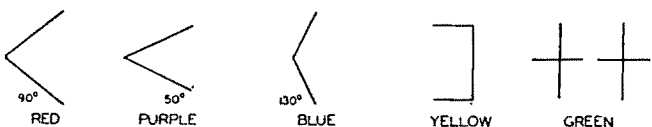
peaks of the curves lie. Accordingly, two ranges of stimuli differing markedly in size were established and it was determined whether the curves of



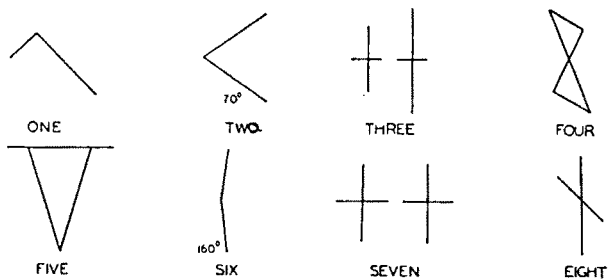
EXPERIMENT I



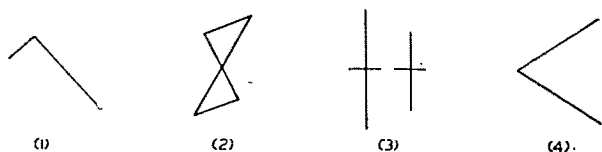
EXPERIMENT II (UNRELATED FIGURES)



EXPERIMENT II (RELATED FIGURES)



EXPERIMENT III



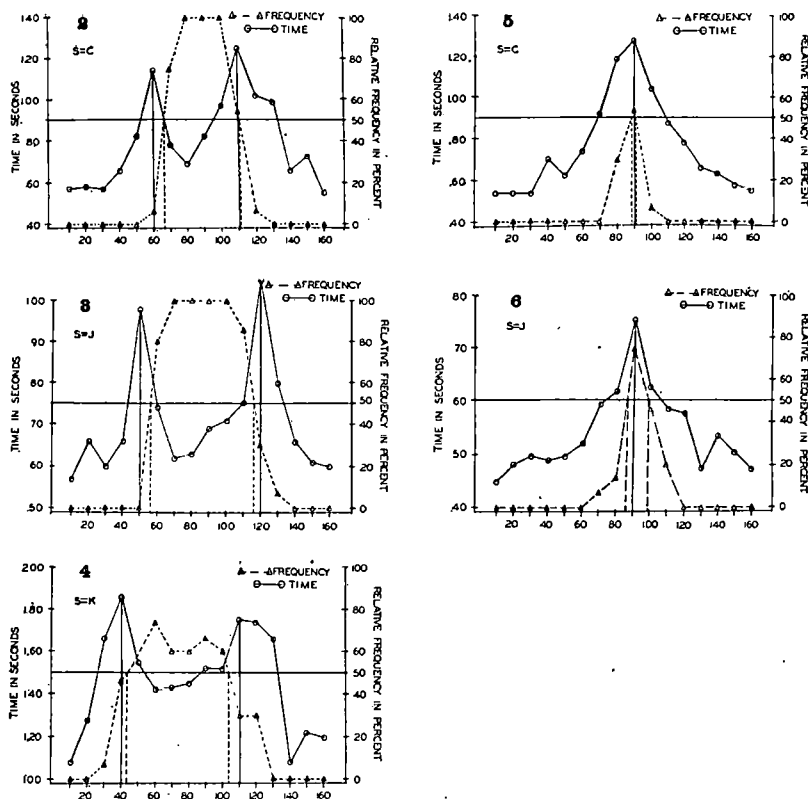
EXPERIMENT IV

FIG. 1. GEOMETRIC PATTERNS USED AS STIMULI IN EXPERIMENTS I-IV

decision-time possessed peaks more widely separated when the range was more extensive than when it was of a smaller size.

(1) *The wide range.* The wide range was established by a learning series containing angles of 60° , 70° , 80° , 90° , and 100° . (See Fig. 1, Experiment I.)

The Ss were told to observe the angles so that they would be able to recognize



FIGS. 2-4. WIDE RANGE IN EXPERIMENT I: RELATIVE FREQUENCY AND DECISION-TIME

Values on abscissa indicate size of angles used in test series.

FIGS. 5-6. NARROW RANGE IN EXPERIMENT I: RELATIVE FREQUENCY AND DECISION-TIME

Values on abscissa indicate size of angles used in test series.

any of them in the test series. Each angle was shown five times. When given the test series (angles ranging in steps of 10° from 10° to 160°), the Ss were asked to decide whether or not a given pattern was one of those presented in the learning series. It was expected that a whole range of angles would be included within the

category of "angles observed in the learning series" and that the curves of decision-time would show two widely separated peaks falling at the ends of the range. Two sophisticated Ss (*C* and *J*) and one naïve S (*K*) made a total of 656 decisions. The two sophisticated Ss made 15 decisions for each stimulus while the naïve S made 11.

The curves of the three Ss are presented in Figs. 2-4. Each curve of decision-time possesses two peaks, although one of the peaks for *K* is less sharp than the rest. Since the curve of relative frequency provides an indication of the location of the border of the range, a comparison of the curves of frequency and of decision-time furnishes a test of the basic hypothesis. The average distance between the *peak* of the curve of time and the 50% point of the curve of frequency is only 4°, giving remarkable agreement when it is considered that no test stimulus yielded a frequency of exactly 50% and that the unit of the scale was 10°. It may be concluded that under these conditions decision-time is maximal at the border of the range of equivalence.

(2) *The narrow range.* The narrow range was established by showing in the learning series only the 90° angle.

It was expected that the range of stimuli corresponding to judgments of "pattern observed in the learning series" would include only one stimulus. The borders of the range would fall, accordingly, on either side of that stimulus and the curves of time should display only one peak or else two peaks with a very small distance between them. The 90° angle was presented five times in succession. The same instructions and the same test series as were employed for testing the larger range were given to the two sophisticated Ss (*C* and *J*). Each S made 15 decisions for each of the test figures, giving a total of 480 decisions.

The curves are presented in Figs. 5-6. Both curves are unimodal, the only test figure with a frequency of decisions of *same* greater than 50% being the figure which also produced the longest decision-time. The average distance from the peak of the curve of time to the 50% point is 4° and only one value deviates from the predicted relationship (the 100° angle for *J*).

A comparison of the curves in Figs. 2-4 and 5-6 shows that the two experimental conditions produced extremely different curves of decision-time. When the Ss consider several patterns within the same category, a large range of equivalence is produced, whereas having the Ss consider only one figure within the category yields so small a range that the two borders are not indicated separately by the experimental techniques employed.

EXPERIMENT II: SIMILAR VS. DISSIMILAR SERIES

In the second experiment it was assumed that, when S learns to associate a word with a visual pattern, he will employ a smaller range of equivalence

for the pattern when the series contains other *similar* patterns than when only *different* patterns are present. In other words, the range corresponding to A will be smaller when it is necessary to distinguish between A and A' than when the distinction is between A and B. As a test, a homogeneous series of patterns was given to one group of Ss while a heterogeneous one was given to another. Both groups were instructed to learn a certain name of a color for each pattern. If the degree of similarity of the patterns within the learning series actually influences the differentiation of that series, the curve of decision-time related to a given pattern should possess peaks less widely separated for the first than for the second group.

(1) *The series of unrelated patterns.* This series contained the five patterns shown in Fig. 1, Experiment II.

The patterns were presented successively and the name of the color to be associated with each was given orally by E. After the series had been presented once, S was asked to name the appropriate color for each pattern as it was presented. When S made a mistake, he was corrected until he completed three successive errorless repetitions of the series. After meeting this criterion the Ss were given a test series which contained angles ranging in steps of 10° from 10° to 160° , the other patterns of the learning series, and some completely new ones. The Ss were instructed to give the name of each test figure, if they had learned one, and to respond "new" if the figure had not been learned. Three Ss (M, B, and H) made a total of 368 decisions, M giving five decisions for each figure, B twelve decisions, and H six.

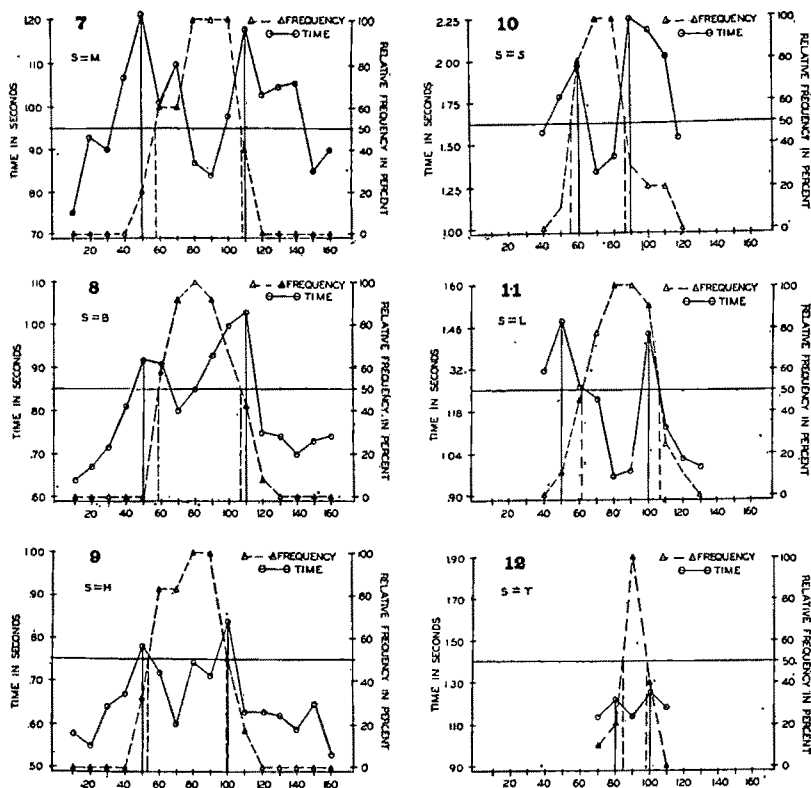
The curves showing the Ss' results are presented in Figs. 7-9. Although rather irregular, all of the curves of decision-time possess two peaks. The average distance between the peak of the curve of decision-time and the nearest 50% point is again only 4° .

(2) *The series of similar patterns.* The series containing patterns related to the standard is shown in Fig. 1, Experiment II.

Except for the selection of the patterns, the conditions of learning and the test series are the same as in the first part of this experiment. Three Ss (S, L, and T) made a total of 464 decisions, S and T each making ten decisions for each pattern and L making nine. Since three of the patterns of the learning series (the 90° , 50° , and 100° angles) lie along the continuum employed in the test series, the curves possess several peaks. The relationships of the curves will be more readily apparent, therefore, if parts of the curves are considered separately.

Figs. 10-12 present only those aspects of the curves related to the response "red" (the same response treated in Figs. 7-9). The average distance from each peak of the curve of decision-time to the nearest 50% point on the curve of frequency is 5° . The only serious discrepancy from the theoretical expectations is for L where the peak of time falls at 50° while the frequency of 50% comes at 62° .

In Figs. 13-15 are presented the curves which complete those of Figs. 10-12. Here are plotted the values related to the figures whose responses were "blue," "purple," and "new." Except for the greater irregularity,



FIGS. 7-9. DISSIMILAR PATTERNS IN EXPERIMENT II: RELATIVE FREQUENCY AND DECISION-TIME

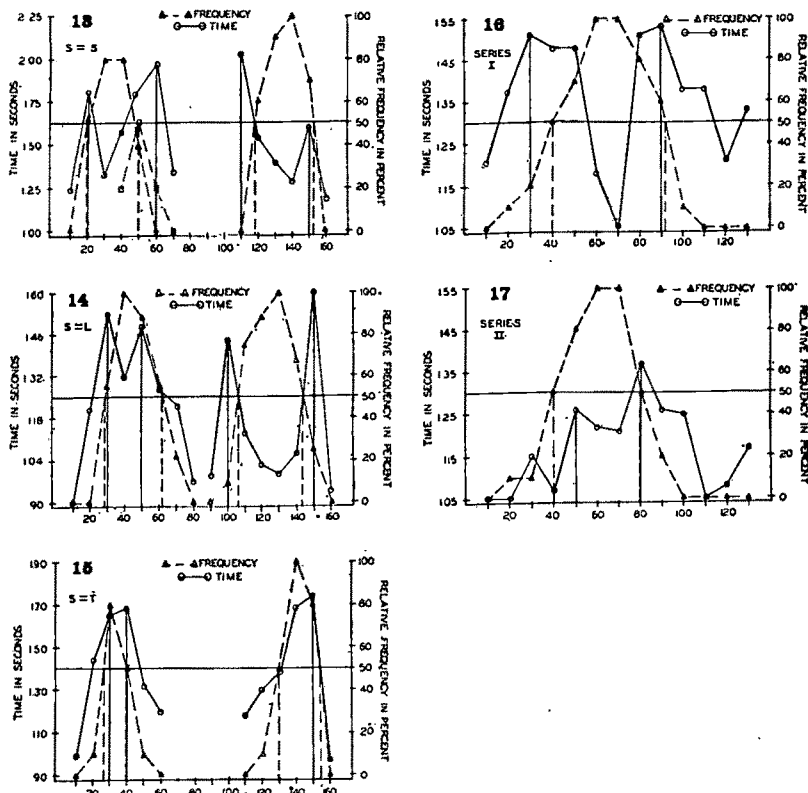
Values on abscissa indicate size of angles used in test series.

FIGS. 10-12. SIMILAR PATTERNS IN EXPERIMENT II: RELATIVE FREQUENCY AND DECISION-TIME

These curves indicate only those values related to responses of "red." See text for details. Values on abscissa indicate size of angles used in test series.

the curves display the same properties as those in Figs. 10-12. For *T*, however, the range of equivalence is so small that only one peak in the curve of decision-time is observed for each range. A comparison of the curves in Figs. 7-9 with those in Figs. 10-12 demonstrates clearly that

the range of equivalence for a given pattern is much smaller when other similar patterns are presented in the learning series than when unrelated ones are given.



FIGS. 13-15. SIMILAR PATTERNS IN EXPERIMENT II: RELATIVE FREQUENCY AND DECISION-TIME

The curves here complete those in Figs. 10-12. Values on abscissa indicate the size of the angles of the stimuli in the test series.

FIGS. 16-17. EFFECT OF REPETITION IN EXPERIMENT III: RELATIVE FREQUENCY AND DECISION-TIME

The results of the first presentation of the test series are shown in Fig. 16; those of the second presentation in Fig. 17. Values on abscissa indicate the size of the angles of the stimuli in the test series.

EXPERIMENT III: EFFECT OF REPETITION OF TEST SERIES

The naïve Ss of the last two experiments expressed considerable surprise when they suddenly realized that variations on one of the learned patterns were being presented in a systematic fashion. Several of them expressed

a determination to do better on the second presentation. It appears, therefore, that the nature of the test series creates a change of attitude which reduces the range of equivalence of the learned pattern.⁶ The present experiment was designed to investigate this change of attitude. It is of considerable theoretical interest to determine whether a change in differentiation does occur with the change of attitude since, in this experiment, the change occurred *after* learning had been accomplished.⁷

The learning series contained the eight patterns shown in Fig. 1, Experiment III. The patterns were presented successively and a number was given for each pattern. After one presentation of the Series, S was asked to respond to each pattern with the appropriate number. He was corrected whenever a mistake was made until he completed three successive errorless repetitions of the series.

The test series consisted of angles ranging in steps of 10° from 10° to 130°, the other patterns of the learning series, and some completely new patterns. S was instructed to give the number learned for each pattern, if he had learned one, and to respond "new" if the pattern had not been learned. Ten Ss made a total of 130 decisions for each of the two presentations of the test series.

Fig. 16 shows the average values for all ten Ss in the first presentation of the test series. One peak of decision-time lies at 90° and the 50% point is only 2° away. The other peak, falling at 30°, deviates 10° from the 50% point. The average deviation is, therefore, 6°.

The curves in Fig. 17 represent the values for the same Ss with the immediate repetition of the test series. The peak at 80° coincides with one 50% point, but the peak at 50° deviates 10° from the other, giving an average deviation of 5°. The lengthening of decision-time at 130° in both figures is expected, since one of the learned patterns (six) was an angle of 160° and the border of the range corresponding to it should lie at the higher end of the series. In fact, one S decided that the angle of 130° was the "six" figure.

A comparison of the curves in Figs. 16 and 17 illustrates the effect of repetition upon the differentiation. The distance between the 50% points is 52° for the first presentation and 40° for the second.

EXPERIMENT IV: USUAL VS. SPECIAL INSTRUCTIONS

The fourth experiment studies the effect of an intention to notice details

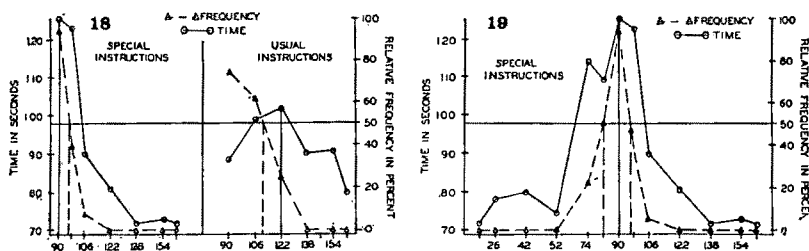
⁶ Under slightly different conditions Carter found a decrease in the range of equivalence with repetition of the test series: J. W. Carter, Jr., An experimental study of the stimulus-function, *Psychol. Rec.*, 1, 1937, 35-48. I wish to acknowledge my indebtedness to Dr. Carter (An experimental study of psychological stimulus-response, *ibid.*, 2, 1938, 36-91) for suggesting the methods employed in the present study.

⁷ A similar study of the influence of attitude upon perception is that of K. Gottschaldt, Ueber den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, II, *Psychol. Forsch.*, 12, 1929, 1-87.

established *prior* to the process of learning. It was assumed that instructions asking *S* to notice details, given before the learning occurs, would reduce the range of equivalence corresponding to each learned pattern.

The learning series contained the four patterns shown in Fig. 1, Experiment IV, and a mirror-image of each. The *Ss* were asked to learn to designate one pattern as the basic one and the other as the mirror-image, responding "one" to the former and "two" to the latter. The criterion of learning was three successive errorless repetitions of the series. Prior to the learning, one of the two groups was given additional instructions to notice carefully any small details about the patterns which they thought might prove to be of importance in a later test.

The test series was composed of variations on the first pattern in Fig. 1, Experiment IV, the other figures of the learning series, and some completely new patterns. For the variable patterns angles of 90° , 106° , 122° , 138° , 154° , and 162° were used. This variation provided a test of the border of the range on only one side. As a check to determine whether the curves would be symmetrical, one group was also



FIGS 18-19. SPECIAL AND USUAL INSTRUCTIONS IN EXPERIMENT IV: RELATIVE FREQUENCY AND DECISION-TIME

Values on abscissa indicate the size of the angles of the stimuli of the test series.

given, as variations on the other side of the standard, angles of 82° , 74° , 58° , 42° , 26° , and 18° . *S* was asked to decide whether a given pattern was 'learned' or 'new.' Seven *Ss* made a total of 300 decisions. Four *Ss* made up the group which had the 'usual instructions, each *S* making two decisions for each test pattern. The group with special instructions was composed of 3 *Ss* each of whom made six decisions for each test figure.

Figs. 18-19 shows the curves for both groups, deviation being considered in only one direction. For the group with usual instructions the curve of decision-time reaches a peak at 122° . Thus, if the curve were extended symmetrically on the other side of the 90° angle, the typical bimodal curve would be obtained. Comparing the curves of decision-time and of frequency, a distance of 11° between the maximal decision-time and the 50% point is noted. This distance is less than one full step of the test series. A comparison of the curves for the two groups shows that additional instructions to notice details, given during the learning period, result in a decrease of the range of stimuli corresponding to the learned

pattern. The distance from the 90° angle to the 50% point is 20° for the group with the usual instructions, while it is only 6° for the other group.

The complete curve for the *Ss* with special instructions is given in Fig. 19. According to the basic hypothesis the curve of decision-time should show peaks at 82° and 98° . It is not known whether more repetitions of the test would bring about a reduction in the average time for 90° and an increase for 82° . These two values would have to change, provided the relative frequency remained the same, if the basic hypothesis is strictly correct.

EXPERIMENT V: LARGE VS. SMALL FRAME OF REFERENCE

In a previous study,⁸ it was demonstrated that the sensitivity to spatial location decreases as the distance of the stimulus from the frame-of-reference increases. In other words, the size of the range of stimuli leading to the response "the same location" increases as the frame-of-reference is removed from the area being tested. In the present experiment, instead of measuring the sensitivity to spatial location, the sensitivity to spatial extent is investigated within two frames-of-reference of different sizes. It is expected that the range of equivalence is larger when the test is made within a large frame-of-reference than when it is conducted within a small one.

Two *Ss* were tested for differential sensitivity to lengths of lines under two different conditions. In one case, the lines were presented within a square black frame whose dimensions were 90×90 mm. In the other, the dimensions were 45×45 mm. A standard horizontal line, 30 mm. in length and 0.5 mm. wide, and six variable lines, 26, 28, 29, 31, 32, and 34 mm. long, were presented in each frame.

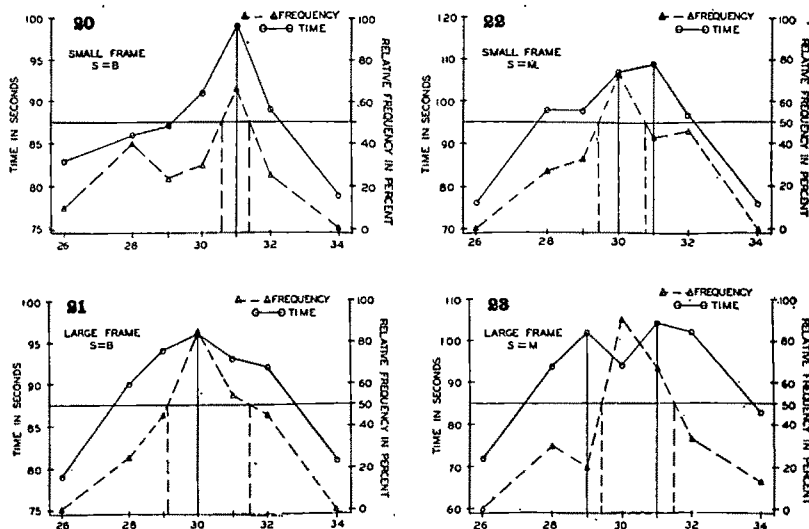
One *S* (*B*) was tested by the method of constant stimuli. Each stimulus was presented for 0.5 sec. with an interval of 3.5 sec. between the standard and the variable. Employing three categories of response, five decisions were made for each pair of lines within one frame, then five decisions were made within the second, the alternation continuing until 20 decisions were made for each pair within each frame. A total of 289 decisions was thus obtained.

The other *S* (*M*) was tested by the method of single stimuli. First, *S* was shown the standard stimulus five times in succession. Then, the lines (including the standard) were presented successively. The frame-of-reference was changed after each stimulus had been presented five times. *M* made 30 decisions for each stimulus in each frame, giving a total of 420 decisions.

The curves in Figs. 20-21 present the results for each frame-of-reference using the method of constant stimuli. For the small frame the 50% points lie closer to the peak of the curve of decision-time than to any other points, giving an average deviation of 0.4 mm. According to the basic

⁸ Dorwin Cartwright, On visual speed, *Psychol. Forsch.*, 22, 1938, 320-342.

hypothesis, the curve for the large frame should possess two peaks, falling at 29 mm. and at 31 mm. or 32 mm., since the 50% points lie closest to these values. The value for 30 mm., however, is now greater than the hypothesis predicts. A comparison of the curves of frequency for the two conditions shows that the distance between the 50% points is greater for the large than for the small frame; consequently, the curve of decision-



FIGS. 20-21. METHOD OF CONSTANT STIMULI: RELATIVE FREQUENCY AND DECISION-TIME

Values on abscissa indicate length (mm.) of lines used as variable stimuli.

FIGS. 22-23. METHOD OF SINGLE STIMULI: RELATIVE FREQUENCY AND DECISION-TIME

Values on abscissa indicate length (mm.) of lines used as variable stimuli.

time should be flatter for the former than for the latter. An inspection of the two curves corroborates this expectation.

Figs. 22-23 give the results obtained with the method of single stimuli. For the small frame the 50% point lying nearest a stimulus is 0.2 mm. from 31 mm., the peak of the curve of decision-time. The second longest time falls at 30 mm., which is 0.4 mm. from one 50% point and 0.8 mm. from the other. Another case supporting the basic hypothesis is illustrated by the curves for the large frame where the curve of decision-time is bimodal. The peak at 29 mm. is 0.4 mm. from the nearest 50% point, while the peak at 32 mm. lies 0.5 mm. from the other.

Considering both Ss together, the average distance between the 50% points is 1.0 mm. for the small frame and 2.2 mm. for the large, giving a ratio of slightly more than 2 to 1, which is that of the linear dimensions of the two frames. When the size of the frame-of-reference is varied, sensitivity to spatial extent, therefore, appears to be influenced in the same way as sensitivity to spatial location.⁹

EXPERIMENT VI: WORDS AS RANGES OF EQUIVALENCE

The experiments reported up to this point have been concerned with the organization produced by learning during the experimental session or by perceptual processes. The sixth experiment represents a similar study of the Ss' normal use of words. It is proposed that a word is a symbol which refers to a range of objects and that decision-time will be maximal at the border of such a range just as in the previous experimental situations.

At the beginning of the experiment the Ss were given a sentence with instructions to arrive at a clear formulation of the meaning of the various words in it. Then, they were shown some new words, which were to be substituted for one of the words of the sentence. Upon seeing each word, S was required to decide whether the substitution of the new word changed the basic meaning of the sentence. Two different sentences were selected so that a word common to both would relate to different ranges of meaning, thus creating different differentiations of the test series.

Ten Ss were given the two sentences and the test-series on different days.¹⁰ Sentence I was: *Yesterday I saw a huge building*. Sentence II was: *Yesterday I saw a huge man*. The test-series contained the following 10 words: 1. *immense*, 2. *grand*, 3. *great*, 4. *vast*, 5. *colossal*, 6. *large*, 7. *magnificent*, 8. *big*, 9. *mighty*, 10. *massive*. It was supposed that a list of words which would be appropriate substitutes for *huge* in Sentence I would possess certain words which would not be appropriate substitutes in Sentence II. It is not proposed that Sentence II limits the total number of appropriate substitutes for *huge*, but rather the number of appropriate substitutes within the test series employed in this experiment. The test series was presented six times to each S. Since ten Ss were tested on both sentences, 1200 decisions were recorded.

Seven of the 10 Ss were tested first on Sentence I and on Sentence II ten days later. Since Experiment III demonstrated that repetition of the test series tends to reduce the size of the range of equivalence, it was necessary to balance the order of the sentences. Three additional Ss were given Sentence II, therefore, before they were tested on Sentence I.

When words are employed as stimuli, it becomes impossible to define the continuum of variation in purely physical terms. In view of this difficulty some new method of locating units along the abscissa of the scale is necessary. A rank-order of similarity, obtained after S has made his decisions, provides only a partially satisfactory measure since it represents the order of similarity as it exists at the end of

⁹ Cartwright, *op. cit.*, 328.

¹⁰ The author is indebted to Mr. E. M. Jandórf for conducting this experiment in connection with a study of his own on a similar problem.

TABLE I
AVERAGE DECISION-TIME AND THE FREQUENCY OF DECISIONS OF "NO CHANGE"
FOR EVERY TEST-WORD

FOR EVERY TEST-WORD												Distance 50% point to peak	Distance 50% point: I minus II		
S	Sentence	Test words													
		1	2	3	4	5	6	7	8	9	10				
Ho	I	Time	138	151	230	322	258	217	280	262	256	245	0.5	+0.5	
		%	100	100	100	67	33	33	17	00	00	00			
		Time	155	141	158	239	170	155	163	163	167	159			
Cr	II	%	100	100	100	50	00	00	00	00	00	00	0.0		
		Time	144	93	94	105	97	87	135	140	140	124			
		%	100	100	100	100	100	100	100	33	17	00			
De	I	Time	88	82	130	131	184	107	106	120	105	106	0.8	+3.2	
		%	100	100	100	83	00	00	00	00	00	00			
		Time	88	105	107	158	125	189	239	344	154	176			
Ba	II	%	100	100	100	100	100	100	33	17	17	00	1.2	+1.0	
		Time	163	196	288	417	409	685	401	214	174	162			
		%	100	100	100	100	100	33	17	00	00	00			
Ba	I	Time	337	413	494	1326	619	744	300	506	414	176	0.6	+0.7	
		%	100	100	100	83	33	17	00	00	00	00			
		Time	347	282	309	474	185	325	242	178	210	133			
Bt	II	%	100	100	100	50	00	00	00	00	00	00	0.0	+0.0	
		Time	269	174	181	351	367	354	243	189	292	213			
		%	100	100	100	100	50	00	00	00	00	00			
Wi	I	Time	188	201	439	283	393	260	291	255	212	212	0.0	+1.5	
		%	100	100	100	00	00	00	00	00	00	00			
		Time	133	123	114	130	152	159	164	201	164	116			
We	II	%	100	100	100	83	83	83	67	33	17	17	0.5	+5.0	
		Time	102	113	128	89	82	94	78	98	93	85			
		%	100	83	17	17	00	00	00	00	00	00			
We	I	Time	108	113	135	131	149	166	158	125	152	136	1.0	+1.7	
		%	100	100	100	100	100	100	50	17	00	00			
		Time	92	204	181	205	204	156	197	121	136	128			
Wo	II	%	100	100	83	83	83	00	00	00	00	00	1.3		
		Time	111	294	410	191	115	132	113	116	167	112			
		%	100	33	00	00	00	00	00	00	00	00			
Ki	I	Time	79	176	140	226	84	94	71	80	70	84	1.2	+1.2	
		%	100	100	17	00	00	00	00	00	00	00			
		Time	145	212	165	134	216	179	171	178	167	160			
Br	II	%	100	100	100	100	17	17	17	00	00	00	0.3	+1.3	
		Time	157	163	165	158	180	182	192	188	188	168			
		%	100	100	100	100	83	50	17	00	00	00			
Br	I	Time	95	124	83	94	97	85	98	101	98	135	1.2	-3.0	
		%	100	100	100	100	100	100	100	100	33	00			
		Time	86	85	84	108	97	111	98	112	91	123			
		%	100	100	100	100	100	33	17	17	17	00	2.2	+1.9	
		Average: First 7 Ss. 0.6													
		Average: Last 3 Ss. 1.2													
		Average: All Ss. 0.7													

the experiment. In order to obtain a better measure of the similarity as it actually functions during the time the decisions are made, the relative frequency of response was used in conjunction with the rank-order. When several words gave the same

relative frequency, the rank-order was used to determine the location of each word on the scale.

Table I presents the average decision-time and the frequency of decisions of "no change" for every test-word. In general, the curves are more irregular than in the previous experiments. Yet, there is a marked tendency for the peak of the curve of decision-time to fall near the 50% point on the curve of frequency. The values in the next to the last column indicate the distance between these two points. The average distance is 0.7 units. Only 8 of the 20 comparisons yield a deviation of one unit or more, the largest deviation being 2.2 units.

The values in the last column indicate the differences between the two sentences in the size of the range of equivalence. A *plus* value indicates that the distance between the two 50% points is greater for Sentence I than for Sentence II. The average decrease is 1.3 units. The special hypothesis of this experiment seems, therefore, to be supported.

EXPERIMENT VII: POLITICAL ATTITUDES AND EQUIVALENCE

The seventh experiment represents an exploration into the applicability of the general methods of the previous experiments to the study of political attitudes. It is proposed that, for a person who considers himself 'radical,' the size of the range of stimuli corresponding to 'radicals' will be smaller than that corresponding to 'conservatives,' and that the opposite will hold for a conservative S. If a list of names of political figures is presented to the radical S, then one should expect him to classify only a few names as 'radical' and, in accordance with the basic hypothesis of all the experiments, the decision-time should be longer where the decisions change from one category to another. Since the major purpose of the present study is to determine the relationship between the border of a range of equivalence and decision-time, only a preliminary study was made of the differences in attitudes.

Of the two Ss who were used, one considered himself to be a 'good radical' and the other a 'solid conservative.'¹¹ A list containing names of 20 political figures was constructed so that a wide range would be represented along a radical-conservative scale.¹² The names were printed on separate cards and presented successively in the tachistoscope. The Ss were instructed to respond "radical," "conservative," or "liberal" upon the presentation of each card, depending upon their judgment of the

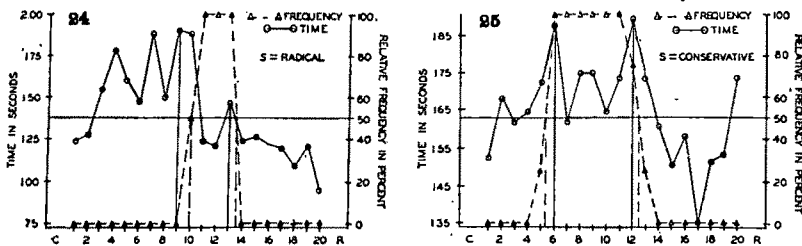
¹¹ The author is indebted to Mr. John Harding for selecting the Ss upon a basis of tests which he has not yet published.

¹² The list contained the following names: Wagner, Hull, LaGuardia, Roosevelt, Green, Franco, Coughlin, Hoover, Garner, Dewey, Dies, Chamberlain, Lenin, Blum, Lewis, Bridges, Browder, Trotsky, Thomas, and Stalin.

proper classification of the name. Each card was presented four times to each *S*, making 160 decisions for the 2 *Ss*. The problem of defining the continuum is essentially the same here as in the last experiment and is treated in the same manner.

The curves for the radical *S* are given in Fig. 24. The left side represents the conservative end of the scale, and the right the radical. The curve of decision-time possesses two major peaks (4 and 7) which are not accounted for by the basic hypothesis. *S* reported that these two political figures did not fit well into any of the three categories. Considering the basic hypothesis, the 50% points fall at 10 and midway between 13 and 14, while the peaks of the curve of decision-time fall at 9 and 13.

Fig. 25 shows the curves for the conservative *S*. Here only one value



FIGS. 24-25. POLITICAL ATTITUDES: RELATIVE FREQUENCY AND DECISION-TIME Values along abscissa represent *Ss*' rank-order of stimuli from most conservative to most radical. In Fig. 24 *S* is radical. In Fig. 25 *S* is conservative.

(20) deviates extremely from the theoretical expectancy. The 50% points fall between 5 and 6, and between 12 and 13, while the peaks of the curve of decision-time fall at 6 and 12.

A comparison of Figs. 24 and 25 provides a test of the special hypothesis of this experiment. While it is impossible to draw any generalizations from only two *Ss*, it will be noted that the curves follow the expectations. For the radical *S*, the judgments of 'radical' extend 7.5 units, while the judgments of 'conservative' extend 10 units. For the conservative *S*, on the other hand, the judgments of 'radical' extend 8.5 units, while the judgments of 'conservative' extend only 5.2 units.

DISCUSSION

(1) *Decision-time and the similarity of alternatives.* Reference was made at the beginning of this paper to the unanimity of agreement that, when decision-time is plotted without consideration of the category of response, it increases with an increase in the similarity of the alternatives.

The experiments just described demonstrate that this generalization must be restricted. *Decision-time is maximal at the border of a range of equivalence, and a simple relationship to similarity holds only when this border coincides with objective equality of the alternatives.* When three categories of response are employed, for example, there are two such borders and the

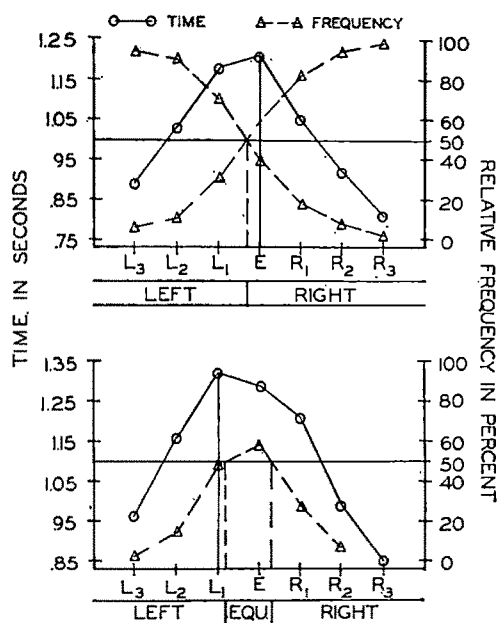


FIG. 26. TWO AND THREE CATEGORIES OF RESPONSE: RELATIVE FREQUENCY AND DECISION-TIME

Curves of time are reproduced from W. N. Kellogg, *The time of judgment in psychometric measures*, this JOURNAL, 43, 1931, 72. The curves of frequency are derived from a table in Kellogg, *op. cit.*, 78. At the bottom of each curve are indicated the categories of response.

curve of decision-time possesses two peaks. Why, then, have other investigators concluded that decision-time always increases with an increase in similarity?

In answering this question, it is convenient to analyze the data of an experiment in which the above generalization was made. In Kellogg's study are compared curves of decision-time obtained with two categories of response and with three.¹³ These curves are reproduced in Fig. 26 to-

¹³ W. N. Kellogg, *op. cit.*, *passim*.

gether with the relative frequencies which have been taken from another table in Kellogg's study. Below each curve is drawn the location of the borders as defined by the 50% points. Making reference to these curves, one can understand how Kellogg's results arose. When two categories are employed, the curve of decision-time should possess one peak located at the border of the range. Since the 50% point does not coincide precisely with any of the stimuli employed, the peak should fall at the value closest to that point. Value E has this location and does actually constitute the peak of the curve. The rest of the values also closely follow the theory. L_1 , being the value next closest to the 50% point, constitutes the second longest time; and the rest of the curve falls off as the distance from the border increases.

For three categories a similar analysis can be made. L_1 lies closest to a 50% point and constitutes the peak of the curve of decision-time; the second closest value is E, the location of the second longest time; the third closest value is R_1 , the third longest time; and the rest of the curve falls off as the distance from the borders increases. It is now clear that Kellogg did not obtain a bimodal curve with three categories because stimulus E was closer to the border than R_1 . Actually, deviation from E toward L_3 did yield a curve in contradiction to Kellogg's general conclusion, but the difference was small enough to be attributed to some extraneous factor. Kellogg's data, then, based upon 3360 decisions, offer no difficulty to the theory; but, on the contrary, provide support in addition to that afforded by the experiments of the present study.

(2) *Decision-time and the number of categories.* From the discussion above, it is clear that the shape of the curve of decision-time is directly affected by the number of categories which S employs. In addition, however, the theory predicts a relation between the number of categories and the average decision-time for all decisions. If, along a given set of stimuli, two ranges are established, there will be one border, and the curve of decision-time will possess only one peak. As a result the *average time* will be raised slightly above the lower values of the curve. If, on the other hand, three ranges are established, there will be two borders and the curve of decision-time will possess two peaks, raising the average time correspondingly more. As the differentiation is further increased, more and more peaks will occur, and as a consequence the average time will increase still further. Kellogg's data confirm this expectation. He concludes that the average decision-time is about 10% longer when three categories are employed than when there are only two.¹⁴

¹⁴ Kellogg, *op. cit.*, 85.

(3) *Decision-time and judgments of equality.* The problem of the inclusion of the equal-category in psychophysical experiments has a long history.

The earlier use of the psychophysical methods followed the common-sense procedure of including the category *equal*; but, as Titchener pointed out, the statistical difficulties in handling the category became pressing; and its rejection has been advocated from time to time since 1884.¹⁵ Many detailed introspective studies have been used in attacking or justifying the use of the category. As early as 1899, Martin and Müller¹⁶ combined an introspective study with observations upon the times required in making the judgments of the various categories, and concluded that the judgment of equality was frequently intended to mean "no difference" rather than to be a report of actual equality. The whole character of the experience was fused with doubt and the time required in making the judgment was longer than for the categories of difference.

In 1917 George¹⁷ found that judgments of equality require longer times than judgments of difference, but he advocated only the elimination of doubt by instruction, not the elimination of the category from experiments. Fernberger,¹⁸ in 1919, reported a similar introspective study in which he concluded that equality judgments almost invariably entail doubt and that they require longer times than judgments of difference.

In 1931, Kellogg¹⁹ studied the average decision-times for each of the three categories of response and also found that the judgments of equality require longer times than judgments of difference. In accounting for the difference in times, Kellogg concluded: "We seem forced to the conclusion that there is some further factor, as yet not clearly perceived, which makes the equal judgment more difficult and hence slower to render. . . . At present this factor can be regarded as an intrinsic characteristic of the equality judgment itself."²⁰

In the following year, Fernberger and Irwin²¹ published frequencies of response and average times for each of the three categories when using the method of single stimuli. These authors found that the intervals of uncertainty were invariably larger than those obtained by the relative method, and that there was *no* significant difference in decision-times between judgments of equality and judgments of difference.

¹⁵ The best history of the earlier treatment of the problem is to be found in E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 285 ff. A more recent survey of the problem is found in S. W. Fernberger, The use of equality judgments in psychophysical procedures, *Psychol. Rev.*, 37, 1930, 107-112. The statistical difficulties involved are discussed most clearly in E. G. Boring, The psychophysics of color tolerance, this JOURNAL, 52, 1939, 384-388.

¹⁶ L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 1-96.

¹⁷ S. S. George, Attitude in relation to psychophysical judgment, this JOURNAL, 28, 1917, 1-37.

¹⁸ S. W. Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 117), 161.

¹⁹ Kellogg, *op. cit.*, *passim*.

²⁰ *Op. cit.*, 82.

²¹ S. W. Fernberger and F. W. Irwin, Time relations for the different categories of judgment in the "absolute method" in psychophysics, this JOURNAL, 44, 1932, 505-525.

Carlson, Driver, and Preston²² repeated the experiment of Fernberger and Irwin in 1934, using the method of constant stimuli in order to determine whether the longer times for the central category were due to the psychophysical techniques employed. No significant difference was found between the times for the three categories. The authors accounted for the difference between their results and those of George and Kellogg on a basis of attitude. "In both George's and Kellogg's experiments, it would seem that there was set up an attitude against giving equality judgments. . . . All observers in the present experiment had been previously trained in an attitude in which the equal judgment was accepted as a category equally valid and quite on a par with either of the categories of difference judgments."²³

In the same year, Fernberger, Glass, Hoffman, and Willig²⁴ repeated the experiment of Carlson, Driver, and Preston, except that they created an attitude in the Ss which was designed to keep them from giving a judgment *equal* except when absolutely necessary. The authors concluded: "It has been found that, with the use of an experimental arrangement such as that employed by Fernberger and Irwin and by Carlson, Driver, and Preston, who both obtained no difference between the judgment times for the three categories of psychophysical judgment, that the judgment times for the equal judgment become significantly longer, on the whole, than judgments of either less or greater if the attitude is created in the observers of not giving equal judgments unless absolutely necessary. . . . It would seem that this experiment adequately explains the differences in the results of judgment times found by Fernberger and Irwin and by Carlson, Driver and Preston, on the one hand, and by Martin and Müller, by George and by Kellogg, on the other."²⁵

Although the experiments combine to demonstrate the importance of attitude in the determination of decision-time, very little explanation is provided by the use of the word *attitude*. The general conclusion of the studies appears to be that somehow the attitude "not to give a judgment of equal unless absolutely necessary" slows down the judgment *equal* when it is made; but it is not clear just how the process works.

It is here proposed that these experimental results can best be understood in terms of differentiation. The crucial factor determining decision-time is the location of the borders of the ranges of stimuli corresponding to the categories. If the distance between the 50% points is small, the stimuli which yield decisions of *equal* will fall, on the average, near these points and the average decision-time for decisions of equality will, therefore, be high. If the distance between the borders of the central range is made larger, a larger proportion of the stimuli will fall some distance

²² W. R. Carlson, R. C. Driver and M. G. Preston, Judgment times for the method of constant stimuli, *J. Exper. Psychol.*, 17, 1934, 113-118.

²³ *Op. cit.*, 117.

²⁴ S. W. Fernberger, E. Glass, I. Hoffman and M. Willig, Judgment times of different psychophysical categories, *J. Exper. Psychol.*, 17, 1934, 286-293.

²⁵ *Op. cit.*, 293.

from the borders and the average decision-time for decisions of equality will be reduced. Unfortunately, the studies mentioned above do not give the curves of relative frequency and of decision-time. In accordance with most extreme class-theory, all times for a given class have been lumped together and discussed as if the class were a separate entity. There is indirect evidence, however, that the effect of attitude upon decision-times functions by means of differentiation. Fernberger²⁶ and others have shown that a change in attitude also changes the range of judgments of equality. It appears, then, that the attitude determines the size of the range of equality and, consequently, the average decision-time.

A comparison of Figs. 2-4 and 5-6 provides an illustration of the point. In the experiment from which these curves were obtained, the attitude of the Ss was kept constant for both experimental conditions, and the size of the central range was controlled by previous learning. The average time of judgments corresponding to the central category differs greatly for the two situations. Attitudes serve to render stimuli equivalent, and the differentiation which they produce is a sufficient consideration for understanding their effect upon decision-time in the situations treated in the present study.

(4) *Decision-time and doubt.* Many studies have been concerned with the relation between the experience of doubt and the length of decision-time.²⁷ The conclusion, that the greater the doubt the longer the decision-time, is so generally accepted that recent authors have attempted to find a precise function between the two variables. Johnson²⁸ has found that doubt increases as the threshold of the category is approached, that the decision-time increases with an increase of doubt, and, consequently, that "judgment time becomes greater as the category threshold is approached."²⁹ It is readily apparent that the conclusions of Johnson are in agreement with those of the present study.

George studied the relationship between doubt and decision-time by introducing a category of *doubtful* judgments.³⁰ He found that judgments of this category required significantly longer times than judgments *equal*,

²⁶ S. W. Fernberger, The effect of the attitude of the subject upon the measure of sensitivity, this JOURNAL, 25, 1914, 538-543.

²⁷ All of the studies cannot be listed here. The following, however, are representative: V. A. C. Henmon, Time and accuracy of judgment, *Psychol. Rev.*, 18, 1911, 186-201; G. H. Seward, Recognition-times as a measure of confidence, *Arch. Psychol.*, 99, 1928, 1-52; and J. Volkmann, The relation of the time of judgment to the certainty of judgment, *Psychol. Bull.*, 31, 1934, 672-673.

²⁸ D. M. Johnson, Confidence and speed in the two-category judgment, *Arch. Psychol.*, 241, 1939, 1-52.

²⁹ *Op. cit.*, 43.

³⁰ S. S. George, *op. cit.*, *passim*.

greater, or *less*. He concluded that a special attitude was operative when judgments *doubtful* were given and that, as a result, they should be excluded from computations of the limen. George's data do not give the distributions of the judgments, but it may be assumed that the range corresponding to the category *doubtful* is extremely small, since *Ss* are usually under pressure to avoid giving them, and that the average distance of the stimuli from the borders of the range is, therefore, correspondingly reduced. The situation then becomes essentially the same as that discussed in relation to the problem of judgments of equality and a longer average time for judgments *doubtful* is expected.

SUMMARY

The seven experiments combine to support the following generalization: the time required for deciding to which category a stimulus belongs increases as the stimulus approaches the border of a range of equivalent stimuli. Since the border of such a range is defined as that stimulus which yields two responses equally often, the generalization is tested by plotting decision-time against a series of stimuli and by comparing the location of maximal time with the location of the stimulus which yields a relative frequency of 50%. Seventy-one such comparisons, based upon 4188 decisions, give an average distance between the two locations of 0.6 units of the stimulus-scale.

In order to give the generalization a broad empirical basis, ranges of equivalence of various sizes were produced in the following ways. (1) The size of the range was controlled by instructing *S* to consider an arbitrary range of stimuli as belonging to the same category. (2) The presentation of other similar nonsense patterns in the learning series reduced the size of the range related to a given pattern. (3) Repetition of the test produced a reduction in the size of the range related to a given pattern. (4) Special instructions to notice details, given prior to the learning series, reduced the size of the range related to a given pattern. (5) The size of the range of stimuli related to a given perceived length (an inverse measure of differential sensitivity) was reduced by reducing the size of the frame-of-reference. (6) By including a given word in different sentences, the size of the range of equivalence related to that word was modified. (7) The size of the range of equivalence related to 'a radical' and to 'a conservative' differed, depending upon *S*'s own political attitudes.

This generalization sheds new light upon several older problems: First, the relationship between decision-time and the similarity of alternatives must be revised to account for the number of categories employed in the

INDIVIDUAL DIFFERENCES IN LEARNING TO REPRODUCE FORMS: A STUDY IN ATTENTION

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In the learning of handwriting, shorthand, art, and other subjects, the ability to learn and reproduce complex forms is important. The nature of such learning has been studied in many investigations, of which the most complete are those of Judd and Cowling,¹ Tripp,² Binet,³ Stratton,⁴ Piéron,⁵ Albein,⁶ and Kuhlmann.⁷ Many of these studies have been based on extremely small groups of subjects.

Judd and Cowling analyzed drawings made by 8 Ss, to determine how a percept develops. Each figure was reproduced by S after successive 10-sec. exposures. Judd and Cowling assumed that changes in the reproduction from trial to trial reflected S's distribution of attention on each trial. Thus, a gain at any point implied that S had studied that point, and a loss was taken to mean a withdrawal of attention from that point.

The present study is a repetition of the experiment by Judd and Cowling, designed to extend its validity and to check their assumption. Photographic eye-movement records were made during the study of three diagrams; comparison of these records with the reproductions makes it possible to determine more validly the methods of study.

Few experiments have studied eye-movements during the learning of form. Rubin tested the hypothesis that sensations of tracing a figure mentally during study lead to eye-muscle sensations which can be recalled.⁸ He concluded that "what we had

* Accepted for publication June 30, 1940. This study is condensed from a doctorate dissertation from the Department of Education, University of Chicago. The author is indebted to Professor G. T. Buswell for guidance. A preliminary experiment was conducted at the University of California at Berkeley, under the supervision of Professor L. C. Gilbert.

¹ C. H. Judd and D. J. Cowling, Studies in perceptual development, *Psychol. Monog.*, 8, 1907, (no. 34), 349-369.

² M. G. Tripp, Perceptual learning at different grade levels, Univ. of Chicago, 1926 (unpublished thesis).

³ Alfred Binet, Attention et adaptation, *Année Psychol.*, 6, 1899, 248-404.

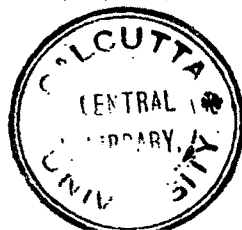
⁴ G. M. Stratton, *Experimental Psychology and Its Bearing Upon Culture*, 1908, 176 ff.

⁵ Henri Piéron, Recherches comparatives sur la mémoire des formes et celle des chiffres, *Année Psychol.*, 21, 1920, 119-148.

⁶ Gustav Albein, Der Anteil der nachkonstruierenden Tätigkeit des Auges und der Apperception an dem Behalten und der Wiedergabe einfacher Formen, *Zsch. f. exper. Pädagogik*, 5, 1907, 133-156; 6, 1908, 1-48; and suppl. to Vol. 6, 1907.

⁷ F. Kuhlmann, On the analysis of the memory consciousness: a study in the mental imagery and memory of meaningless visual forms, *Psychol. Rev.*, 13, 1906, 316-348.

⁸ Edgar Rubin, *Visuell wahrgenommene Figuren*, 1921, 153 ff.



hitherto supposed, that these sensations are connected with the movements of the eyes, is proven false, and all assertions about eye movements which rest upon these sensations are undemonstrable." Stratton⁹ and Buswell¹⁰ have photographed the eyes while *S* looked at forms or traced them visually. Brandt photographed eye-movements while *S* studied a symmetrical pattern with intent to reproduce.¹¹ He was primarily interested in the attention-values of parts of the figure, and disregarded relations between study methods and reproductions. Guilford and Hackman also used eye-movement records to study the attention-values of stimuli.¹² These studies are agreed that *S* may attend to a point in the figure without fixating it foveally. It appears reasonable, however, to assume that any shift of fixation during study is toward a position of greater attractiveness, and that the eye-movement record gives a close approximation to the point given attention.

The specific purpose of the present study is to investigate the following questions: (1) Is the assumption of Judd and Cowling valid; namely, that the distribution of attention is indicated by *S*'s reproduction? (2) What characteristics distinguish the study patterns of good and poor learners? (3) What changes in study pattern accompany a change in the difficulty of the figure studied? (4) Are individual differences in ability to learn figures constant from figure to figure? (5) How does such ability correlate with mental abilities measured by American Council Psychological Examination? (6) Are the 'types' of study defined by Judd and Cowling, those defined by Albein, Katz, and others, or those defined by Wulf, characteristic of individuals throughout their study of several figures?

Seven of Judd and Cowling's *Ss* drew the form as a whole on the first trial, and refined details in later reproductions. One *S* drew only part of the figure on the first trial, attending to details. He augmented this drawing piecemeal on further trials. These differences were thought by Judd and Cowling to indicate 'whole' and 'part' procedures in studying, respectively. Albein, Katz,¹³ and other workers classified their *Ss* as 'objective' or 'subjective.' 'Objective' learners formed visual impressions, using no associations during study, while 'subjective' learners always relied on associations in studying nonsense forms. While Albein noted that some persons are between these extreme positions in behavior, Katz and his followers have been inclined to view the classification as dichotomous. Wulf,¹⁴ on the other hand, denied the validity of Katz' types, and classified learners as 'isolative' or 'comprehensive,' depending on the type of association they employed. Wulf held that every learner

⁹ G. M. Stratton, Eye-movements and the aesthetics of visual form, *Philos. Stud.*, 20, 1902, 336-359.

¹⁰ G. T. Buswell, *How People Look at Pictures*, 1935, 1-198.

¹¹ H. F. Brandt, Ocular patterns and their psychological implications, this JOURNAL, 53, 1940, 260-268.

¹² J. P. Guilford and R. B. Hackman, Varieties and levels of clearness correlated with eye-movements, this JOURNAL, 48, 1936, 371-388.

¹³ David Katz, Über individuelle Verschiedenheiten bei der Auffassung von Figuren, *Zsch. f. Psychol.*, 45, 1913, 161-180.

¹⁴ F. Wulf, Über die Veränderung von Vorstellungen, *Psychol. Forsch.*, 1, 1922, 33-373.

uses associations at times during learning. If any of these three trait descriptions is to be accepted, it becomes necessary to demonstrate (1) that such differences do exist, and (2) that learners belonging to a given type on one figure will belong to the same type on other figures.

METHOD

In repeating the Judd-Cowling experiment, the following methodological changes were made: (1) the number of Ss was increased to 118; (2) the stimulus-objects were presented to Ss in groups as well as individually; (3) high school pupils instead of graduate students were used as Ss; and (4) stimulus-figures of varying, instead of uniform, difficulty were employed. The procedure in this investigation falls into two parts: (I) a group experiment with an unselected group of Ss; and

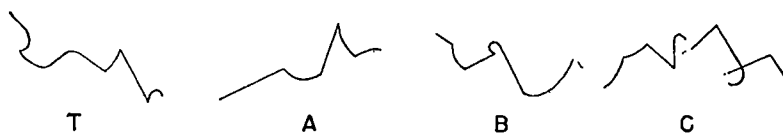


FIG. 1. STIMULUS-FIGURES USED IN THE GROUP EXPERIMENTS

(II) an individual test of selected good and poor learners whose eye-movements were photographed during the study.

(I) *Group experiment.* (1) *Subjects.* All members of the ninth and tenth grades of University High School, Chicago, Illinois, present at the time of the test served as Ss. There were 40 boys and 78 girls, 118 in all.

(2) *Stimulus-figures.* Four line diagrams, T, A, B, and C, were used as the stimulus-figures (see Fig. 1). They were designed to give a minimum of regularity or pattern and to provide a range of difficulty. They were also designed so that they could be drawn with a minimum of left-to-right movement or doubling back; this made the horizontal eye-movement records more interpretable and increased the resemblance to shorthand symbols. Diagram T was used as a specimen while instructing the Ss. Each diagram was drawn on groundglass, and presented by projection on the front wall of a semi-darkened room.

(3) *Procedure.* Directions were as follows:

We are investigating the methods of study you use in learning. After I say 'Ready,' you will be shown a figure like this on the screen for a brief interval. [T projected on screen.] When I turn off the light, draw it as well as you can on the piece of paper you have been given. Keep as near the correct shape of the figure as possible, drawing as much as you can remember. Study carefully, paying attention to the size of angles, to whether the lines are straight or curved, and to how their lengths compare with each other.

After you have completed your drawing, you will be given another chance to study the figure. Then you will draw it on another of your sheets of paper. Enough trials will be made so that most of you will be able to reproduce the figure perfectly.

Before the test, every S was given 30 small sheets of paper. The stimulus-figures were presented in the order of difficulty—A, B, C—for 7, 10, and 10 trials respectively. Every exposure was of 10-sec. duration.

(4) *Scoring.* The following arbitrary rules (adapted from Judd and Cowling) were used in scoring. Each of the following was scored as one gross error.

- (a) An added line, or closure of a gap present in model figure.
- (b) A straight line shown as curved, or a curved line shown as straight.
- (c) A convex curve shown as concave, or vice versa.
- (d) A line over one and one-half times its proper size, as compared to adjacent lines; or a line less than two-thirds its proper size, as compared to adjacent lines.
- (e) An angle showing an absolute error of 60° from its proper size, or a corner rounded, or an angle added to the model figure.
- (f) A curve having a radius of curvature over twice its proper size, or under half its proper size, as compared to its length.
- (g) In a discontinuous figure, a marked failure to orient separate sections properly with relation to each other.

An omission of a line, or discontinuity not present in original figure, was scored as three errors, to provide a more linear scale. Drawings with one or two errors

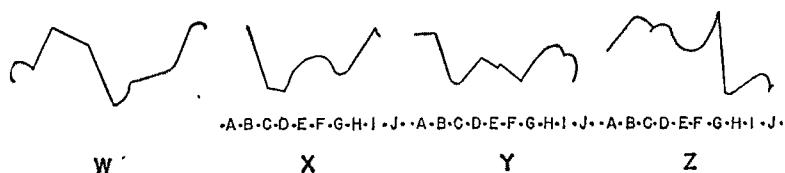


FIG. 2. STIMULUS-FIGURES USED IN THE INDIVIDUAL EXPERIMENTS

Dots beneath diagrams X, Y, and Z indicate how each diagram was divided into tenths; the letters between the dots refer to the areas.

are nearly perfect. It cannot be assumed that intervals on the scale are equal, and, of course, a scoring which neglects structure relations cannot be completely satisfactory.

The total score on each figure was the sum of scores on all trials. The total score on the group test was obtained by the formula $3A + 2B + C$, which approximately equalized the contribution of the tests to the total variance.

E scored the drawings by transparency. A test of the reliability of scoring was made by rescored a sample of the drawings after a 2-wk. interval. *E* rescored drawings of 40 Ss on each of 9 trials; the correlations between the two sets of scores on each trial ranged from 0.69 to 0.92, averaging 0.86.

(II) *Individual experiment.* (1) *Subjects.* On the basis of group test scores, 24 good and 22 poor learners were selected for retesting. In selecting the Ss, preference was given to those having scores that deviated consistently on all figures. The good group contained 11 boys, 13 girls; the poor group, 9 boys and 13 girls. The mean scores of the groups on Diagrams A, B, and C were respectively for the good group, 15.5, 26.7, and 57.2, and for the poor group, 34.9, 69.2, and 127.7.

(2) *Stimulus-figures.* Four new stimulus-figures, W, X, Y, and Z (Fig. 2), were prepared as before and photographed on 35-mm. films.

(3) *Procedure.* About 2 mo. after the group test, the Ss were summoned individually for retesting. The equipment consisted of an eye-movement camera, stool, table, and a mounted still-film projector. The projector, placed about a foot above the camera, cast a white-on-black image $10\frac{1}{2}$ by $8\frac{1}{4}$ in. on the buff-colored wall about

39 in. in front of *S*'s eyes. The wall was unmarked except for two black dots at the upper corners of the area where the image was to appear. These dots were used for pre-fixation. *S* was informed that his eye movements were to be photographed to obtain a record of his study habits. He was not told, however, that he had been selected because of atypical performance on the group test; but was, instead, let to believe that a random sample of the class was to be retested.

S was seated on the stool, adjusted to the camera, which was focussed on his right eye. Trials on *W* were then made without the use of film. After several trials with stimulus-figure *W*, *S* was asked to copy it while it was exposed. Series of trials on *X*, *Y*, and *Z* were then made, during which the right eye was photographed. Procedure for each trial included focussing of camera, instruction to fixate the left dot, instruction to fixate the right dot, exposure and instruction to study, end of exposure and withdrawal of *S* from camera, and reproduction of figure. Exposure lasted 10 sec.

S was allowed a maximum of 8 trials on stimulus-figure *X*, 10 each on *Y* and *Z*. If, in the opinion of *E*, two consecutive drawings showed no error, the series was terminated at that point to save time and to reduce boredom. This judgment was made rapidly and somewhat subjectively; some series were terminated too early as a result.

At the end of the *Z*-series, *S* was informally interviewed. He was first encouraged to describe his methods of study, then to answer the following questions. (a) Did you study the figure as a whole, or part by part? (b) Did you attempt to trace the figure in the air while studying? (c) Did you gain assistance from noticing that the diagram or any part of it resembled any object, letter, or number?

Every diagram was shown to *S*, who was asked to point out associations. Use of kinesthesia was also determined by *E*, who observed frequently during study whether tracing movements appeared.

(4) *Scoring.* Drawings were scored as before. The formula $2X + Y + Z$ was used for the total score. In any case where the series had been ended even though the last reproduction contained errors, the total score for the figure was obtained by assuming that *S* would have made the same number of errors in each further trial if more had been allowed.

(5) *Plotting of films.* All films were plotted by projection, using the fixations on the dots before study and the head record to determine the location of fixations. Each fixation-record on the film appeared as a dotted line, each dot representing 1/30 sec. After the initial plotting, all records were rapidly replotted, as a check. Records for trials in which the plotting was uncertain were discarded.

When *S* is looking at a section of the stimulus-figure, what he sees varies little if he shifts his fixation-point a short distance. On the assumption that such movements are not significant, consecutive fixations falling within $\frac{1}{2}$ cm. of each other on the plotting sheet (equivalent to 0.30 in. on the figure projected on the wall or to a shift of visual angle of $0^{\circ} 26'$) were counted as a single fixation. This assumption tends to increase the average duration of fixations.

RESULTS

(1) *Learning curves.* The mean score on each stimulus-figure, for every trial, is shown in Table I. It is evident that development was gradual and

that the relative difficulty of the stimulus-figures was the same on all trials. Group learning curves were obtained by plotting each score as a percentage of the score on Trial 1; this equalized the stimulus-figures for initial difficulty. Such curves for the group test are quite similar in shape. The curves for the individual test, based on a less normal group, fit the same shape, but less closely. Judd and Cowling reported that "the most radical changes in the records occur in the first part of each series. . . . The number of lines in the pattern might have been increased and then the serious errors would have appeared through a larger part of the series."¹⁵ The

TABLE I
MEAN NUMBER OF ERRORS MADE ON EACH REPRODUCTION OF STIMULUS-
FIGURES A, B, C, X, Y, Z

Stimulus-figure	N	Score on Trial										Total Score
		1	2	3	4	5	6	7	8	9	10	
A	118	5.4	4.3	3.8	3.2	3.1	2.9	2.8				25.2
B	118	7.6	6.1	4.9	4.2	4.0	3.9	3.4	3.2	3.4	3.1	44.5
C	118	16.9	13.1	10.3	9.1	8.1	7.3	7.0	6.9	6.4	6.0	91.4
X	46	5.4	2.9	2.3	2.3	1.8	1.6	1.5	1.2			18.9
Y	46	9.3	7.4	6.4	5.5	5.1	4.6	4.3	4.2	3.8	3.8	53.1
Z	46	11.6	9.2	7.4	6.2	5.2	4.6	4.1	4.0	3.4	3.5	58.2

present data do not contradict their statement, in the literal sense, as the raw scores for Diagram C do remain constantly above those for Diagrams A and B. It appears, however, that the learning of more difficult stimulus-figures does not differ qualitatively from that for simple figures, insofar as these curves are representative. This is a more cautious version of Fehrer's conclusion that "the learning of complex forms follows laws holding for the simplest perceptions."¹⁶

(2) *The relation between abilities.* Intercorrelations among total scores on stimulus-figures A, B, and C were used to determine whether individual differences in ability are consistent from form to form. The intercorrelations were as follows: A with B, 0.43 ± 0.05 ; A with C, 0.39 ± 0.05 ; B with C, 0.58 ± 0.04 ; average, 0.47.

Q (quantitative) and L (linguistic) scores on the American Council Psychological Examination for High School Students, 1938 edition, were correlated with performance on the figures. The average correlation of A, B, and C with Q was -0.21 ; with L, -0.20 . Since a high score on any figure indicates poor performance, these

¹⁵ Judd and Cowling, *op. cit.*, 358.

¹⁶ E. V. Fehrer, An investigation of the learning of visually perceived forms, this JOURNAL, 47, 1935, 192.

negative correlations show that there is a slight positive relation between ability to learn forms and mental abilities measured by the psychological examination.

A factor analysis of the intercorrelations between A, B, C, Q, and L was employed. A bi-factor pattern was postulated, and the working assumption that Q and L each represents a group factor was made. The results of this analysis are shown in Table II.

If the general factor represents elements common to all five tests, it is reasonable to call the group factor 'form-learning ability' since it represents what A, B, and C have in common with each other but not with Q and L. The size of the specificity loadings indicates that, under the conditions of this test, mental ability and general form-learning ability have no more influence on S's performance than do other un-

TABLE II
FACTOR LOADINGS FOR FIVE VARIABLES

Variable	General factor	Group factor	Specificity
A	0.311	0.462	0.830
B	0.256	0.759	0.598
C	0.270	0.673	0.689
Q	0.735	—	0.678
L	0.716	—	0.698

identified factors. The specificity may represent variations in the testing situation, or may be related to specific characteristics of the figures themselves.

The relation of initial ability to final ability was derived by correlating the score on the first trial with the total score on the last two trials for each stimulus-figure. The average correlation of initial score with final score for the same figure is 0.29 ± 0.05 . This indicates that an individual's performance at the outset of a series of trials is only slightly prognostic of his final performance.

A drawing ability score was obtained from the copy of W which S drew during the group test. The correlations with total score on Diagrams X, Y, and Z, respectively, for 46 cases, were 0.36, 0.41, and 0.27. These correlations are high enough to suggest that a more reliable measure of copying ability could usefully predict quality of form learning. Actually, of course, the score on Diagram W represents S's attitude toward the experiment, his concentration, and carefulness, as well as perceptual and motor abilities.

(3) *Use of association and other cues.* The results of the post experimental interviews indicate clearly that association was not used by every S. This is in contrast to the opinions of Gibson¹⁷ and Kuhlmann¹⁸ that association is essential to learning. On diagram X, 24 Ss reported a total of 34 associations (16 different). On Y, 32 Ss used a total of 53 associations (27 different). On Z, 28 Ss used 47 associations (27 different). It appears that the number of associations is related to the difficulty of

¹⁷ J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

¹⁸ Kuhlmann, *op. cit.*, 319.

the stimulus-figure, but further evidence from more stimulus-figures is needed to establish this conclusion. It may be that associations become more necessary when experimental conditions, such as shortness of exposure or immaturity of the Ss, make learning more difficult.¹⁹

The relation between number of associations reported and performance is slight; the product-moment correlation between total score on the individual test and number of associations is -0.13 ± 0.09 . To a very small extent, then, use of associations is correlated with superior performance.

The Ss were classified into two groups, one reporting use of verbal description during study, one reporting no verbalization. Reports were not dependable as to the exact amount used on each figure. In all, 26 of the 46 Ss reported some verbalization. Such descriptions as "down, over, around, up," or "straight, curved, straight" predominated. Geometrical terms and measures were also used. The mean total score of the verbalizing group on Diagrams X, Y, and Z was 129.2; that of the non-verbalizers, 162.0. The critical ratio of this difference, which favors the verbalizers, is 2.2. Verbalization was associated with superior linguistic ability; the bi-serial correlation with linguistics (L) is $+0.24 \pm 0.12$. The bi-serial correlation between number of associations and L is -0.17 ± 0.10 .

The Ss were also classified on the basis of their kinesthesia into three groups: no kinesthesia, conscious kinesthesia, and unconscious kinesthesia. The last-named group made observable tracing movements during study, but denied knowledge of them when interviewed. The frequencies in these groups, respectively, were 14, 28, and 4. The mean total scores of the groups on the individual test were 140.9, 163.1, and 134.5, respectively. The difference between the first two groups has a critical ratio of only 1.27.

The tetrachoric correlation between use of association and conscious kinesthesia is 0.26; between verbalization and conscious kinesthesia, -0.38 . There was no clear relation between use of association and verbalization.

(4) *Perceptual types.* Of the Ss, 16 used associations in the learning of all the stimulus-figures; these may be considered 'subjective' in behavior. Only 8 never used associations; these would be classified as 'objective.' The remaining Ss, 22 in number, reported associations only on some figures. The product-moment intercorrelations between the number

¹⁹ Cf. A. R. Granit, A study on the perception of form, *Brit. J. Psychol.*, 12, 1921, 232; and Gibson, *loc. cit.*

of associations made on each stimulus-figure are: X with Y, $+0.53 \pm 0.07$; X with Z, $+0.007 \pm 0.10$; and Y with Z, $+0.18 \pm 0.09$.²⁰ The persistency of 'type' was also tested by the tetrachoric correlations between type on different figures, with these results: X with Y, 0.89; X with Z, 0.40; Y with Z, 0.55. Use of associations appears more common with some Ss than others; this trait was somewhat consistent on all figures. Few Ss, however, were consistently 'objective' or 'subjective' throughout the test. It therefore appears less misleading to refer, not to 'type,' but to a 'tendency to form associations,' which is a trait to be conceived as a continuous variable, in contrast to the dichotomy previously employed in description. The intermediate positions on such a scale may be as characteristic of some Ss as are the extreme positions for others.

Wulf contrasted 'isolative' learners, who used geometric and verbal descriptions of the figure and studied it by parts, with 'comprehensive' learners, who studied the figure as a whole, and associated the complete figure with 'real' objects. In this study, no S could be found who certainly was 'comprehensive.' Only 3 Ss gave names to any figure as a whole; 2 of these also used verbal descriptions. This implies that the characteristics described by Wulf are not mutually exclusive. An attempt was made to classify each S as 'isolative' or 'comprehensive,' on the basis of the type of association used. The former group included those using verbal and geometric descriptions; the latter, those whose associations were 'real.' Only 4 Ss were clearly comprehensive; 21 were consistently isolative. Characteristics of both types were shown by 18 Ss. It therefore appears that Wulf's categories are not helpful as descriptions.

(5) *Eye movements during study.* The records of the eye movements of the Ss were analyzed by methods resembling those of other investigators.

(a) *Types of pattern.* The patterns of eye movement varied markedly from trial to trial and from S to S. Some similarities do exist which are best described in terms of types of pattern. Caution in discussing these data is necessary, as some resemblance can be imagined within almost any set of irregular patterns. It should also be noted that the types of pattern described are a convenient fiction, and that few records fit the described types exactly.

Of frequent occurrence was the *cross-study* pattern—a regular progression of fixation in either direction.²¹ The *forward cross-study*, in which the eyes move from left to right, is similar to the typical pattern used in reading. Seldom found in reading, but common here, is the *backward cross-study*, in which the eye makes

²⁰ Not corrected for use of broad categories.

²¹ This terminology for pattern types is that used by L. C. Gilbert, (An experimental investigation of eye movements in learning to spell words, *Psychol. Monog.*, 43, 1932, (no. 196), 22 ff.). His descriptions of the eye movement patterns used while studying words (*ibid.*, 31-33) would apply verbatim to the patterns in this investigation.

successive leftward movements. This seldom occurred alone; most frequently, it followed a forward cross-study pattern. The cross-study is in contrast to the pattern in which large portions of time are spent in continuous study of one small area. This may be shown by one extremely long fixation or by a *concentration oscillation*.

An *adjustment regression* was usually shown by a long movement in either direction followed by a brief fixation and a subsequent short movement in the same direction. This is similar to the regression which follows an incomplete return sweep in reading. A special case of adjustment was common at the beginning of trials. Before the exposure of the stimulus-figure, *S* was fixating the right dot. When the figure appeared, *S* shifted his gaze leftward to some point near its center, made a short fixation, then again moved, usually leftward, to the part that he apparently wished to study. It appears from the present results that orientation takes place

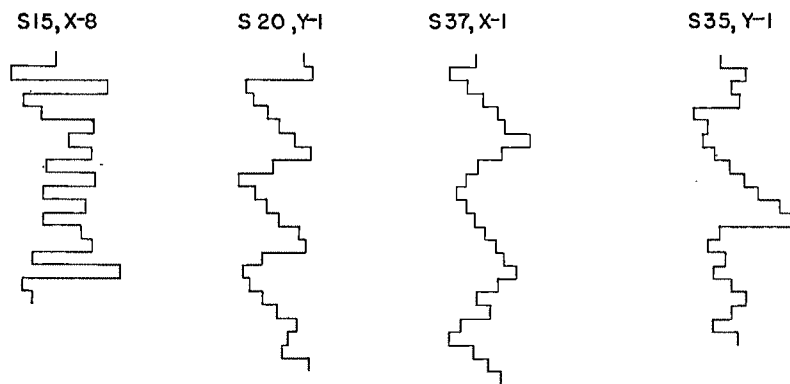


FIG. 3. SELECTED EYE MOVEMENT RECORDS OF FOUR LEARNERS

The captions at the head of the records give the *S*, the stimulus-figure, and the trial.

during the first fixation, and that the location of that fixation does not indicate the point which *S* wishes to study.

The *random oscillation* is a series of shifts of fixation between two or more widely separated points. This form occurs often in the records of *S* 15; one of the most striking examples is shown in Fig. 3. This rapid fluctuation implies an attempt to grasp the stimulus-figures as a whole, but may indicate merely that *S* was looking idly across the figure with little concentration. The random oscillation was also used for orientation. Many students showed three or four oscillations before commencing a cross-study or concentration pattern.

(b) *Patterns used on initial trials.* On the first trial of a series, the customary pattern showed one or more orientation fixations followed by a forward cross-study. This pattern was at times interrupted by a single backward movement, such as Gilbert calls the "pick-up" regression resulting from overreaching." After the forward cross-study, most *S*s continued with a series of two or three more forward cross-studies. Others alternated forward and backward cross-study patterns. A third pattern was that in which the cross-study was followed by concentration on some point. Each of these types is illustrated in Fig. 3.

Other Ss showed less regularity. Some oscillated throughout the first trial. Many interrupted fairly regular cross-study patterns for brief concentration. During several trials on stimulus-figure X, S 34 studied with successive backward cross-studies. No forward cross-study was used on the first trial. This S was right-handed.

(c) *Patterns on later trials.* On trials other than the first of a series, little uniformity appeared. Many trials began with a cross-study, usually forward. Concentra-

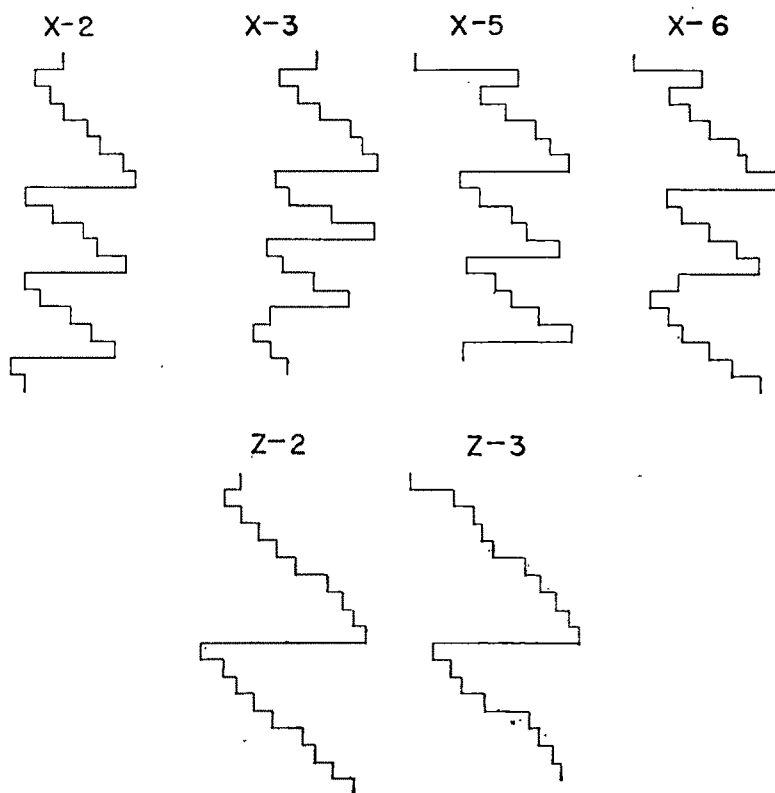


FIG. 4. EYE MOVEMENT RECORDS OF S46 ON SIX TRIALS

These results show close similarity of pattern.

tion patterns and slower cross-studies were then used. In other trials, concentration began at once, without cross-study. The following conclusions seem warranted by the results.²²

(i) Eye movement patterns were most regular on the first trials of a series.

²² The eye movement records of all Ss whose records were plottable have been microfilmed and deposited in the University of Chicago Libraries. See L. J. Cronbach, *Individual differences in learning to reproduce plane figures*, Univ. of Chicago, 1940 (unpublished thesis), appendix.

(ii) Regular forward cross-studies were used more on early trials than on later trials; on late trials, concentration on one area at a time was more common than continuous progression across the figure.

(iii) Cross-study patterns from left to right were more frequent than those from right to left, even though the conditions of the experiment forced *S* to make a leftward movement before his first study fixation.

(d) *Consistency of pattern within individual series.* Many *Ss* appeared to have a preference for one type of pattern, which they used on trial after trial. It is not feasible to demonstrate this fact statistically, and space does not permit presentation of many illustrative cases. The records of *S46*, a good learner, on 6 trials are shown in Fig. 4; the constancy of her pattern was greater than that of most *Ss*. Her pattern is unusually regular; many other *Ss* had patterns which were consistent but less regular in form. The resemblance between the study patterns of the same *S* on different trials and different figures is similar to that reported by Gilbert, who found resemblance between patterns when *S* studied the spelling of different words.²³

(e) *Duration of fixations.* The average duration of fixations on any trial (*TF*) was obtained by dividing the total time of study by the number of fixations made, including the first and last ones. The average duration throughout a series of trials on one stimulus-figure (*SF*) was computed by averaging the *TFs*. Series containing over three records not capable of being plotted were discarded in computing the *SFs*. The mean *SFs* for stimulus-figures X, Y, and Z were 15.71/30, 17.86/30, and 16.75/30 sec., respectively. The differences between X and Y, and Z and Y, are statistically significant; the difference between X and Z has a critical ratio of 1.9 (2.3 if mean *TFs* on all trials are averaged). The range of fixation-lengths also differed from stimulus-figure to stimulus-figure. The standard deviation (*SD*) of the *TFs* for the respective figures were 3.66, 5.44, and 4.32. All of the differences are significant. Since the means and variabilities change together, and since this change is not in the order of presentation, it is likely that characteristics of the stimulus-figures rather than of the situation are responsible for these variations.

Bartlett reported experiments with exposures of 1/15 to 1/4 sec. (2/30 to 7.5/30 sec.), and commented:

The brief controlled exposure may be regarded as an artificial and forced condition. This objection certainly holds good to some extent. But the ordinary glance of everyday life rarely rests for long on any given object; and further, when a person is definitely set to observe and reproduce, his keenness and criticism are as a rule considerably increased; so that in another sense the short exposure approximates most closely of all to normal conditions.²⁴

While it is not possible to compare the present conditions to those of everyday life, *S* was certainly set to observe and reproduce. In no case did any *S* have a *TF* below 8.9/30 sec.; very few individual fixations fell below 7/30. The average duration of fixations in general was far above Bartlett's estimate. Moreover, Buswell's *Ss*, looking at pictures, made fixations averaging 10.1/30 sec.²⁵ It appears that Bartlett's exposures were substantially shorter than the ordinary glance.

It was apparent in analyzing the records that some *Ss* tended to make consistently short fixations, while others tended to make frequent very long fixations. The consistency of individual differences in fixation-length is most readily shown

²³ Gilbert, *op. cit.*, 22.

²⁴ F. C. Bartlett, *Remembering*, 1932, 17.

²⁵ Buswell, *op. cit.*, 85 f.

by the correlation of the *SFs* from figure to figure. These were: X with Y, 0.600 ± 0.073 ; Y with Z, 0.515 ± 0.088 ; and X with Z, 0.394 ± 0.100 . A study of the average deviations of the *TFs* within a series also revealed a tendency for the *Ss* to be consistent within each series.

The mean *TF* on each trial was obtained by averaging the *TFs* for all the *Ss* whose records were capable of being plotted. These means are shown in Table III. The variation of the group on each trial was high, so that few of the differences from trial to trial are statistically significant. The consistency of the differences warrants the conclusion that there is an increase in the mean *TF* on the first 6 trials with each stimulus-figure. Although these trends seem to continue on trials after the sixth on Y and Z, the small number of cases available makes those averages unstable. The

TABLE III
AVERAGE DURATION OF FIXATIONS (*TF*) ON EACH TRIAL (1/30 SEC.)

Stimulus figure	Trial									
	1	2	3	4	5	6	7	8	9	10
X	13.6	15.0	15.1	15.7	16.5	16.6	16.6	16.2		
Y	14.9	18.1	17.2	16.5	18.6	17.7	18.0	17.3	20.2	21.1
Z	14.1	14.5	15.8	16.6	16.2	17.2	16.2	17.0	18.7	18.1

trend is less steady for Y than for X and Z; this appears to be due to a few exceptional *Ss* having very high individual *TFs* on two trials. Since the number of cases varied from trial to trial, a study was made of a sub-group for whom all records were usable; this confirmed the conclusion drawn from Table III.

With only three exceptions, the mean *TFs* on each trial increased in the order X to Z to Y. This is in accord with the *SFs* and supports the hypothesis that such differences are due to the nature of the stimulus-figures.

To determine the trend of fixation-length within trials, the sums of the first five, second five, and last five fixations on each trial were obtained. The average of each sum on all trials for each *S*, and the average for all the *Ss* on each trial were computed. These data supplement the previously established differences among the *SFs* and the *TFs* on the three stimulus-figures; it further appears that an increase from X to Z to Y occurred for fixations in all parts of the trials.

Differences between the sums for each stimulus-figure also existed. For every figure, the last fixations were greater than the second five, and these in turn were greater than the first five. The differences between the first and second five fixations, and between the second and last five, are not statistically significant by the usual test; critical ratios range from 0.6 to 2.3. The differences between the first and last five fixations are statistically significant for two figures (critical ratios: X, 3.0; Y, 3.6; and Z, 1.7). If the formula for the significance of differences of correlated variables had been employed, the critical ratios would have been increased. The consistency of these differences from stimulus-figure to stimulus-figure also enhances their validity. It is not true, however, that the sums increase from first five to second five to last five fixations on all trials; with few exceptions, the first sum was less than the last, but the second sum was at times as large as the last sum, and at times as small as the first. The first sum increased fairly regularly from trial to trial throughout the series; this tendency was less consistent for the other sums.

Whipple, who analyzed the introspections of learners, referred to a "steady stare" or 'impression-of-the-whole' method which observers are prone to use at first.²⁶ The foregoing data consistently fail to indicate any such tendency. Steady fixations (stares) are far less common, at first, than cross-studies. Long fixations are most common on the later trials. While the cross-study may be used to obtain an impression of the whole, it is erroneous to say that it is characteristically used "at first"; its use is decreased, but not abandoned, on later trials.

(f) *Reversal ratios.* The use of regressions has previously been found to be a valuable index of the efficiency of eye movement patterns. In the present situation, it is incorrect to consider leftward eye movements as 'backward,' and the study of regressions is therefore not completely appropriate. A substitute index, the reversal ratio, has been defined. A reversal is any eye movement in a direction opposite to that of the preceding movement. The reversal ratio (RR) is defined by the equation: $RR = \text{number of reversals} \div (\text{number of fixations minus two})$. This index has a possible range from 0.00 to 1.00. The term may be illustrated by reference to Fig. 3: S15, on X-8, had an RR of 0.88; S20, on Y-1, of 0.33; S37, on X-1, of 0.30; and S35 on Y-1, of 0.60. It must be recognized that this function is arbitrary and crude, as the sampling of the numerator is so small that wide fluctuations are possible.

The series reversal ratios (SRRs) for each S were obtained by averaging his RRs on all trials for each figure. The SRR for most Ss was in the neighborhood of 0.50. The mean SRR for stimulus-figures X, Y, and Z were 0.4781, 0.4751, and 0.4467, respectively. The differences between X and Z, and Y and Z, may be considered significant (critical ratios: 3.1 and 2.7, respectively); the difference between X and Y has a critical ratio of only 0.4. Unlike the differences in fixation-length, these differences follow the order of presentation of the figures. It is, therefore, impossible at present to attribute them either to factors in the experimental situation or to characteristics of the figures.

Individual differences in RR and SRR were large. The range of RR on individual trials was from 0.08 to 0.88. The range of SRR was from 0.334 to 0.608. The correlations between SRRs on different stimulus-figures are positive and moderately large. For X and Y, the correlation is 0.69 ± 0.06 ; for Y and Z, 0.51 ± 0.09 ; and for X and Z, 0.57 ± 0.08 . The size of these correlations is especially significant in view of the coarseness of the terms forming the reversal ratio. These correlations are statistical evidence that the Ss tend to use similar eye movement patterns on different figures; those using many oscillations on one figure do so consistently, and vice versa.

An attempt was made to detect changes in the RR from trial to trial. The average RR for each trial was computed by averaging the RRs of all the Ss whose records for that trial were usable. These averages, shown in Table IV, yield no consistent differences or trends.

(g) *Location of fixations.* Records in which the location of fixations was uncertain, due to poor head records or poor pre-fixation of the dots on the wall, were discarded before the distribution of attention was analyzed. The width of the

²⁶ G. M. Whipple, The effect of practise upon the range of visual attention and of visual apprehension, *J. Educ. Psychol.*, 1, 1910, 262.

stimulus-figure was divided into 10 equal parts. From left to right, these were labelled, in order, *A, B, C, . . . J* (cf. Fig. 2). For every record, the fixations falling in each area were totalled, and the results converted into percentages by dividing by the total study-time. Fixations to the left of the marginal dot were grouped in Area *L*; fixations to the right of the right dot were grouped in Area *R*. The complete set of 12 concentration totals formed the concentration distribution.

The concentration distributions on various trials, and for different *Ss*, showed wide variety. Some were skewed to the right or left, showing that *S* had fixated one side of the stimulus-figure more than the other; some were quite peaked, showing that *S* had spent much of his time on one small area; others were rather flat, indicating a fairly even distribution of fixation. The group concentration distribution for each trial was obtained by averaging individual distributions. These values are

TABLE IV
AVERAGE REVERSAL RATIO ON EACH TRIAL FOR STIMULUS-FIGURES X, Y, AND Z

Stimulus-figure	Trial									
	1	2	3	4	5	6	7	8	9	10
X	0.444	0.466	0.458	0.498	0.517	0.446	0.495	0.493		
Y	0.465	0.468	0.452	0.503	0.488	0.499	0.502	0.454	0.509	0.421
Z	0.472	0.410	0.460	0.437	0.465	0.439	0.457	0.442	0.462	0.413

shown in Table V. There is apparently a consistent tendency to give greater attention to the left side of the figure. This is in accord with previous investigations.²⁷ The median position of fixation is within or very near to Area *E*, and the distribution is skewed to the left on each trial. It seems likely that the left end of *X* is less difficult than the right; certainly, it contains more straight lines, which Judd and Cowling showed to be easier than curves. It is less simple to estimate the difficulty of the halves of *Y* and *Z*. It is doubtful that the left is always most difficult; some force other than difficulty, perhaps habits from reading, is probably responsible for the preponderance of fixation on the left.

Each stimulus-figure has a distribution of distinctive form. That for *X* is uni-modal, that for *Y* is uni-modal and sharply peaked, and that for *Z* is like that for *X* on areas *A* to *D*, but has a 'saddle' in the middle and a secondary mode at *G* and *H*. That these distributions are characteristic of the stimulus-figures is implied by the consistent appearance of the distribution on every trial for the figure, with only trivial deviations. This consistency does not mean that the *Ss* individually look at the same points on trial after trial, but it does demonstrate the absence of any systematic shift of concentration from trial to trial for the group as a whole. The positions receiving greatest average attention on Trial 1 continue to do so on all trials.

The fact that the distributions are characteristic of the stimulus-figures implies

²⁷ K. M. Dallenbach, Position *vs.* intensity as a determinant of clearness, this JOURNAL, 34, 1923, 285; Ruth Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left handed observers, *ibid.*, 35, 1924, 267-268; C. L. Friedline and K. M. Dallenbach, Distance from point of fixation *vs.* intensity as a determinant of attention, *ibid.*, 41, 1929, 467; H. F. Brandt, Ocular patterns and their psychological implications, *ibid.*, 53, 1940, 266.

that some factor other than mere visual habit, or chance, is influential. The only other explanations that present themselves are that the differences arose from the experimental conditions or from the nature of the figures. The only variable element in the situation was the order of presentation; if this were influential, the distribu-

TABLE V
PERCENTAGE OF THE STUDY PERIOD WHICH THE GROUP USED IN FIXATION ON EACH TENTH OF THE STIMULUS-FIGURE

Stimulus-figure	Trial	No.	Area											
			L	A	B	C	D	E	F	G	H	I	J	R
X	1	27	1	2	7	15	16	12	12	12	11	10	2	1
	2	31	1	2	7	15	18	17	10	13	8	5	3	0
	3	29	0	2	9	12	18	14	14	12	10	6	2	1
	4	33	0	1	7	18	16	14	12	13	9	7	1	1
	5	24	0	1	6	17	16	16	11	12	10	9	3	0
	6	20	0	3	8	15	18	10	13	12	11	6	3	0
	7	17	0	0	7	13	14	14	11	13	10	12	4	1
	8	13	0	2	6	16	18	11	13	10	11	10	3	1
	Mean		0	1	7	15	17	13.5	12	12	10	8	3	1
Y	1	29	0	6	14	14	13	22	11	7	9	4	1	0
	2	32	0	4	9	15	22	18	13	8	6	4	0	0
	3	33	1	3	10	18	16	21	11	10	7	2	1	0
	4	24	0	3	7	14	19	22	14	6	9	4	2	0
	5	27	0	3	8	14	18	22	13	8	8	5	0	0
	6	21	0	3	9	13	16	18	16	11	8	3	1	0
	7	17	0	5	9	12	16	20	13	9	5	7	2	0
	8	17	1	4	7	13	18	22	15	10	8	2	1	0
	9	16	1	5	9	10	14	19	20	11	5	2	2	2
	10	15	1	3	8	11	18	23	12	8	10	5	0	0
	Mean		0	4	9	13	17	21	14	9	7.5	4	1	0
Z	1	30	1	3	7	16	20	10	7	7	11	10	6	3
	2	27	0	1	7	15	17	11	7	11	11	8	7	3
	3	29	1	2	7	15	16	12	8	11	13	7	7	0
	4	29	1	3	5	12	15	13	9	8	12	10	9	3
	5	26	1	2	6	12	20	11	11	8	10	8	7	3
	6	29	0	1	9	19	17	10	8	11	9	10	4	1
	7	24	0	3	6	13	17	10	11	10	10	10	5	4
	8	18	0	2	8	16	17	9	11	8	7	9	1	1
	9	16	0	2	12	19	18	8	6	13	12	5	3	2
	10	16	2	2	7	15	14	12	8	10	8	6	14	2
	Mean		1	2	7	15	17	11	8	10	10	8	7	2

tions for X and Z would be expected to be least, rather than most, similar. Therefore this is ruled out as an explanation, and it becomes necessary to conclude that eye movements are determined by characteristics of the figure studied. This conclusion, while in agreement with Brandt's findings, contradicts the declaration of Guilford and Hackman that eye movements have little value as indicators of the attention values of stimuli.²⁸ These experimenters, however, appear to have based

²⁸ Guilford and Hackman, *op. cit.*, 383.

their conclusions on the direction of the first eye movement after the stimulus was exposed. It has been found, in the present study, that the first fixation apparently is often used for orientation; this would make it a poor indicator of *S*'s attention. While it appears likely, then, that eye movements are useful as indicators of the "attractiveness" of parts of the figure, further research is needed to determine whether eye movements are related to clearness. The present data do not indicate what characteristics of the figures determine the distribution of attention.

A measure of each *S*'s tendency to concentrate on each trial was obtained in a concentration quotient (*CQ*): the two adjacent tenths of the stimulus-figure given the greatest total attention were determined; the time spent on this fifth of the figure, divided by the total study time, gave the *CQ*. This function has a theoretical

TABLE VI
MEAN CONCENTRATION QUOTIENT ON EACH TRIAL AND MEAN SERIES CONCENTRATION QUOTIENT FOR EACH STIMULUS-FIGURE

Stimulus-figure	Trial										Mean <i>SCQ</i>
	1	2	3	4	5	6	7	8	9	10	
X	Mean	43.1	45.8	44.2	46.8	43.2	46.0	41.9	49.3		44.9
	No.	27	31	29	33	24	19	16	12		31
Y	Mean	44.2	51.7	52.2	51.8	50.6	50.0	50.6	51.9	49.7	53.8
	No.	30	33	32	24	27	21	17	17	16	15
Z	Mean	45.0	42.6	44.8	46.2	46.3	46.8	46.9	42.0	47.8	41.7
	No.	30	27	29	28	26	29	22	18	16	14

possible range from 0.20 to 1.00. Actually, the range found was from 0.26 (*S*31 on Z-1) to 0.98 (*S*36 on Z-10). The series concentration quotient (*SCQ*) for each *S* on each stimulus-figure was obtained by averaging the *CQ*s on all usable records. Table VI presents the mean *CQ* on each trial and the mean *SCQ* on each figure. The mean *SCQ* on Diagram Y was considerably greater than that for X or Z. This is in conformity with the group concentration distributions for these figures, for the distribution for Y was more peaked than the others. The differences between the mean *SCQ*s for X and Y, Z and Y, and X and Z were 2.6, 3.1, and 0.6 times their standard errors, respectively. Only the first two differences may be considered significant.

Both Judd and Cowling, and Tripp, reported that many *S*s sought the general shape of the figure on early trials, and on later trials attempted to learn specific details. Attention, they reported, was withdrawn from some parts of the figure and increased on others.²⁰ If this were true, one would expect the *CQ* to be relatively high for most *S*s on later trials. The actual data, in Table VI, show no tendency toward a steady increase in concentration. On Y, the mean *CQ* on Trial 1 is smaller than on any later trial. On X, there is a slight rise from Trial 1 to Trial 2. On Z, however, the mean on Trial 1 is greater than on Trials 2 or 3. On trials after the first, the mean *CQ*s are approximately equal. It may be concluded that for the group as a whole concentration—as defined by the *CQ*—did not increase substantially during the series, except in the case of the change from Y-1 to Y-2. Individual instances of increase in concentration from trial to trial were found, but such a phenomenon did not generally occur.

²⁰ Judd and Cowling, *op. cit.*, 356.

Unlike previously discussed measures of study tendencies, individual differences in *CQ* were not consistent from figure to figure. Correlations between *SCQs* on any two series are close to zero.

The center of concentration for each *S* was defined as the midpoint of the fifth of the figure receiving greatest attention on each trial. The distribution of centers of concentration of the group on each trial was in general similar to the concentration distribution, although narrower in spread. The distributions for different trials within the same series were similar. Two tendencies were noted which may be significant. On stimulus-figures *X* and *Y*, the distribution of centers of concentration on Trial 2 was slightly to the left of that on Trial 1. The distributions for *Z* show that few *Ss* concentrated on the right end of the figure on the first two trials, but that many concentrated on areas *GHIJK* on trials after the second.

Some of the foregoing data seem to support, and some to contradict, the finding of Judd and Cowling and Tripp that study on later trials is marked by increased concentration on some areas, and a withdrawal of attention from others. The mean fixation-length increases during the series, and concentration patterns appear most frequently on later trials. On the other hand, the distribution of attention for the group changes almost none from trial to trial, and the centers of concentration are distributed similarly on most trials. The *CQs* remain constant or fluctuate irregularly on trials after the first. This seeming contradiction may be reconciled by this interpretation of the previously accepted conclusion: on later trials there is a tendency for the *Ss* to concentrate for a longer *consecutive* period on one portion of the stimulus-figure, but this concentration may be followed or preceded by study of other portions of the figure. Whereas on early trials the time spent on any portion of the stimulus-figure may be scattered in small and widely spaced intervals, on later trials that time will tend to be spent on the area continuously.

(6) *Relation of eye movements to quality of performance.* In previous investigations, measurable differences between the eye movements of good and poor learners have been found.³⁰ Differences in the patterns of good and poor learners were much less striking in this investigation. It is true that the learner with the most regular patterns (*S46*) was among the best in performance, but the next most regular patterns were those of *S4* and *S7*, both poor learners. Each type of pattern was found for both good and poor learners. It does appear that poor learners used slightly more oscillations, either concentration or random, but this may be caused by poor learning on early trials. Possibly, too, good learners would demonstrate more oscillations if they attempted more trials than they needed for mastery.

The correlation between *SF* on each series and total score on that series is: for *X*, $+0.309 \pm 0.099$; for *Y*, $+0.103 \pm 0.109$; for *Z* $+0.235 \pm 0.103$. The correlations between *SRR* and performances are negligible in size. An attempt to compare the concentration distribution of a selected group of good-learners with that of a

³⁰ Cf. Gilbert, *op. cit.*, 36-62.

group of poor learners revealed only trivial differences. The correlation of mean *SCQ* and score on each series is: for X, $+0.258 \pm 0.100$; for Y, $+0.074 \pm 0.106$; for Z, $+0.288 \pm 0.098$. It may be concluded that the eye movements of the good learners, as a group, do not differ markedly from the eye movements of the poor learners, as a group.

(7) *Relation of study methods to the reproductions.* Statistical generalizations are primarily useful when interpreted in the light of individual variations. Great caution must be employed in generalizing from case descriptions; certainly statements made about one *S* must not be considered typical of others. Limitations of space preclude a description of each *S*; one case is described below in great detail, to illustrate the method of analysis employed.³¹ Generalizations have been based on the analysis of all cases whose records were plottable.

S45 was outstanding in his performance on both the group and individual tests, requiring only a few trials on stimulus-figures X, Y, and Z. His introspections were more than usually voluble and seemed mature. He reported that he looked for the general shape of the figure and then looked specifically at definite points, and that he plotted in advance of study what he wished to look for. According to him, his study was affected by the directions; if he had known there were to be 10 trials to learn the figure, he would have divided the figure and studied by parts, but when told to learn as rapidly as possible, he did not use a part-study procedure. He claimed to have formed visual impressions most of the time, but also recalled the lines verbally as "straight, curved, straight, etc." He associated the left of X with the keel of a sailboat, or its rudder. (Model boat-building is his principal hobby.) On Y, he used the associations "mountains" for Area DEF and "clouds" for IJ; on Z, "two mountains" for the left end of the figure.

On X-1, he used a cross-study, then further cross-studies on the middle and right end. There was an apparent brief concentration pattern in Area DEF. Three atypically long fixations fell at H. The center of concentration was at DE. The reproduction was correct in general shape, although the angles were slightly inaccurate. The most serious error was at G. On X-2, attention on areas BC and E was reduced, while attention on G was greatly increased. The pattern as a whole was made of forward and backward cross-studies with no concentration pattern. The reproduction of Area G was nearly correct; the curve in DE was changed, but not improved; the angles in CD were improved. There was now no serious error. On X-3, most of the study period was occupied by a concentration pattern on FG. Fixations were generally quite short, which suggests either a tracing back and forth with the eyes or a scanning of the figure to find some point for concentration. In the reproduction, the proportion of the lines in FGH changed, but was over-corrected. On X-4, there was a long first fixation on D, then a cross-study with a long fixation at F. The remainder of the record was exclusively on EFGHI; no attention at all was given to ABC. The reproduction differed only slightly from that for Trial 3. (The records for Y and Z were treated similarly.)

³¹ The complete set of drawings made during the individual experiment has been deposited in the University of Chicago Libraries.

S seemed characteristically to recognize the point at which he had made an error in reproduction and to concentrate on that area in the next study period. He rarely concentrated at the start of the trial, but cross-studied until he reached a point of previous error, where he made a long fixation or a concentration oscillation. He frequently withdrew attention from a point previously learned, but this was in no case accompanied by a forgetting of the neglected part. When his concentration distributions were totalled for each figure and compared with those of the group, sharp differences appeared. On X, he concentrated much less on Area *ABCD*, which he reported naming with an association that was highly descriptive, for him. On Y his pattern was close to that for the group, but on Z he was much below the group in attention to *ABCD*; this area he also recalled by association. It appears that to some extent he may be able to substitute association for the concentration needed by others to learn some of the areas, but this hypothesis does not hold for Y.

The analyses of other good and poor learners failed to disclose clear and well-defined differences between successful and unsuccessful methods of study. Differences within each group were at least as great as differences between the groups. The influence of study pattern upon reproduction is not readily apparent. In general, improvement in reproduction most often followed a concentration oscillation on the part needing improvement. Improvements did occur which followed no corresponding concentration on the figure, and many concentration oscillations were not followed by corresponding improvements, but for most Ss, little improvement followed trials (after the first) which showed no concentration oscillation. Since improvement can occur without an increase of attention to the changed part and since attention alone at times does not bring improvement, it appears that the reproduction is not a dependable indication of the S's distribution of attention. Little dependence can be placed on such introspections regarding study methods as were obtained; they were frequently at variance with the objective photographic records.

The concentration oscillation seems to be characteristic of most superior learning. While many of the good learners concentrated at the point where their most serious error had occurred, many poor learners repeated an error for several trials before concentrating on that point during study, and some avoided concentration almost entirely. Among Ss who were more successful on some figures than others, concentration was most often present during study of the figure where performance was best. It cannot be concluded that concentration oscillations produce superior improvement; they may be only symptoms of the S's recognition of a point of error. Many poor learners failed to improve even after concentration; this suggests lack of some ability to retain which good learners possess. Some poor

learners appear to have had considerable difficulty in grasping the structure of the figure. This may be related to their lack of associations during study.

Many of the Ss of Judd and Cowling showed losses in accuracy from trial to trial. The experimenters ascribed some of these losses to withdrawal of attention from the part which deteriorated. In the present study, losses like these were found. A study of the eye-movement records preceding such losses showed that some losses were associated with withdrawals of attention, but not the majority of them. At times, losses occurred despite an actual increase in attention to the changed area. This implies that no single explanation is likely to be correct for all deterioration.

These findings do not invalidate the principal conclusion of Judd and Cowling—that learning of forms is gradual and quite complex. In fact, it is now demonstrated that the learning is even more complex than they stated. On the basis of their data, they accepted the hypothesis that the changes in a percept might be traced to S's distribution of attention. The present data show that this is frequently untrue, and that some further hypothesis is needed. No evidence is available to demonstrate why attention at one time brings improvement, and may at another time lead to no change, or even loss. Speculation suggests that mental set, motivation, and carefulness may be determining factors. It is possible that attention to a given line may fail to improve that line unless attention is given to its relation to the entire gestalt.

(8) *Drawing of the figures by parts.* Drawings by all Ss on each of the six stimulus-figures were classified as *whole* or *part*, depending on whether parts were missing. Among the 118 records on A, B, and C, 52 Ss made whole drawings on all trials. The remaining 66 drew incomplete reproductions on at least one trial. The number making one or more part drawings on each stimulus-figure is: A, 9; B, 18; and C, 60. Of the 46 Ss taking the individual test, 23 made whole drawings on all trials. The number making part drawings on each stimulus-figure is: X, 6; Y, 15; and Z, 16. The appearance of part drawings increased as the difficulty of the stimulus-figure grew. Figures Y and Z, of equal difficulty, were drawn by parts by the same number of Ss.

Of the 9 Ss making part drawings on A, 7 made them also on B or C, or both. Of the 18 Ss in the part group on B, 14 showed part drawings in the C series. Four of the 6 who made part drawings on X also did so on Z or Y, or both. It appears, therefore, that a person who makes part drawings on one figure is likely to do so on more difficult figures. The product-moment correlation, corrected for broad categories, between number of part drawings in the group and individual

tests is $+0.41 \pm 0.08$. The tetrachoric correlation, obtained by classifying each *S* as "all whole" or "occasionally part" on each test is $+0.20$. A third method of determining this relation is the product-moment correlation between the total number of trials showing part drawings on each test. This correlation, based on variables with a wider range than the others, is $+0.77 \pm 0.01$. Therefore, while *Ss* who make few part drawings may not show that tendency on other figures, those who make a comparatively large number of part drawings once may be expected to do so again.

In addition to the part drawings of the type described by Judd and Cowling, in which the left is mastered first and gradually augmented, many *Ss* in this study drew first the right, middle, or both ends of the figure, completing it later. Some figures seemed to lead to one type of learning more than others; thus drawing of the right was especially frequent on B, C, and X. The evidence as to whether individuals tend to learn the same part of the figure on each series is inconclusive.

Two hypotheses present themselves as interpretations of part drawings. They may indicate an attempt to learn the figure piecemeal, as Judd and Cowling supposed, or they may show that *S* tried unsuccessfully to grasp the figure as a whole. If the former explanation is correct, the part drawing should be preceded by a concentration eye movement pattern, with a higher *CQ* than usual. If the second is correct, one would expect the eye movements of those who draw only part of the figure to be no different from those of other *Ss*. An analysis of the records on the basis of these views was made for stimulus-figures X, Y, and Z. An unsuccessful attempt to learn the whole figure seems to have been responsible for drawing of only the left part by 3 *Ss* on Y and 3 on Z. A deliberate study of the left preceded drawing of the left for 2 *Ss* on Y and for the same 2 *Ss* on Z. Concentration on the left, following or combined with an attempt to study the whole, occurred for 3 *Ss* who made part drawings on Y; 2 of these, and one who had previously drawn the whole, used the same type of study before drawing the left on Z-1. Part learning is not clearly described by either hypothesis, then; some *Ss* seem to have employed a combination of the two previously mentioned alternatives. Among the few *Ss* who drew both ends of figure Y or Z, the evidence shows an apparent attempt to learn the whole in each case. Since neither hypothesis is correct for all *Ss*, it seems impossible to determine *S's* method of study from the fact that he makes part drawings.

The bi-serial correlation between the final score and use or non-use of part drawings for each figure was computed. These correlations, with their standard errors, are: for A, -0.12 ± 0.17 ; B, $+0.32 \pm 0.13$; C, $+0.10 \pm 0.11$; X, $+0.32 \pm 0.22$; Y, $+0.15 \pm 0.19$; and Z, $+0.13 \pm 0.19$. In general, then, part drawing is associated with poor final per-

formance, but these correlations are not statistically significant. Too few data are available to justify comparison of quality of performance of Ss using different types of study, but making part drawings.

DISCUSSION OF RESULTS

It is unjustifiable to generalize that the learning of figures of this type is similar to the learning of ink blots, of stimulus-figures made of several discrete units, or of meaningful forms. Caution is also necessary in interpreting the performance of these Ss as representative of that of persons of other ages.

The hypothesis was made, at the outset of the study, that the individual differences in form-learning ability are stable. This has been demonstrated to be in part false. The average intercorrelation of total scores on the three figures in the group test is only $+0.47$. Comparison of performances on the group and individual tests showed similarly low relations. The correlations of form-learning performance with the scores on the American Council Psychological Examination are even smaller. The correlation between the learning of form and ability to copy a figure directly is also low. It has been shown that individual differences in the learning of form cannot be entirely described by postulating a 'general mental ability' and a 'form-learning ability'; some other factors, apparently specific to each figure, are likely to influence S's performance greatly. Further investigation is needed to identify these other probable elements. The difficulty of predicting performance in form learning shows the complexity of the problem of predicting such behaviors as learning in shorthand and in artistic memory drawing, where form-learning is just one component.

Since individual differences fluctuate somewhat from figure to figure, one might expect the factors causing the differences between good and poor learners to be difficult to detect. Actually, no clearcut difference in learning methods appeared between good and poor learners. It was mentioned, above, that mental ability, as measured by the psychological examination, and ability to copy a figure are positively related to performance. The eye movement records of good learners disclose a tendency to concentrate on points of previous error more often than the records of poor learners. Use of association and use of verbalization are also associated with superior performance to some extent. The following are associated with inferior performance; use of kinesthetic devices, high average fixation length during study, high mean concentration quotients, and the making of part drawings. In view of the low relation between each of

these factors and performance, it is at present impossible to recommend any particular type of study as likely to be efficient. The case descriptions indicate that disabilities arise from different causes in various cases of poor learning. Further investigation is necessary to determine whether such disabilities can be remedied by training. It would also be desirable to determine what factors in an individual's development lead to such disabilities.

Many *Ss* have characteristic study methods which they employ on several figures. The eye movement patterns used by the same *S* on different trials and different figures often resemble each other. Individual differences in both mean fixation-length and mean reversal-ratio are moderately consistent from figure to figure. The tendency to form associations and to make part drawings are also somewhat stable. It is reasonable to suppose that each of these traits is either useful or undesirable as a method of study for most persons, even though this experiment has not detected clear relations between study method and quality of performance. A controlled experimental study, in which each *S* alternatively uses each method, might be planned to measure such relations. Since persons who use, or do not use, each method tend to do so consistently, it may then be possible to improve the learning of those who now employ poor methods, or who neglect to use good devices.

Previous investigators have not agreed whether all persons use naming, or association, in learning nonsense figures. The present study, based on a large number of *Ss*, shows that not all persons consciously use association on any one of the figures used. The use of association is increased on figures of greater difficulty. This may arise simply because difficult figures contain more lines and hence offer greater possibilities to the imagination; on the contrary, there is some evidence in this and other studies that association is increased whenever the conditions of learning become more difficult, making mnemonic devices more necessary. The evidence in this study contraverts the theory that learners can be divided into two exclusive types, either "objective" and "subjective," as Katz and others proposed, or "isolative" and "comprehensive," as Wulf proposed. It has been demonstrated that those who use associations do tend to do so consistently, and it is recommended that this trait be described by a continuous scale, rather than in terms of a dichotomy. Further investigation is needed to discover whether the persons who used no associations in this study might do so if the figures were made still more complex.

A major objective of this investigation was the validation of the con-

clusions of Judd and Cowling and Tripp, who attempted to infer the methods of study used by *S* from the reproductions made. The eye movement technique was used to record *S*'s actual method of study; this was compared with his reproduction to determine whether changes in the reproduction were directly related to the attention distribution. In general, such evidence supported the major conclusions of Judd and Cowling as to the complexity of form-learning and the general description of how a person improves his percept. Changes in the reproduction cannot, however, be related to shifts in attention in many cases, and the reproduction is therefore not a completely reliable indicator of the attention distribution. The statement of Judd and Cowling that losses usually are the results of withdrawals of attention from the changed portion is not borne out, in many instances, by the eye-movement records. One of Judd and Cowling's *Ss* consistently learned figures piecemeal, drawing only a part of the figure on the first trial, and adding to it on later trials. Whereas they inferred that this type of drawing indicates a deliberate study by parts, the eye movement records indicate that this is not so in all cases. Many cases of part drawing appear to arise, not from study of just one part, but from study of the whole accompanied by inability to retain the complete image. Further intensive investigation of this type of learning to determine the causes of this failure to retain would be desirable.

Since most previous investigators limited their studies to figures of uniform difficulty, in this investigation an attempt was made to determine the changes in learning method that accompany changes in the difficulty of the figures. Variations from figure to figure were found for most characteristics of the study method among the figures on the group test. Differences in the relative difficulty of figures are consistent on all trials of the series. More difficult figures lead to the use of more associations and to more part drawing. On the other hand, differences in fixation-length, reversal-ratio, and concentration-quotient are apparently not directly related to difficulty, even though the differences are probably related to some unidentified characteristics of the figures. The concentration-distribution during study is also apparently related to characteristics of the figure. Generalizations about changes from figure to figure are not highly valid, in the absence of evidence that the figures employed in this study are typical. So far as the present data are indicative, study methods used on figures of varied difficulty differ quantitatively, but not qualitatively. The evidence supports the hypothesis suggested by Fehrer, that the learning of complex forms is similar to that for simpler figures. It would be desirable to isolate

the characteristics of the figure which cause the changes in study method noted; such knowledge could only be determined by experimentation with a large number of figures of different forms and complexities.

Two findings have implications for the technique of further research. It was demonstrated that for the figures employed in this study, the equalized learning curves have very nearly the same shape. Therefore, for any group of *Ss*, if the learning curve for one figure is known, and the initial difficulty of another is known, the difficulty of the second figure at any trial after the first can be predicted. If this conclusion is substantiated by experiments with other figures and other *Ss*, it becomes possible to estimate the complexity of figures with increased accuracy. In the past, it has been necessary to determine complexity by such a technique as that of Fehrer, who presented each figure to every *S* for as many trials as were needed for mastery; the average number of trials needed for any figure gave a measure of its difficulty. The learning curves in this study suggest that one might estimate the complexity of figures by presenting them for one trial each, to a large group of *Ss*; the mean difficulty on this one trial would be a valid indication of final difficulty.

It has been demonstrated that the eye movement record is related to characteristics of the figure presented. Further studies, patterned after that of Guilford and Hackman, but making an allowance for orientation fixations, would be desirable to evaluate more thoroughly the eye movement technique for studies of attention.

In general, it may be concluded that the problem of the learning of form is exceedingly complicated. Between two individuals, some difference in study method may be expected. Such differences, which are a basic determiner of differences in quality of performance, must be investigated for each individual case before efficient remedial measures can be taken. The eye movement technique has apparent value in providing valid records of study methods and may provide a method for diagnosing the nature and causes of individual differences.

FAILURE OF TRANSPOSITION IN SIZE-DISCRIMINATION OF CHIMPANZEES

By KENNETH W. SPENCE, State University of Iowa

A differential response to two members of a stimulus-series, such as steps in brightness or size, has been interpreted, by the Gestalt psychologists in particular, as being based upon the perception of some relational property of the stimulus-configuration.¹ In opposition to that interpretation, the writer has proposed an explanation that is based on the principles of conditioning or association and on the concepts of stimulus-response.² His theory, unlike the various Gestalt interpretations, does not assume that the animal is responding to some abstract relational property, or to a primitive structure-process. He holds, on the contrary, that the positive member in the stimulus-series acquires excitatory properties as a result of training to the response of approaching it and, furthermore, that there is a gradient of generalization of this excitation to other members of the stimulus-series, including the negative stimulus-object. With failure of reënforcement of response to the latter, however, an inhibitory tendency develops which, in turn, is assumed to generalize to adjoining members of the stimulus-series.

By means of this theoretical scheme and certain other assumptions regarding the shape of the generalization-curves and the summation of excitatory and inhibitory tendencies, the writer has shown not only that the phenomenon of transposition can be deduced, but that it would be somewhat less perfect the farther one departs from the original training pair, and the larger the original difference between the training stimuli. These theoretical implications have been substantiated by evidence already in the literature³ and also by experiments with chimpanzees recently reported.⁴

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¹ Kurt Koffka, *The Growth of the Mind*, 1928, 153-159, 233-242; Wolfgang Köhler, *Aus der Anthropoidenstation auf Teneriffa*. IV. Nachweis einfacher Strukturfunctionen beim Schimpansen und beim Haushuhn: Über eine neue Methode zur Untersuchung des bunten Farbensystems, *Abh. preuss. Akad. Wiss.*, 1918, 1-101.

² K. W. Spence, The differential response in animals to stimuli varying within a single dimension, *Psychol. Rev.*, 44, 1937, 430-444.

³ Harold Gulliksen, Studies of transfer of response. I. Relative versus absolute factors in the discrimination of size by the white rat, *J. Genet. Psychol.*, 40, 1932, 37-51.

⁴ Spence, "Relative" vs. "absolute" size discrimination by chimpanzees, *Psychol. Bull.*, 35, 1938, 505.

The purpose of the present experiment was to test further certain implications of this theory, particularly with reference to the differential generalization of inhibition or non-reinforcement effects.

METHOD AND PROCEDURE

Subjects and apparatus. Four separate and somewhat different tests were carried out with four chimpanzees. All of the Ss had had considerable training in discrimination experiments. Each had within the previous year or two been trained to differences of size, tested for transposition, then trained to the reverse size and again tested for transposition. The present experiments were carried out some 4 mo. following the latter work. All the Ss were adult chimpanzees, members of the Florida colony of the Yale Laboratories of Primate Biology.

A detailed description of the discrimination apparatus employed has been described elsewhere.⁵ Briefly, it consisted of one or two small stimulus-objects (food-boxes), which were presented to the animal by pushing the platform on which they were placed to 1 in. from the cage screen, where the animal could reach its fingers through the 2-in. wire mesh, open the boxes, and obtain the food. Stimulus-forms, squares cut from No. 28 galvanized iron and enameled a glossy white, were clamped to the front of the boxes.

Experiment I. In the first experiment, a full grown adult chimpanzee, named Jack, was used. In a preliminary test of 30 trials, Jack was confronted with two white squares, 256 and 160 sq. cm. in area, respectively, and was rewarded with food whichever member of the pair he chose. This series served as a control test exhibiting the relative excitatory strengths of the two stimulus-objects, which were, of course, a function of the animal's past experience. Following this preliminary test, squares of three different sizes (256, 160 and 100 sq. cm.) were presented, one at a time, in series of 5 trials. Responses to Squares 256 and 160—those used in the control test—were always reinforced by food, but responses to Square 100 were never reinforced—the food-box always being left empty. After five days of training, during which each of these squares was given a total of 50 trials, the original squares (256 and 160) were presented again. As may be seen from Table I, instead of an 83% choice of the smaller square as in the control tests, Jack now chose the larger square in all 10 trials (100%). A similar result was obtained the following day after 10 more presentations of each of the stimulus-objects singly.

It will be observed that this reversal of the response to the stimulus-combination, Squares 256 and 160, fits nicely within the theoretical

⁵ Spence, Analysis of formation of visual discrimination habits in chimpanzee, *J. Comp. Psychol.*, 23, 1937, 77-100.

picture presented by the writer.⁶ The results tend to support the interpretation that the inhibition developed with frustration on Square 100 generalized as a gradient so as to weaken Square 160 more than Square 256 and thus reverse the relative effective excitatory strengths of these two stimulus-areas. None of the Gestalt theories, in their present forms at least, is able to explain such a reversal of response on the test-combination since reversed training was not given to it, nor to any other similar configuration. That is, the intervening training involved no experience with a configuration but involved only single stimulus-objects, and yet the animal now responded quite differently to the combination.

Experiment II. A second and similar experiment was carried out with another *S. Pan*, who had shown, in the preliminary control series, a prefer-

TABLE I

RESULTS OF EXPERIMENT I, SHOWING THE NUMBER OF TIMES SQUARES 256 AND 160 WERE CHOSEN IN THE CONTROL AND TEST SERIES, AND THE NUMBER OF TIMES SQUARES 256, 160, AND 100 WERE PRESENTED IN THE TRAINING SERIES

Control series (times chosen) Squares		Training series (times presented) Squares			Test series (times chosen) Squares	
256	160	+256	+160	-100	256	100
5	25	50	50	50	10	0
		10	10	10	10	0

+ = reinforced; - = not reinforced.

ence (90%) for Square 256 over Square 160. A similar training period was then given this *S.*, in which a large stimulus-area (409 sq. cm.) was used in place of the small area in Experiment I. Responses to Square 409 were never followed by food whereas responses to Squares 256 and 160 were always reinforced. As will be seen from Table II, 40 trials were given with each of the Squares 256 and 160, and 60 trials with Square 409 in a four day period, after which a test-series was given with the control pair, Squares 256 and 160. While the results do not show such a clear cut reversal as in Experiment I, Pan did show a shift in his preferential response toward Square 160.

A further series of training trials was given for three more days, involving 45 frustrations on Square 409 and 25 reinforcements on each of the Squares 256 and 160. As Table II shows, the results of a second test with

⁶ Spence, The differential response in animals to stimuli varying within a single dimension, *Psychol. Rev.*, 44, 1937, 430-444.

the Squares 256 and 160 were less clear cut. On the day following this second test; 25 more non-reinforced trials to Square 409 were given, but with no change in Pan's performance. When 20 non-reinforced trials were given directly to Square 256, however, the responses in the test-combination shifted completely to Square 160. Such dominance of large stimulus-areas over smaller has been frequently noted. The writer, for example, never has been able to obtain transposition with chimpanzees, after training them to respond to the smaller of two stimulus-objects, when still smaller objects were employed in the transposition-tests.

Experiment III. In the third experiment, with Lia as S, the preliminary control series involved two stimulus-combinations: a 202 sq. cm. square

TABLE II

RESULTS OF EXPERIMENT II, SHOWING THE NUMBER OF TIMES SQUARES 256 AND 160 WERE CHOSEN IN THE CONTROL AND TEST SERIES, AND THE NUMBER OF TIMES THE SQUARES 256, 160, AND 409 WERE PRESENTED IN THE TRAINING SERIES

Control series (times chosen) Squares			Training series (times presented) Squares			Test series (times chosen) Squares		
256	vs.	160	-409	+256	+160	256	vs.	160
18		2	60	40	40	4		6
			45	25	25	6		4
			25	0	0	6		4
				(-20)		0		10

+ = reinforced; - = not reinforced.

vs. a 303 sq. cm. square, and then a 303 sq. cm. square vs. a 455 sq. cm. square. Following these control experiments, a daily training series, involving the reinforcement of 10 responses to Squares 202 and 303 and the non-reinforcement of 20 responses to Square 455, was carried out for three days. At the beginning of the test day the Squares 202 and 303 were presented together, response to either being rewarded. On the following day, after 5 training trials were given to Squares 202 and 303 and 10 trials to Square 455, two further test series of 10 trials each were given: one with Squares 202 vs. 303 and one with Squares 303 vs. 455. The results of these pre-training and post-training experiments are shown in Table III. It will be seen that Lia responded in chance fashion in both the pre-training (control) tests, but after the training series responded 100% of the time to the smaller square in both combinations. Care was taken in these tests to avoid the possibility that the animal was responding on the basis of some other cue aspect on the stimulus forms than their area by substituting duplicate forms.

Experiment IV. The fourth experiment was somewhat more elaborate than the first three. The S, an adult female named Mimi, was first given a series of preference tests involving six different sized squares, 60, 90, 135, 202, 303, and 455 sq. cm., respectively. The method of paired comparisons was employed: every square being paired with every other one for 10 trials. Throughout these tests every response made was reinforced by food. A total of three days was required for these tests, five being given each day.

The training series consisted of the presentation of a single stimulus-box at a time as in the earlier experiments. The procedure followed was that of presenting Square 135 without reinforcement for five trials alternated with a series of five trials in which each one of the other five squares

TABLE III

RESULTS OF EXPERIMENT III, SHOWING THE NUMBER OF TIMES STIMULUS-COMBINATIONS 202 VS. 303 AND 303 VS. 455 WERE CHOSEN IN THE CONTROL AND TEST SERIES, AND THE NUMBER OF TIMES THE STIMULUS-AREAS 202, 303, AND 455 WERE PRESENTED IN THE TRAINING SERIES

Control series (times chosen) Squares				Training series (times presented) Squares			Test series (times chosen) Squares			
202 vs. 303	303 vs. 455			+202	+303	-455	202 vs. 303	303 vs. 455		
9	11	10	10	30	30	60	10	0	—	—
				5	5	20	10	0	10	0

+ = reinforced; — = not reinforced.

was presented once and always reinforced. After two days of such training, involving 50 non-reinforcements on Square 135 and 10 reinforcements on each of the other five squares, a second series of paired comparisons tests was instituted, involving all the combinations. These latter tests were again spread out over three days with a training series of 10 trials (five trials with Square 135 and one with each of the other squares) at the beginning of the second and third days.

The results of the pre-training tests appear in Table IV, which shows the proportion of times in 10 trials the square indicated at the top of a column was preferred over the squares to the left. The values in parentheses are the theoretical proportions that would be expected if a square was presented with another of same area. These values are included in order to compute rough scale values by averaging the proportions of each column as suggested by Guilford.⁷ The latter scale values are shown at the bottom of the table. They appear also as the solid line graphed in Fig. 1. It will be seen that Square 202 is the most highly preferred. This graph may be taken as

⁷ J. P. Guilford, *Psychometric Methods*, 1936, 236-238.

giving a rough, but fairly good measure of the relative excitatory strengths of the various sized squares prior to the training series.

The lower, broken line represents the relative response-evoking strengths

TABLE IV
RESULTS FOR PRE-TRAINING TESTS IN EXPERIMENT IV
Proportion (p) of the times the square given at the top of the column was preferred to those at the left.

Square	60	90	135	202	303	455
60	(.50)	.80	1.00	1.00	.60	.30
90	.20	(.50)	.50	.80	.40	.00
135	.00	.50	(.50)	.50	.00	.00
202	.00	.20	.50	(.50)	.00	.00
303	.40	.60	1.00	1.00	(.50)	.10
455	.70	1.00	1.00	1.00	.90	(.50)
Mean p	.30	.59	.76	.80	.40	.18

of the squares as determined by the method of paired comparisons after the training series. These values, obtained in the same manner as those for

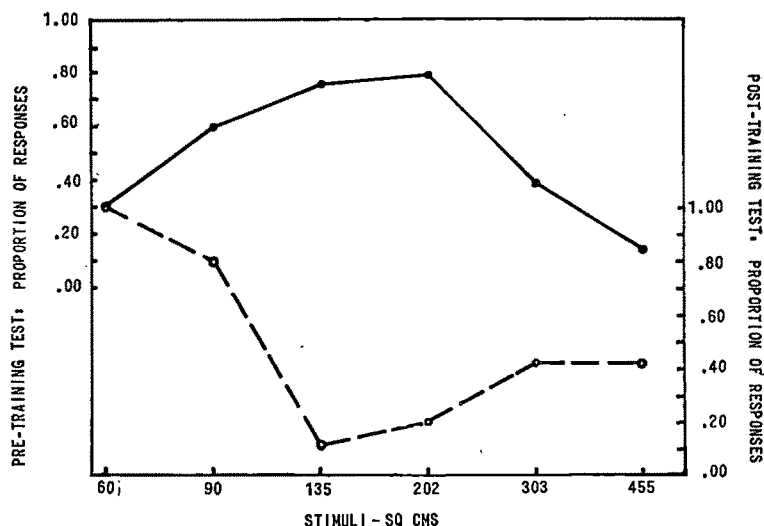


FIG. 1. RELATIVE STRENGTH OF S'S PREFERENCES FOR THE DIFFERENT STIMULUS-SQUARES IN THE PRE-TRAINING AND POST-TRAINING EXPERIMENTS

the pre-training tests, should be read by means of the scale to the right of the figure. It will be observed that Square 135 is now chosen the least frequently and Square 60 the most frequently. In order to gain a rough indication of the nature of the generalization of the inhibition-curve we

have arbitrarily placed the scale-value of Square 60 on the post-training test at the same point on the graph as it was on the pre-training tests. By subtracting the distance between the two points for each square we can get some indication of the relative amount of weakening (or inhibition) at each stimulus-point resulting from the frustration at Square 135. The results of Fig. 1 suggest that the shape of the generalization curve in the direction of larger stimulus-areas differs from that of the smaller. Of no little significance in this connection is the fact that the assumption of such generalization-functions as are indicated would agree in its implications with the facts of failure of transposition in the direction of smaller stimulus-areas after training to the smaller area of a pair.

There are several further interesting facts revealed by this graph. First of all, it should be noted that in preliminary tests the animal did not show any consistent larger or smaller response. Nor was this the case after the training period with single stimulus-areas. *S*'s behavior cannot be accurately described as reacting on the basis of some relationship such as larger or smaller. Also, the reversals are of considerable interest. Thus Mimi chose Square 90 over Square 60 before the training series, and Square 60 over Square 90 after it—a shift from larger to smaller preference as a result of the training to isolated stimulus-areas. Yet the same training produced just the opposite change on the other side of Square 135; Square 202 (smaller) being chosen over Square 303 (larger) before training, and the reverse (larger over smaller) after training. The inconsistency of one or the other, and possibly all, of these results with current Gestalt theory is too obvious to need elaboration.

SUMMARY AND CONCLUSIONS

Four tests with chimpanzees which had been trained to respond to differences in size showed that response to any stimulus-configuration (combination) can be altered, even reversed, by appropriate training on single, isolated stimuli. The results are interpreted as supporting the writer's assumption of the operation of differential generalization of conditioning effects, particularly inhibition, to explain the transposition phenomenon.

A STUDY OF PERSONAL VARIATION IN HAND-ARM STEADINESS

By CONSTANCE LOVELL, University of Southern California

Since recognition of the importance of individual differences has shifted the emphasis of psychology from study of the "average man," many experiments have been performed to show, quantitatively, how individuals are unlike each other. Most of them have used the average of a series of trials to express each subject's performance. In such a series, the individual's level of performance usually varies from one trial to the next. If persons differ not only in average performance, but also in degree of variation around that level, a measure of the dispersion might furnish valuable information concerning them. This experiment was a preliminary study of such measures of personal variation in the field of hand-arm steadiness.¹

Review of the literature indicates that there is a trend away from considering variation as a hindrance in the treatment of data and toward the idea that it may not be altogether lawless—that, indeed, it may even furnish valuable information concerning performance. In early studies,² it was assumed that degree of variation was typical of individuals, but more recently the necessity of testing this assumption has been recognized. From a study of 2300 Ss over a 5-yr. period, Gemelli concluded that individuals could be classified according to the speed and variation they showed in reaction-time measurements.³ Flügel found that oscillation in adding-test scores for two periods of 23 days each correlated to the extent of ± 0.84 , indicating considerable consistency in individual variation for such a situation.⁴ Lennes and Fee found that variation in different school tests, for a set-up in which the various tests in each series were given on different days, showed positive correlations, though not very high ones.⁵ Finally, Asch found that single deviations from the theoretical norm of performance of an individual were relatively consistent from day to day, and that there was present in his functions a slight factor of efficiency which resulted in a positive correlation of the variations found in different tasks on the same day.⁶

Preliminary studies by Washburn⁷ and Walton⁸ have indicated a relationship

* Accepted for publication October 4, 1940.

¹ This study was directed by Dr. Milton Metfessel.

² G. M. Stratton, Psycho-physical tests of aviators, *Sci. Mo.*, 8, 1919, 421-426; R. M. Yerkes, The variability of reaction-time, *Psychol. Bull.*, 1, 1904, 137-146.

³ A. Gemelli, Sul valore dei tempi di reazione semplice specie in ordine all'applicazione di essi alla selezione personale, *Pubbl. Univ. Cattol. d. S. Cuore*, 6, 1931, 485-500.

⁴ J. C. Flügel, Practice, fatigue, and oscillation, *Brit. J. Psychol. Monog. Suppl.*, 13, 1928, 1-92.

⁵ N. J. Lennes and I. B. Fee, Variability of individual performance, *School & Soc.*, 29, 1929, 264-268.

⁶ S. E. Asch, An experimental study of variability in learning, *Arch. Psychol.*, 21, 1932 (no. 143), 1-55.

⁷ M. F. Washburn, K. Keeler, K. B. New, and F. M. Parshall, Experiments on the relation of reaction-time, cube fluctuations, and mirror drawing to temperamental differences, this JOURNAL, 41, 1929, 112-117.

⁸ R. D. Walton, The relations between the amplitude of oscillations in short-period efficiency and steadiness of character, *Brit. J. Psychol.*, 27, 1936, 181-188.

between degree of variation and temperamental differences as judged by ratings. Another application of measures of personal variation has been made by Guidi and Vampa, who concluded from a study of factory mechanics that variation in the daily hourly performance of a worker was symptomatic of his degree of ability.⁹

Problem. The possible value of a measure of personal variation depends on the extent to which variation is characteristic of the individual, at least for the type of behavior in question. If from one series of trials to the next, such variation showed no consistency,¹⁰ a single measure would have little descriptive worth. It was the purpose of this experiment to investigate consistency of variation in the field of hand-arm steadiness, by correlating the measures of variation for a group of individuals on two series of trials, approximately one month apart.

Preliminary investigation indicated that computation of results in terms of the 12-sec. trials of steadiness used might cover up differences in variation present when measures for the same trials were calculated in terms of smaller time intervals. For instance, two scores for the 12-sec. trial might be the same, 25, but when broken into 3-sec. intervals, the scores of the first might consist of 6, 6, 6, and 7 and those of the second might be 12, 11, 1, and 1. The second phase of this problem was a study of the possibility that degree of variation might be highly specific to the size of time interval; the investigation involved correlating the measures of variation computed for the same series of trials in terms of three different time intervals.

Apparatus. The Seashore Photo-Electric Target Register was used in this experiment to measure hand-arm steadiness.¹¹ It operates as follows. A beam of light is projected through a small round window in the end of a cylinder which leads to a photo-electric cell. The minute electric current, caused by the light-beam falling on the cell, operates a sensitive relay, and the movements of this relay are recorded by an impulse counter. The apparatus is so constructed that the speed of operation of the relay and, thus, the impulse counter score are in relation to the amount of light which falls on the cell. To differentiate between those of varying degrees of hand-arm steadiness is a lever, suspended in front of the window through which the light reaches the cell. On the end just in front of the window is a target of the same size and shape as the window. If the lever is held directly in front of the window and entirely steady, the target prevents any light from reaching the cell. The slightest movement of the lever permits some light to enter. The S, holding the other end of the lever, keeps the target in front of the window as best he is able. The apparatus gives measures of unsteadiness in terms of the number of movements of the impulse counter which result from failure to hold the target in front of the window. The scores are products of (1) the deviation of the target from the center and (2) the time of that deviation. Hand-arm steadiness has been

⁹ P. Guidi and D. Vampa, *Abilità e variabilità nel produrre*, *Organizz. sci. d. lavoro*, 11, 1933, 73-78. Abstracted in *Psycholog. Abst.*, 8, 1934, (1275), 130.

¹⁰ The term "personal variation" has been used in this study to characterize the changes in an individual's performance that are found in a number of trials of the same activity within a short period of time. To avoid confusion, the term "consistency" has been used only to express the degree to which variation on one series of trials is characteristic of variation on a second series.

¹¹ A description of this apparatus is under preparation by Dr. Robert H. Seashore.

defined in terms of scores on this apparatus for the present study. The range of possible scores for the 12-sec. period of testing used was from 4 to 288.¹²

Because it was desired, in this experiment, to divide each trial into fractional parts, the impulse counter was wired so that each movement of it would be recorded on a moving kymograph drum. A machine recording time in seconds was attached to give a time line on the kymograph records.

Procedure. The Seashore apparatus and the following procedure were used throughout the experiment.

(1) *Preliminaries.* Preceding the arrival of the S, the apparatus was adjusted with a stopwatch. A high frequency of 1440 movements of the impulse counter per min. and a basic frequency of one movement in 3 sec. were decided upon. The apparatus was set at those speeds for every S so as to insure comparable records.

(2) *Instructions.* After the apparatus was explained to S and directions were given regarding its operation, the following instructions were read to him.

The object of this experiment is to measure the steadiness of your arm and hand. You are to manipulate the target on the end of the lever rod so that the beam of light will not be able to enter the little circular window. The more light you are able to intercept, the better your score.

Breathing affects the scores on this test. To control this factor, this experiment will involve holding the breath.

You will be given a series of 12-sec. trials. When I say 'Ready,' take hold of the lever as you have been directed and hold it as steady as possible. When, after a few seconds, I say 'Hold,' begin holding your breath. When I say 'Stop,' you may take your hand from the apparatus until I say 'Ready' again.

Questions concerning the apparatus were then answered. As far as most of the Ss knew, the object of the experiment was to obtain a measure of steadiness. It is fairly certain, therefore, that no attempt was made to control directly the factor of variation.

(3) *Practice series.* Three practice trials were given every S to familiarize him with the apparatus and to make certain that he understood the instructions. The practice series also served to reduce variations due to initial adjustments.

(4) *Checking apparatus.* Before the main series of trials, the high frequency movements of the impulse counter were checked. The full beam of light was allowed to fall upon the window and a kymographic record was made of the impulses. The record thus obtained was used in analyzing the comparability of the records of the various Ss.

(5) *Main series.* The 25 trials of the main series were divided at the thirteenth trial, when S was given a 3-min. rest. During this interval, E changed the kymograph drum. Between the other successive trials, a 19-sec. rest-period was allowed to elapse.

(6) *Checking apparatus.* After the main series of trials, both the high and basic frequencies of the impulse counter were again tested—the high frequencies by

¹² Even when no light falls on the cell, movements are registered by the impulse counter from three times every second to one every 5 sec., depending on conditions in the atmosphere, the electric current, and the adjustment of the machine. This is the basic frequency of the apparatus, and it can be regulated so as to be equal from one testing period to the next. Similarly, the high frequency (movements of the impulse counter when all the beam of light falls on the cell) varies between 1340 and 1560 movements per min., depending on conditions which vary from day to day. This frequency is also adjustable.

means of the kymograph, as in (4), and the basic frequencies by means of a stopwatch.

Subjects. The Ss were 50 in number—47 students and 3 others of college age. Of the 50 Ss, 28 were men and 22 were women.

Experiments. The experiments in the main series consisted of 25 trials, and every trial was of 12-sec. duration. Forty of the Ss repeated the experiments a second time, four weeks after their first performance. The remaining Ss, 10 in number, did not repeat the work; for them we have only a single set of records.

Treatment of the data. For comparative purposes measures of both absolute and relative variation have been computed in this study. The average deviation (*AD*) was used as the measure of absolute variation in preference to the standard deviation because there were numerous gaps in the frequency distributions toward the extremes. The standard deviation gives more weight to extreme scores of a

TABLE I
CORRELATIONS OF MEASURES OF VARIATION COMPUTED FROM SAME TRIALS
IN DIFFERENT TIME-UNITS

Variables	Measure	AD	RV	SSD
1- and 12-sec. intervals	<i>r</i>	$+.89 \pm .02$	$+.74 \pm .04$	$+.74 \pm .04$
	<i>r</i> ²	.79	.55	.55
3- and 12-sec. intervals	<i>r</i>	$+.92 \pm .02$	$+.85 \pm .03$	$+.72 \pm .05$
	<i>r</i> ²	.85	.72	.52

distribution than does the measure chosen. The relative variability (*RV*) is the average deviation divided by the mean and multiplied by 100, *i.e.* $RV = 100 AD/M$. It was chosen to give a measure of relative variation about the mean.

Examination of the records indicated, in some cases, either a practice or a fatigue effect. Both of these factors tend to increase variation of performance. To lessen their effect, the variation from trial to trial was calculated by subtracting each score from the one before it and averaging these results for each series of trials. This measure is called the average score to score deviation (*SSD*).

Results. (1) Relation of time-interval to measures of variation. Using 50 records of steadiness, the correlation of the different measures of variation for the 1-, 3-, and 12-sec. intervals was calculated.¹³ The results are given in Table I. The correlation coefficients of all the measures of variation are sufficiently high to indicate that large variations in the 1- and 3-sec. intervals tend to be paralleled by large variations in the 12-sec. interval. Analysis of these correlations may be made in terms of the percentage of the variance of one variable that is associated with the variance of the other variable (given as *r*² in Table I).¹⁴ For the data involved in this

¹³ The 1-sec. period and 12-sec. period were used because they were the smallest and largest for which there were accurate measures. The 3-sec. interval was selected as the intermediate one because the basic frequency of the instrument involved one movement of the impulse counter every 3 sec. Use of this interval insured equality of the periods as far as the machine was concerned.

¹⁴ H. E. Garrett, *Statistics in Psychology and Education*, 1937, 355.

study, the association ranges from 52% to 85%. Such figures appear high until it is realized that these measures were calculated from the same trials merely by using different combinations of the total number of units. Means for the same scores with the same three time-intervals should correlate ± 1.00 . Several factors may have been involved.

(a) The cancelling out, mentioned previously, may have proceeded irregularly, so that the Ss with different degrees of variation on the smaller intervals showed relatively the same variation on the larger ones.

(b) The scores for the different time-periods are not exactly comparable. This is particularly true of the 1-sec. intervals in comparison with the others since, in every third 1-sec. period, the basic frequency of the machine caused the recording of one movement regardless of how steady the individual might have been.

(c) Again, a large number of the Ss obtained zero readings for many of the 1-sec. periods, indicating that the instrument was not sensitive enough for use with such small time-intervals. Only a few Ss obtained the lowest possible score for the 3-sec. intervals, and none obtained it for the 12-sec. interval. The differ-

TABLE II
CORRELATION OF MEASURES OF VARIATION FROM THE TWO TESTING PERIODS

Variables	Measure	AD	RV	SSD
12-sec. intervals	r	$+.64 \pm .06$	$+.49 \pm .08$	$+.59 \pm .07$
	r ²	.41	.24	.35
3-sec. intervals	r	$+.68 \pm .05$	$+.58 \pm .07$	$+.69 \pm .05$
	r ²	.46	.34	.48
1-sec. intervals	r	$+.74 \pm .05$	$+.56 \pm .07$	$+.71 \pm .05$
	r ²	.55	.31	.50

ential effect of this might have influenced the size of the correlation between the various time-periods.

Because of the limitations of the apparatus noted above, it was impossible to show the differences caused by the irregular cancelling of the steadiness variations. The correlations among the three time-periods studied are so high, however, that it seems unlikely that irregular cancelling had much effect. It is highly probable, therefore, that no serious error would have been made if any one of the three time-intervals had alone been used as the basis of comparison.

(2) *Consistency of variation over a period of time.* The correlations obtained from the records of the 40 Ss who repeated the experiments after an interval of approximately four weeks are given in Table II. Comparison of the correlations for the three time-intervals is given there. Because of the greater effect of the apparatus-limitations at the smaller time-intervals, our chief consideration is to the results of the 12-sec. period.

Analysis of the findings revealed that the average deviation (AD) of an S on the second trial could be predicted from his (AD) in the first test-series with an accuracy 23% greater than that obtained by chance alone. The corresponding figures for the relative variability (RV) and the average score to score deviation (SSD) are 13% and 19%, respectively. Although variation as measured in this study was

not sufficiently consistent for accurate individual prediction, the correlations were high enough to give definite indication of some common or similar influences affecting individual variation on both series of tests. Examination of the results in terms of variance gives a quantitative expression of this relation. For the *AD* measures, 41% of the variance of the second test was associated with, or dependent upon, the variance of the first test, and 59% of it was associated with other factors. For the measure of *RV*, 24% of the variance of one series was associated with the variance of the second test, and for the *SSD*, the figure is 35%. In other words, despite differences in physical condition, mood, etc., which might be expected to have large influence on the scores for any one day, there were factors at work making the variations on two sets of trials, four weeks apart, similar for every *S*.

The discussion of the results presented so far has treated the three measures of variation as if they were equally valuable in describing the situation. As stated previously, the influence of learning on the scores made the *SSD* seem the most useful of the three. Part of the correlation between the series for the average deviation and relative variability might have been merely a measure of the relation between the learning involved in the two situations. The *SSD* does eliminate this effect to a considerable extent by measuring the deviation of each score only in terms of the one nearest it. A rough check of this was made by correlating the *SSD* measures with the differences found between the means for the first and last 12 trials of the first series. The correlation obtained was $+0.39 \pm .08$, indicating that, if all the scores had shown the same difference between the means (a crude measure of learning), 85% of the variance in the *SSD* measures would have remained. Although this measure of learning was inexact and incomplete, the results of the correlation were indicative of the utility of the *SSD* measure. On the other hand, the correlation between the average deviation and the *SSD* was $+0.84 \pm .03$, indicating that if everyone in the group had had the same average deviation, 77% of the variance in the *SSD* measures would have been removed.

Attempts to interpret the results of this experiment force clear recognition of the limitations of the measures used. The agreement of all of them, if not their validity as single measures, seems, however, to justify the conclusion that there was present in this study a consistency of variation large enough to be valuable in describing performance. Such a conclusion agrees with the findings of other experimenters.

SUMMARY

This experiment investigated, in the field of hand-arm steadiness (1) the relation of measures of variation for the same series of trials computed in terms of three different time intervals, and (2) the relation of measures of variation for two series of trials separated in time. Fifty *Ss* of college age made 25 records, each of 12-sec. duration, of hand-arm steadiness as measured by the Seashore Photo-Electric Target Register. Forty of them took another series of trials approximately one month later. Analysis of the records for such factors as differences in speed of machine, time of day, and number of days between trials seemed to justify use of the scores as comparable.

Correlations of the variation as expressed in the three different time-intervals—using the average deviation (*AD*), the relative variability (*RV*), and the average score to score deviation (*SSD*) as measures—ranged from $+0.72 \pm .05$ to

THE CHRONAXY OF PRESSURE IN HAIRY AND HAIRLESS REGIONS

By F. NOWELL JONES and MARGARET H. JONES, University of Alabama

Although several reports have been made of the chronaxy of pressure,¹ only Neff and Dallenbach have obtained measures under conditions which insured adequate differentiation of pressure and pain.² Their chronaxies were derived entirely from experiments upon the dorsal forearm, a hairy region. Since it is generally accepted that pressure is mediated by nerve endings surrounding the hair follicles in hairy regions, and by Meissner's corpuscles in non-hairy regions, we wished to measure the chronaxies of a hairy and a non-hairy region, and thus compare the excitability of two dissimilar end-organs which, peculiarly, subserve the same sense.

Method. As the source of the electrical pulses necessary for our measurements, we used a condenser-discharge chronaximeter, as described by Kreezer and Bradway.³ Here the time of discharge is determined solely by the capacity of the condenser and the resistance through which it discharges. By means of a simple constant, 0.37, it is possible to equate, in terms of physiological effect, the exponential discharge of the condenser to the rectangular pulse of electricity ideally used for the determination of chronaxies. The negative electrode consisted of a glass funnel, containing a silver wire, filled with salt solution, from which projected a short wick about 1.5 mm. in diameter. The positive electrode was a large sheet of copper screening, sewed into a cover of linen cloth, and which was wet with salt solution. *O* simply laid upon this electrode the arm or hand being stimulated. The authors *F* and *M*, served alternately as *E* and *O*.

The determinations for the hairy regions were made upon the dorsal forearms, 20 measures being taken for each arm—a total of 40 for each *O*. For the hairless regions the fingertips were chosen. Twenty measures were made upon each hand, or 40 in all for each *O*. Usually five chronaxies were obtained from each experimental region in the course of one sitting. It should be noted that it often happens that the electrical stimulus will excite a nerve-trunk rather than an end-organ. *O* can usually detect this occurrence by the diffuseness and peculiar quality of the sensation. No such case has been included among our data.

The rheobases, which have not been previously reported, were determined by means of the discharge from a 28 mf. condenser, which gave a pulse equivalent to a rectangular pulse of 119 ms. This is well above physiological infinity, as is

* Accepted for publication October 31, 1940. This research was made possible by a grant from the University Research Fund of the University of Alabama.

¹ H. Stein, Untersuchungen über die Chronaxie der Sensibilität, *Dtsch. Zsch. f. Nervenheilk.*, 100, 1927, 221-232; H. Ruffin, Chronaximetrische Untersuchungen des sensiblen und optischen Apparates, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 140, 1932, 641-656; K. M. Walthard and A. Weber, Zur Chronaximetrie der normalen Hautsensibilität des Menschen, *ibid.*, 67-88. L. J. Hut, *De sensibele chronaxie*, 1936, 88, has also made measurements using the technique of Neff and Dallenbach, but his results are not readily available for comparison.

² W. S. Neff and K. M. Dallenbach, The chronaxy of pressure and pain, this JOURNAL, 48, 1936, 632-637.

³ George Kreezer and K. P. Bradway, Relation between Binet mental age and motor chronaxy, *Arch. Neurol. & Psychiat.*, 34, 1935, 1149-1171.

evidenced by the fact that a pulse of 68.2 ms. required the same intensity to excite.

Finally, two strength-duration curves were obtained from hairy and hairless areas for each *O*.

Results. Table I shows the average rheobases and chronaxies with their S.D._{dis.} for the hairy and hairless regions for each *O*. It will be seen that the rheobases found in the hairless regions are higher than those in the hairy regions for both *Os*. This is undoubtedly due to the greater thickness of the skin on the fingertips, with a resulting insulating effect upon the end-organs lying beneath. It will be noted that the correspondence of the rheobases for the two *Os* is very close, which would indicate receptors lying at approximately the same depth in both cases. This is true because we have ruled out, by our use of a physiologically infinite current in our

TABLE I
AVERAGE RHEOBASES IN VOLTS AND CHRONAXIES IN MILLISECONDS FOR PRESSURE

O	Hairy regions				Hairless regions			
	Rheobase		Chronaxy		Rheobase		Chronaxy	
	Av.	S.D. _{dis.}	Av.	S.D. _{dis.}	Av.	S.D. _{dis.}	Av.	S.D. _{dis.}
<i>F</i>	34.23	9.04	0.081	0.053	63.98	7.48	0.082	0.044
<i>M</i>	34.90	8.66	0.123	0.047	64.98	8.80	0.053	0.018

rheobase determinations, the sensitivity of the end-organs themselves, and magnified the effects of the surrounding tissues, which act as insulators.

The chronaxies require somewhat more detailed consideration. It is apparent that genuine individual differences exist between the *Os*. For the hairy regions, the difference shows a *CR* (Diff./S.D._{dis.}) of 3.7, and for the hairless regions a *CR* of 3.8. Neff and Dallenbach report no reliable differences among their *Os* for the chronaxy of pressure.⁴ This difference in result seems to be due, however, mainly to the much greater dispersion of their measures as compared with ours. Thus their *CRs* do not show reliable differences, even though absolute differences did exist. That individual differences should exist would seem, however, less surprising than that they should not exist, considering the results to be expected from measurements of any physiological function.

When we consider the differences between hairy and non-hairy regions, it appears that whereas *M* shows a reliable difference in chronaxy (*CR* 8.8), *F* shows no reliable difference (*CR* 0.06). This apparent anomaly may be explained in the following manner. As among individuals, the chronaxies of the hair follicles and of Meissner's corpuscles may vary with relative independence. Thus, by chance, we should expect to find *Os* showing reliable differences between hairy and hairless regions, and also those who do not. Suppose, for example, that the average chronaxies for the hair follicles should show, for the population at large, a distribution curve, and that the chronaxies of Meissner's corpuscles for various individuals should also show a distribution curve, but with an average somewhat lower, and with some degree of overlap. Then some individuals will be found whose chronaxies lie in the area of overlap, and reliable differences will not appear. Similarly, it should occur that occasionally *Os* will be found who have chronaxies similar to those of each other,

⁴ Neff and Dallenbach, *op. cit.*

either for both or for one of the regions under consideration. Thus the independent variance of the two types of pressure end-organs may be considered to be, at least tentatively, established. That F should be one of the statistically to-be-expected individuals *not* showing this difference, is purely accidental.

The absolute magnitudes of the chronaxies from the hairy regions seem to corre-

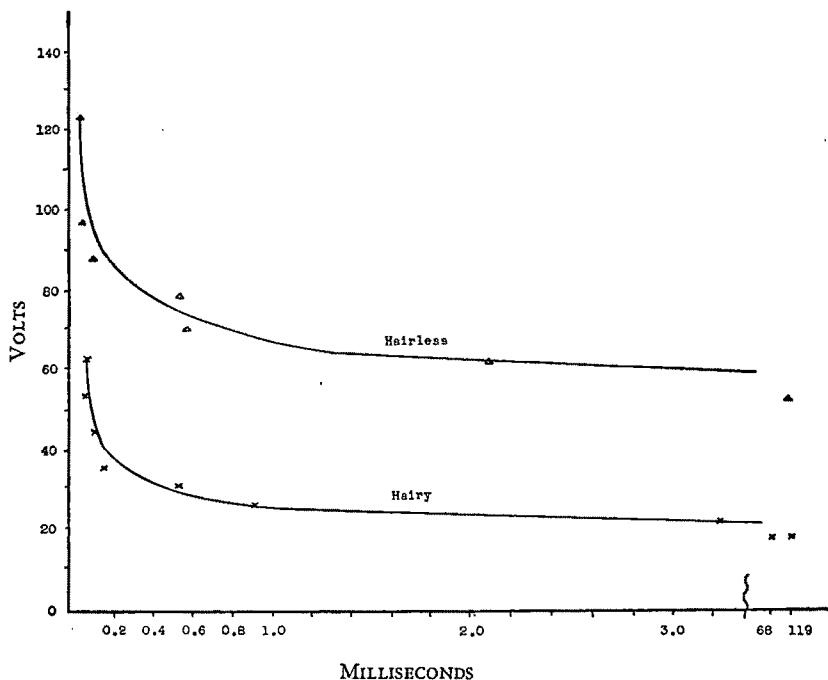


FIG. 1. F 'S STRENGTH-DURATION CURVES FOR PRESSURE FROM HAIRY AND HAIRLESS REGIONS

spond to those previously reported by Neff and Dallenbach. F shows a value somewhat shorter than any others reported, but not extraordinarily so. Therefore, our two additional cases may be considered to have corroborated the general range of magnitudes found for 3 O s by these previous investigators.

Fig. 1 shows F 's strength-duration curves for a hairy and a hairless region. It will be noted that the curve from the fingertip is displaced upward a considerable distance along the intensity parameter. This is, of course, simply another demonstration of the higher rheobases to be found in these regions.

Summary. (1) We have measured the chronaxies of pressure from hairy and from hairless regions. (2) In the case of one O there is highly a reliable difference between the measures from these two regions, and we have suggested that the chronaxies of the hair follicles and of Meissner's corpuscles vary independently. (3) Our values corroborate those obtained by Neff and Dallenbach. (4) We have shown that individual differences in chronaxy may be expected to exist.

THE CHRONAXY OF PAIN

By F. NOWELL JONES and MARGARET H. JONES, University of Alabama

In a previous paper, the senior author suggested a bi-modal distribution for the chronaxies of pain.¹ In order to test this hypothesis, and to add two more cases to the five reported in the literature where the chronaxies of pressure and pain were obtained from the same *O*,² we have measured the chronaxy for pain.

Method. A condenser-discharge chronaximeter, the same apparatus as was used in a previous investigation on the chronaxy of pressure,³ provided the electrical pulses

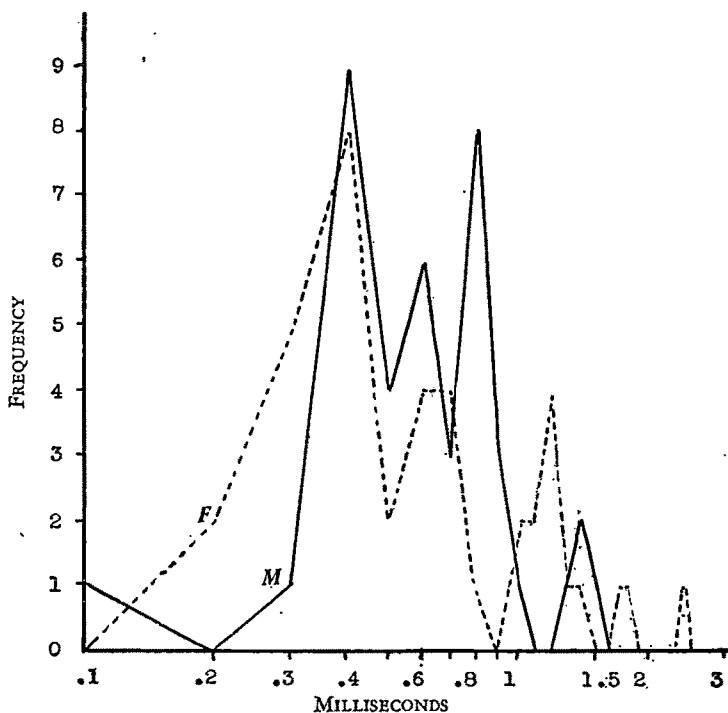


FIG. 1. DISTRIBUTION CURVES OF CHRONAXY FOR PAIN

* Accepted for publication December 9, 1940. This research was made possible by a grant from the University Research Fund Committee of the University of Alabama.

¹ F. N. Jones, The chronaxy of cold and warmth, this JOURNAL, 53, 1940, 224.

² W. S. Neff and K. M. Dallenbach, The chronaxy of pressure and pain, this JOURNAL, 48, 1936, 632-637; Jones, *op. cit.*, 221, 224.

³ F. N. Jones and M. H. Jones, The chronaxy of pressure in hairy and hairless regions, this JOURNAL, 54, 1941, 237-239.

necessary for our measurements. The authors, *F* and *M*, served alternately as *E* and *O*. Forty measures were made for each *O*, 20 upon each dorsal forearm.

When we began experimentation, it became immediately apparent that the chronaxy obtained from any given spot was extremely unstable, successive readings yielding widely divergent results. Also, the rheobases tended to shift by several volts in the course of a series of determinations. In an attempt to control this factor, we introduced the procedure of following each shock with a "neutralizing" shock, applied with a reversed polarity of the electrodes. This was immediately successful, and stability was thereafter the rule. Further, we rechecked the rheobases

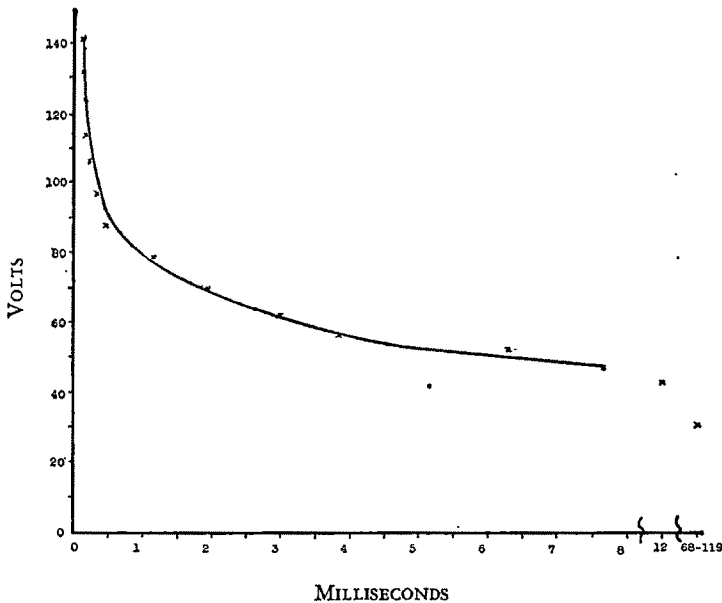


FIG. 2. STRENGTH-DURATION CURVES FOR PAIN FOR *M*

in each case following the determination of the chronaxy, and we have only reported those which checked within 2 v.

At the completion of experimentation, two strength-duration curves were obtained from each *O*.

Results. Table I shows the average rheobase and chronaxy for pain for each *O*, with the S.D._{abs.} for each average. We have also included the measures previously reported for the chronaxy of pressure from hairy regions, for purposes of subsequent comparison.⁴ The measures average larger than those reported by Neff and Dallenbach, but probably lie within the same general range. Distribution curves for the pain chronaxies are shown in Fig. 1. Although the curve for *M* might hint

⁴*Ibid.*

at bimodality, the strength-duration curves, of which Fig. 2 is a fair sample, do not force this interpretation upon us. Parsimony requires that we assume that these are irregular normal curves.

A comparison of the pressure and pain chronaxies bears out the results of Neff and Dallenbach. The critical ratios ($\text{Diff.}/\text{S.D.}_{\text{diff.}}$), 8.66 for *F* and 11.8 for *M*, assure us that the differences are statistically reliable. This adds further evidence

TABLE I

AVERAGE RHEOBASES IN VOLTS AND CHRONAXIES IN MILLISECONDS FOR PRESSURE AND PAIN

O	Pressure				Pain			
	Rheobase		Chronaxy		Rheobase		Chronaxy	
	Av.	S.D. _{dis.}	Av.	S.D. _{dis.}	Av.	S.D. _{dis.}	Av.	S.D. _{dis.}
<i>F</i>	34.23	9.04	0.081	0.053	21.70	5.80	0.792	0.514
<i>M</i>	34.90	8.66	0.123	0.047	29.20	7.00	0.661	0.283

of a genuine differentiation between the end-organs subserving pain and pressure, and thus lends support to the Von Frey theory. This is an important result, since the previous evidence from chronaxy was based on only four cases.⁵

Summary. (1) We have measured the chronaxy for pain, under circumstances assuring the stability of the measures. (2) We have found that there is no bi-modal distribution of pain chronaxies. (3) A comparison of pain and pressure chronaxies supports the view that these are separate sense modalities.

⁵ Neff and Dallenbach, *loc. cit.*

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY
OF CORNELL UNIVERSITY

XC. THE INFLUENCE OF EYE MOVEMENT AND POSITION ON
AUDITORY LOCALIZATION

By T. A. RYAN and FRANCES SCHEHR

There is a growing body of evidence that various portions of the sensory and motor systems, even portions which seem to be only remotely related, influence one another in a complex way. The senior author of this paper has summarized some of the evidence upon such interrelations in another place.¹ Among the researches considered in that summary was the work of Goldstein and various co-workers upon auditory spatial orientation under the influence of stimuli from other sense-departments. This paper is primarily a check of some of the findings which Goldstein has reported.

In studies of pathological cases in which a cerebellar or cortical lesion resulted in disturbances of tonus and posture in one side of the body, Goldstein also noted certain disturbances of spatial orientation. These disturbances affected not only tactile and kinesthetic perception but also visual and auditory spatial discriminations.² To take an example from the auditory studies, a patient with a left cerebellar disturbance tended to localize all sounds from the left too far to the left. The apparent median plane was also shifted toward the disturbed side. This was not a motor difficulty in pointing since the same errors were made with the undisturbed arm used as pointer.

There was thus a parallel between the spatial orientation and the motor disturbances. These same patients were unable to hold the arm straight ahead with the eyes closed. Instead, the arm would gradually wander to the side without the patient's knowledge. Goldstein found that a sound presented while the arm was thus wandering would also progressively change its position, and would return to its normal position when the arm was dropped to the patient's side.

Earlier experiments had discovered that pointing is influenced by the position of the eyes in the head.³ Although there is some conflicting evidence, Goldstein and

* Accepted for publication September 27, 1940.

¹ T. A. Ryan, Interrelations of the sensory systems in perception, *Psychol. Bull.*, 37, 1940, 659-698.

² K. Goldstein, Über induzierte Tonusveränderungen beim Menschen (sog. Halsreflexe, Labyrinthreflexe, usw.). VIII. Über den Einfluss unbewusster Bewegungen resp. Tendenzen zu Bewegungen auf die taktile und optische Raumwahrnehmung, *Klinische Wochenschrift*, 4, 1925, 294-299; K. Goldstein, Über induzierte Veränderungen des "Tonus," *Schweiz. Arch. f. Neurol. u. Psychiat.*, 17, 1926, 203-228.

³ B. Fischer, Der Einfluss der Blickrichtung und Änderung der Kopfstellung (Halsreflex) auf den Bärányschen Zeigerversuch, *Jahrb. f. Psychiat. u. Neurol.*, 35, 1915, 155-158; J. Kiss, Über das Vorbeizeigen bei forciertem Seitwärtsschauen, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 65, 1921, 14-17; K. Goldstein and W. Riese, Über induzierte Veränderungen des Tonus (Halsreflexe, Labyrinthreflexe und ähnliche Erscheinungen). III. Blickrichtung und Zeigerversuch, *Klin. Woch.*, 2, 1923, 2338-2340.

Riese conclude that movement of the closed eyes or any 'bare' eye movement causes deviations in pointing in the direction opposite to the eye movement. *Looking-at* something at the side, however, results in deviations in the same direction as the eye movement.

Because of these two facts (the parallel between motor disturbance and spatial orientation, and the effect of eye-movement upon the motor performance of pointing) Goldstein tried out the effect of eye movements directly upon auditory localization. The results, with both pathological and normal *Os* paralleled exactly the results of the pointing experiments.⁴ (1) When the closed eyes were rolled to one side, the apparent position of a sound shifted in the opposite direction. (2) When *O* looked-at something at the side, the shift in position of sound followed the direction of the eye movements.

These results could not be attributed to a motor disturbance in pointing out the apparent position of the sound. Goldstein worked with a system of tubes so that he could ask *O* to report verbally whether the sound appeared in the median plane or at one side without the sound source being visible. For every position of the eyes, the tubes could be adjusted until the sound was reported as coming from the median plane.

Goldstein does not state, however, how reliable these results were. The pathological *Os* were naturally few in number, but it would still be possible to determine the reliability of the shifts in localization for any given individual. This was not done either in the pathological or in the normal series. In reference to the normal *Os*, there is no statement of the number of *Os*, the number of observations made upon each individual, nor of the percentage who showed the effects. Goldstein implies that the only exceptions to the first general rule stated above occur when a certain *O* is unable to rotate his eyes to the side with the lids closed. Two cases where this happened are described, but even here it is not explicitly stated that these were the only *Os* who failed to show any shifts in localization with the eyes closed.⁵ As for the results of experiments with the eyes open and *O* looking toward something at the side, the authors state: "This result was also to be obtained for the majority of subjects, but was not as constant as the first."⁶

Goldstein's report is also incomplete in another way. We are not told enough about the conditions of the experiment or the exact instructions used, so that there is a definite possibility that 'suggestion' may have been important in determining the results obtained. For all of these reasons, a check upon the findings seemed to be in order.

Procedure. The major difference between our procedure and that of Goldstein was that we used a sound cage rather than adjustable tubes. Our sounds were presented in a way more like everyday auditory localization. The sounds were clicks produced by completing a battery circuit connected to a telephone receiver.

In order to eliminate pointing, which might itself be influenced by the position of the eyes, we required *O* to report by signals whether the sound appeared at the right, left, or in the median plane. In this way we were able to determine the

⁴ K. Goldstein, *op. cit.*, *Schweizer Arch. f. Neurol. u. Psychiat.*, 17, 1926, 203-228; K. Goldstein and O. Rosenthal-Veit, *Über akustische Lokalisation und deren Beeinflussbarkeit durch andere Sinnesreize*, *Psychol. Forsch.*, 8, 1926, 318-335.

⁵ Goldstein and Rosenthal-Veit, *op. cit.*, 332.

⁶ *Ibid.*, 332.

position of the apparent auditory median plane under each condition of the experiment.

Use of the sound-cage brings with it the disadvantage that the sound source must be screened from *O* during the experiments with the eyes open. At the same time it is necessary that the screen should not cut off the sound stimuli from direct passage to *O*'s ears. In order to accomplish this, a strip of cardboard 9 in. in width was bent to form a cylindrical surface extending three-fourths of the way around *O*'s head with a radius of 12 in. Fixation points were placed on the inside of this cylinder in the median plane and at 40° to the left and right of the median. Black cloth was draped over the top of this screen so that *O* could not see the upper parts of the sound cage as they moved about. The whole screen was fastened rigidly to a stand which also held a biting-board determining the position of *O*'s head.

Instructions. The instructions which were read to *O* were always in the following pattern. (Words in parentheses indicate the changes which were made for the various sections of the experiment.)

A click will be sounded. While listening for it keep your eyes closed (open) and fixate straight ahead (toward the left, right). After you have heard the click, raise your right hand if the sound came from the right and your left hand if it came from the left. Raise both hands if it came from straight ahead, and point both hands downward if the click came from directly behind. You will be given the signal 'ready—now' before each click is sounded.

The sounds were presented in the pattern of the method of limits. The first stimulus of a series was given at one side, and then moved by steps of 5° toward the median plane. A series was completed when *O* first reported that the sound appeared on the side opposite to that on which it had first appeared. We followed the conventional order of alternately ascending and descending series with a repetition of the ascending order in the eleventh series of a group.

The experiment as a whole is divided into two main sections: Section I with eyes closed, and Section II with eyes open. Each of these Sections was further divided. In Section I all sounds were presented in the front half of the field with three different positions of *O*'s eyes. The three divisions of this experiment will be designated as *S_I*, *L_I*, and *R_I* indicating the position in which *O* was instructed to hold his eyes while listening for the sound.

In Section II the sounds were presented in both the front and back halves of the auditory field. This part of the experiment included six divisions designated as *FS_{II}*, *FR_{II}* and *FL_{II}*, in which the stimulus was moved toward the front median position (*F*), and *BS_{II}*, *BL_{II}* and *BR_{II}* for those sections in which the stimulus was presented behind *O* (*B*). Here the position of the eyes was controlled by means of the three fixation-points.

The reason for introducing the added divisions in Section II was that the screen which prevented *O* from seeing the sound source was open at the back. We were quite certain that the screen was small enough to have little effect upon localization through blocking of stimuli, but we wished to make more certain by presenting the sounds from the back, where there was no obstruction at all. Certain additional by-products came from the results of this Section of the experiment.

Observers. The 8 *O*s of varying degrees of practice in observation were all undergraduate students at Cornell. They are identified in the summary of results by the

first two letters of each name. Two of these, *Ca* and *Ja*, were carried through the whole experiment first, as the technique was being devised. They therefore followed a different order of experimentation from that used for the remaining *Os*. The first two *Os* performed all of the 20 series of a single section of the experiment at a single sitting. Thus they carried out Section *S₁* in the first observation period, *R₁* in the second period, and so on, going through the various sections and divisions in the order in which they have been mentioned above.

For the other *Os* it was thought desirable to control the daily variations, if any, by a special order of procedure. These *Os* were given only five series at a time of any one section or division, after which the position of their eyes was changed and five other series were taken. In this way each average for these later *Os* represents four samples of five determinations each, obtained in four separate observational periods. For the first two *Os*, on the other hand, each average represents 20 determinations obtained at a single sitting.

Unfortunately *Ca* and *Ja*, the first two *Os* used, were the most highly practiced. The differences between their results and those of the other *Os* are, consequently, difficult to interpret.⁷

Results. The mean position of the apparent auditory front or back was determined for each *O* and for each position of the eyes. Table I summarizes the differences between these average positions for Section I with the eyes closed. Table II shows the results for Section II with the eyes open and a fixation-point provided.

The differences are entered in the tables with a sign to indicate whether the shift in the apparent auditory median plane is in the same direction (plus) or in the opposite direction (minus) from the change in position of the eyes. Thus a plus sign indicates that the apparent median was farther to the right when the eyes were rotated to the right than it was when the eyes were straight ahead or rotated to the left.

It was thought possible that these effects might be greater on one side than they were on the other. That is, a movement of the eyes from the front position to the left might have a greater or smaller effect than a movement from the front to the right side. It is even possible that opposite movements of the eyes might produce the same direction of effect rather than opposite effects. Because of these possibilities it was not sufficient to consider only the differences between the auditory front positions obtained with the extreme positions of the eyes. We must also compare each of these positions with the position of the median when the eyes are in their normal frontal direction. The differences in the tables are indicated in terms of L, R, and S. The critical ratio of each difference is entered in the succeeding column.

In discussing the results we shall consider critical ratios of three or more as significant. It has been suggested that this is a very stringent requirement, and Fisher accepts as significant any critical ratio where the probability of obtaining the value by chance is less than 0.01.⁸ Usually, where the number of cases is small, it is better to give the probability (*P*) values and allow the reader to evaluate them for

⁷ *Ca* had heard of Goldstein's results before observing in this experiment. She did not know, however, the specific directions of shift which Goldstein reported for various eye movements. In addition, she was doubtful about the validity of Goldstein's findings, so that this knowledge cannot be regarded as a suggestive factor in her reports.

⁸ R. A. Fisher, *Statistical Methods for Research Workers*, 7th ed., 1938, 131.

himself. In this case, however, it is not necessary to enter all of the P 's because all of our samples are of the same size ($N=20$). As a result of this fact, we can state limiting values of the critical ratio which will apply to all of the material in our tables.

It happens that there are two possible ways of determining P from our critical ratios. (1) If we determine the standard error of the difference according to the usual procedure, the number of degrees of freedom to be used in entering Student's tables of t would be 19. From this we find that a critical ratio of 3.0 corresponds to a probability of 0.0037, and a critical ratio between 2.5 and 2.6 would correspond to a probability of 0.01.⁹ These are the probabilities of finding differences of like size and of *like sign*.

(2) By assuming the null hypothesis and Fisher's method of pooling the squared deviations, we can get a different estimate.¹⁰ It happens that this new estimate of t is exactly equal to the critical ratio determined in the usual fashion and entered in our tables. This is a result of the fact that our samples all have the same number of cases. The number of degrees of freedom is, however, 38 instead of 19. The probability of obtaining a critical ratio of 3.0 or more and in the same direction is therefore less under the null hypothesis than the probability given in the preceding paragraph. Also, a critical ratio of 2.4 might be considered as significant under Fisher's criterion of 0.01 probability.

Since we are testing certain very general statements which seem to imply a high degree of consistency of results, we are justified in making stringent requirements of the reliability of our differences. At the same time, however, we are interested in the facts of the matter as well as in Goldstein's statement of conclusions. We should therefore take note of those critical ratios which fall in the range from 2.4 or 2.5 to 3.0. Fortunately, most of our data show critical ratios either above three or below even the most lenient standards of significance, so we shall not be very much troubled by this problem.

Effect of eye-movement with the eyes closed. As shown in Table I, there are only 3 O s who show significant changes in locating the apparent front position corresponding to changes in position of the eyes. It is to be noted that two of these were the two O s who served first and who completed all of the observations of each section of the experiment at a single sitting. The differences for these two O s might therefore be interpreted as due to wide variations in the auditory frame of reference from day to day, rather than as the effects of eye movement. Under this hypothesis, however, it is difficult to account for certain consistencies in the results. In both cases, the differences resulting from movement of the eyes to the right are so much greater than those resulting from a left rotation, and the same pattern carries through the second experiment as well. At any rate, if a given O 's frame of reference varied at random from day to day, we should expect the differences between observations taken on separate days to fluctuate in a random manner about zero.

There is another hypothesis to be considered. Perhaps the repeated movement of the eyes to the same side during an observation period had a cumulative effect, resulting in a progressive shift in the frame of reference. In fact, one O (Ca) reported

⁹ C. C. Peters and W. R. Van Voorhis, *Statistical Procedures and Their Mathematical Bases*, 1940, Table XLV, 488-491.

¹⁰ R. A. Fisher, *op. cit.*, 128.

occasional dizziness during this series and it was necessary to give frequent rest periods. This hypothesis is, however, also untenable. The results of *Ca* and *Ja* in *L*₁ and *R*₁ were examined for evidence of such trends. The results of the first half of the observation period do not differ significantly from the results of the second half. The largest critical ratio was 1.07.

For these *O*s, therefore, there seems to be only one conclusion—that the eye positions do affect auditory localization in a regular manner.

For the lack of significant effects among the other *O*s we may suggest two possible interpretations. One possibility is, of course, that eye position has no effect for these *O*s. The other is that they may have varied so much from day to day in the normal position of the median plane that these variations covered up any variations due to

TABLE I
CHANGE IN AVERAGE POSITION OF AUDITORY FRONT FOR VARIOUS POSITIONS OF EYES
(Eyes closed)

O	L-R		L-S		S-R	
	Diff.	CR	Diff.	CR	Diff.	CR
<i>Ca</i>	-23.50°	10.68	-3.68°	3.25	-19.83°	10.66
<i>Ja</i>	-19.00°	18.81	-1.94°	2.13	-17.06°	19.04
<i>Be</i>	-6.25°	3.73	-2.75°	1.91	-3.50°	2.02
<i>Br</i>	-1.63°	1.38	-1.75°	1.46	0.13°	0.09
<i>Lo</i>	0.75°	0.44	1.38°	0.77	-0.62°	0.39
<i>Ma</i>	-1.25°	0.88	1.50°	0.99	-2.75°	1.76
<i>Ne</i>	0.13°	0.07	2.50°	1.22	-2.38°	1.47
<i>Se</i>	2.88°	1.39	3.25°	1.53	-0.38°	0.22

eye position. This does not seem likely because the standard deviations of their differences do not exceed those of *Ca* whose averages were based upon more homogeneous data from the point of view of daily variation. That is, *Ca* shows greater variability even though each section was completed in a single sitting.

Nevertheless, we have examined this possibility statistically since the above comparison with *Ca* is scarcely conclusive. For several *O*s whose variation from day to day was large, we have recomputed the reliability of the differences by a method which eliminates the factor of day-to-day variation. In this method of computation the point of change for the first observation of the day with the eyes turned to the right is subtracted from the value of the first observation of that same day in which the eyes had been turned left. The same procedure is carried through pair by pair for the whole of Section I. Then the differences are averaged and the standard deviation of this distribution is determined.

Daily variations are eliminated by this procedure, since this factor is a constant for each member of any pair used in determining the differences. Another factor which might increase variability is also eliminated in this treatment. This is the factor of trend introduced by the method of limits. Since each ascending series is paired with another ascending series, and each descending series with another descending, the factor of the direction of the series will also be a constant.

In spite of the fact that these two factors were held constant, this second method of computing reliability did not change appreciably the critical ratios obtained for

the *Os* who appeared from casual observation to have marked daily variations. *Ma* and *Se* have, by this method, critical ratios of 0.85 and 1.64, respectively, for the difference L-R. By comparing these values with those of Table I (0.88 and 1.39 respectively) it is evident that daily fluctuations in frame of reference, and the factor of series direction do not contribute appreciably to the unreliability of the results.

Effect of eye-movement with the eyes open. The results obtained with the eyes open and *O* fixating a visible point are shown in Table II. The situation here is much the same as that presented in Table I. When the sounds are presented in the

TABLE II
CHANGE IN AVERAGE POSITION OF AUDITORY MEDIAN FOR VARIOUS POSITIONS OF EYES
(Eyes open)

Series	O	L-R		L-S		S-R	
		Diff.	CR	Diff.	CR	Diff.	CR
(A) Stimulation in front	<i>Ca</i>	-8.88°	3.35	-3.63°	1.65	-5.25°	2.45
	<i>Ja</i>	8.25°	6.20	3.75°	2.47	4.50°	3.60
	<i>Be</i>	-2.75°	1.63	-3.88°	2.42	1.13°	0.79
	<i>Br</i>	-2.38°	1.65	0.00°	0.00	-2.38°	1.93
	<i>Lo</i>	-5.63°	3.29	-3.25°	2.21	-2.38°	1.48
	<i>Ma</i>	-4.38°	2.82	-4.50°	3.08	0.12°	0.10
	<i>Ne</i>	-1.00°	0.45	-1.50°	0.65	0.50°	0.29
	<i>Se</i>	1.38°	0.85	-0.25°	0.17	1.62°	1.13
(B) Stimulation in back	<i>Ca</i>	-0.88°	0.42	1.50°	1.49	-2.38°	1.21
	<i>Ja</i>	1.00°	0.56	4.38°	2.19	-3.38°	1.67
	<i>Be</i>	8.00°	4.76	3.38°	1.97	4.63°	2.66
	<i>Br</i>	5.13°	2.86	5.88°	3.19	-0.75°	0.39
	<i>Lo</i>	5.88°	4.82	2.88°	2.30	3.00°	2.50
	<i>Ma</i>	5.63°	2.38	4.13°	2.06	1.50°	0.63
	<i>Ne</i>	4.13°	2.75	5.13°	2.96	-1.00°	0.59
	<i>Se</i>	4.00°	2.05	0.75°	0.52	3.25°	1.53

front the first two *Os* (*Ca* and *Ja*) again show reliable differences, but they disagree in the direction of the effect. For *Ca* ocular position with the eyes open has the same effect as it does with the eyes closed. For *Ja* we have the reversal of effect which we should expect from Goldstein's statements. Only two other significant differences appear in this part of the experiment. *Lo* shows a consistently negative shift and a reliable difference between the values for the extreme positions of the eyes (L-R). *Ma* was significantly affected by the shift of the eyes to the left but not by the movement to the right. Neither *Lo* nor *Ma* had any reliable effects in Section I, and the changes which appear in F_{II} are in opposition to the findings of Goldstein.

When the sounds come from the rear half of the field (Table II) the situation is again changed. The first two *Os* no longer show significant effects. *Br* has the only significant effect which appears for him in any part of the experiment, and *Be*, *Lo*, and possibly *Ne* were also significantly affected.

In recording the differences for the rear half of the field, the signs have a slightly different significance from that in other parts of the tables. Here a plus sign means that as the eyes move to the right, the apparent back position moves to the left. In other words, the apparent front-back axis as a whole is rotated in the same direction

as the eyes have moved, if we think of this front-back axis as an imaginary rigid line.

A comparison of the two parts of Table II, shows that in many cases the assumption that the front-back axis is shifted as a whole is not justified. If this axis did shift as a whole, and if the differences were reliable, the sign of each difference in the A part of the table should agree with the sign in the B part. *Be*, however, shows a reliable shift in the positive direction when the sounds come from the back, and an unreliable negative shift when the sounds come from the front. *Lo* shows reliable shifts in both cases, but the shift is opposite in sign in the two halves of the experiment. *Ma*, whose differences almost meet the criterion of reliability, also shows this reversal.

Actually, the axis is not always straight, even with the eyes in the normal position. Out of 8 *Os*, three show statistically reliable distortions of the axis of 6° to 8° , one has an unreliable distortion of 3.75° , and the remaining *Os* had distortions of less than 1° . The reliable distortions are found in the records of *Lo*, *Ma*, and *Ne*. They may possibly be attributed to differences in acuity of the two ears, but this was not measured.

Nevertheless, the effect of eye rotation is to distort further the front-back axis. The differences $L - S$ and $S - R$ in parts A and B of Table II show this. If the axis is shifted as a whole and not further distorted, the A and B values should correspond in sign and in size within the limits of error. If they do not, one half of the axis is moved more by the eye-movements than the other half, and further distortion is evident. This additional distortion is best illustrated by the record of *Lo*.

Open vs. closed eyes. There are two reasons for comparing the results of experiments in which the only difference in the conditions is that the eyes are open for one set of results and closed for the other. The results obtained with *Ca* and *Ja* indicated that the mere opening of the eyes might affect the position of the apparent frontal position of the sound. The second reason for making such a comparison is that Goldstein describes opposite effects of eye movement depending upon whether the eyes are open or closed. We have not found this opposition of effect in all *Os* and where we have found it it is not always reliable. It is possible, however, that these effects might show up more clearly if we compare across from Section I to Section II.

According to Goldstein, rotation of the eyes to the left tends to move the apparent front to the right when the eyes are closed and to the left when the eyes are open. Thus a comparison of the averages of L_I and FL_{II} should show reliable differences because the same left rotation of the eyes should determine opposite shifts in the auditory median. The same argument holds, of course, for R_I and FR_{II} .

The comparisons discussed here are presented in Table III. With the eyes in the normal position (S_I and FS_{II}) our first two *Os* are again the only ones showing reliable effects. Again, the factor of variation from day to day might be considered as the responsible factor in these differences. We have, however, already considered this problem above (p. 247), and the same arguments against it hold here. In fact, we have here merely further evidence of the consistency of the results of these *Os*, a consistency which argues against the factor of daily variation.

The 'Eyes-right' and 'Eyes-left' sections of Table III should be useful in checking Goldstein's conclusions further. We find, however, that these columns are almost impossible to interpret, even where the differences are significant statistically. Consider, for example, the results of *Ca*. The *O* fails to show the reversal of effect be-

tween Section I and Section II called for by Goldstein's statements. This can be determined from Tables I and II. In spite of this fact, however, *Ca* exhibits reliable differences when we compare averages which differ only with respect to the opening or closing of the eyes. We can only conclude that opening and closing the eyes does not reverse the effect for *Ca*, but merely changes the *amount* of the effect.

Lo and *Ma* are the only members of the second group of *Os* who show reliable differences in the section of Table III under consideration. Tables I and II show that both of these *Os* had reliable effects of moving the eyes when the eyes were

TABLE III
CHANGES IN AVERAGE POSITION OF AUDITORY MEDIAN PLANE THROUGH OPENING OF EYES

O	Eyes straight		Eyes left		Eyes right	
	Diff. (S _I -FS _{II})	CR	Diff. (L _I -FL _{II})	CR	Diff. (R _I -FR _{II})	CR
<i>Ca</i>	7.43	6.04	7.38	3.45	22.00	8.63
<i>Ja</i>	-6.19	5.29	-11.88	8.93	15.38	15.38
<i>Be</i>	-1.75	1.24	-0.63	0.39	2.88	1.65
<i>Br</i>	2.50	2.05	0.75	0.64	0.00	0.00
<i>Lo</i>	0.63	0.41	5.25	3.05	-1.13	0.67
<i>Ma</i>	0.25	0.18	6.25	4.01	3.13	2.22
<i>Ne</i>	-4.88	2.64	-0.88	0.36	-2.00	1.38
<i>Se</i>	-4.50	2.92	-1.00	0.49	-2.50	1.51

open, but no significant changes when the eyes are closed. The results in Table III merely furnish additional confirmation of this fact.

The analysis presented in Table III does not, therefore, furnish any further support of Goldstein's conclusions than that already contained in the previous analyses.

SUMMARY AND CONCLUSIONS

(1) The effect of eye position upon auditory localization is exceedingly irregular from *O* to *O* and even at different times for the same *O*. A reliable effect is the exception rather than the rule. Any single *O* may exhibit reliable effects under one direction of eye movement and not under another.

(2) The *same* direction of shift of the apparent auditory median position may result from *opposite* movements of the eyes. In each part of the experiment almost exactly half of the *Os* exhibit this effect, although not all of the shifts are reliable.

(3) There is no rigid front-back axis which is shifted as a whole by movements of the eyes. A clockwise movement of the eyes may result in a clockwise movement of the apparent front position and in a counter-clockwise movement of the apparent back position, or in any other combination of directions.

(4) The mere opening or closing of the eyelids affects the apparent position of the auditory front. The two *Os* who worked continuously on each part of the experiment show the most reliable differences in this respect as they do in all other comparisons reported. Two other *Os*, however, show critical ratios of more than 2.5.

(5) Why the two *Os* who followed a different order of experimentation from that of the others should be the most consistent in showing effects of eye rotation is undetermined. Daily variations in the frame of reference could not account for the consistency of the results. A cumulative effect of repeated movement of the eyes

in the same direction could not be demonstrated. The fact that these *O*s were more highly trained than the others might be a factor, but just how is difficult to see. They were no more consistent in their results than other *O*s who showed no reliable effects.

The results of this experiment scarcely support the findings of Goldstein upon the effects of eye movement upon auditory localization. Certainly his implication that such effects are frequent and uniform is not justified by our findings. We have found occasional significant differences scattered through our results, but eye movements seem to be as likely to result in a shift in one direction as in another. An eye movement to one side may result in a significant shift while a movement to the other side may not. Some *O*s who show an effect when the eyes are closed do not show any significant shifts in localization while the eyes are open. Others exhibit the reverse picture.

The two *O*s, who show the most consistently reliable changes in localization, differ in the direction of these changes. Both give results in accordance with Goldstein's rule while the eyes are closed, but only one of them agrees with his rule for the open eye.

In emphasizing the lack of uniformity of the results, however, we must not go too far. Some of the effects for some *O*s are reliable. Of course, we might possibly obtain an occasional critical ratio greater than three by chance alone and with the whole collection of data homogeneous except for chance factors. The significant critical ratios in this study are, however, more than 'occasional.' Considering the comparisons involving the extreme positions of the eyes, one difference in three is significant (8 out of the 24 comparisons determined). When the series with deviations of the eyes are compared with the normal series, one difference in seven is reliable.

The opposition of effect from *O* to *O*, and other irregularities, however, show that the interrelations of factors are much more complex than Goldstein would lead us to expect. The opening and closing of the lids, for example, has an effect for some *O*s quite apart from the rotation of the eyes. Perhaps there are still other factors related in such complex ways to the problem.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY
OF THE UNIVERSITY OF NEBRASKA

XI. RATINGS OF VIVIDNESS OF IMAGERY IN THE WAKING STATE
COMPARED WITH REPORTS OF SOMNAMBULISM

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The term somnambulist is usually applied not only to persons who walk in sleep but also to those who perform in sleep any acts which require extensive muscular coordination.¹ Reichenbach, on the basis of numerous personal observations, has insisted that all sleep-walkers talk, and that speaking in sleep is the first symptom of somnambulism.² As little factual material is available on this subject, we analyzed

TABLE I
DISTRIBUTION OF REPLIES TO THE QUESTIONS

Combination of replies		Number	%
Walk (No)	Talk (No)	1069	59.1
Walk (No)	Talk (Yes)	400	22.1
Walk (No)	Talk (?)	194	10.7
Walk (Yes)	Talk (No)	7	0.4
Walk (Yes)	Talk (Yes)	88	4.8
Walk (Yes)	Talk (?)	3	0.2
Walk (?)	Talk (No)	3	0.2
Walk (?)	Talk (Yes)	9	0.5
Walk (?)	Talk (?)	21	1.2
Not answered		14	0.8
Total		1808	100.0

the replies of 1,808 freshmen on two questions in the Thurstone Personality Schedule—"Do you ever walk in your sleep?" "Do you ever talk in your sleep?"—in the hope of establishing the relationship between walking and talking in sleep.

The questions were answered by encircling the "Yes," "No," or "?," that appeared opposite them. The "Yes" answers were probably correct, but the "No" and "?" answers may have been returned by a number of students who were sensitive about their behavior during sleep and who wished, therefore, to conceal the true answers. What percentage of the answers were thus falsified, we have no way of knowing.

As Table I indicates, nearly 60% of these freshmen report that they neither walk

* Accepted for publication September 15, 1940.

¹ Havelock Ellis, *The World of Dreams*, 1911, 95; H. C. Warren, *Dictionary of Psychology*, 1934, 255.

² C. von Reichenbach, *Somnambulism and Cramp*, Tr. by J. S. Hittell, 1860, 7.

³ L. L. Thurstone and T. G. Thurstone, *Personality Inventory*, 1929. This test was given the freshmen in the College of Arts and Sciences as part of the personnel testing-program. The replies considered in this study were obtained during the academic years 1931-1934.

nor talk in sleep. In all, approximately 92% say that they do not walk in their sleep, while an additional 2% seem to be in doubt regarding sleep-walking. More than 27% state definitely that they talk in sleep, but the majority of these claim not to walk during sleep. More than 5% admit that they walk during sleep and almost all of these report talking as well. Thus sleep-walkers who do not talk during sleep appear to be rare, while not more than one-third of the non-walkers talk. If one may trust the reports, these data seem to support the hypothesis that sleep-walking and sleep-talking are but special cases of a general tendency toward overt motor activity during sleep, (somnambulism), walking being the more advanced degree of this tendency.

Several somnambulists have indicated to us that kinesthetic imagery plays a prominent part in their dreams. When they have actually walked in sleep, however, they do not remember any dream imagery connected with the walking, even though upon being awakened from the somnambulism they may have a logical explanation (rationalization) for their acts. Ellis,⁴ Reichenbach,⁵ Sadger,⁶ and others⁷ have insisted that somnambulists, upon waking, do not remember any imagery from their periods of sleep-walking. This seems reasonable in view of the fact that acts which have become well habituated may not be accompanied by imagery, even in the waking state.⁸ Washburn states that "it is universally admitted that kinesthetic images have a marked tendency to result in actual movement and thus to transform themselves into peripherally excited sensations."⁹ Even Tolman has suggested, somewhat reluctantly, that images affect behavior.¹⁰ He cites the work of Davis, who seems to have presented evidence of the functional relationship between imagery types and performance in various tests.¹¹

These and other considerations have led us to the following hypothesis: The somnambulist is an individual whose kinesthetic imagery is strong. When he is partially relaxed in sleep, the "ratio between excitation and inhibition"¹² is such that his imagery is vivid, but overt movements do not occur. Should his muscles become tense, constituting readiness for action, overt movement may occur readily, sometimes amounting to walking or carrying out other complicated acts requiring a high degree of muscular coordination. (The latter, we suggest, are instances of what Washburn designates as "full motor response without delay and without consciousness" and being "without consciousness" are not remembered upon awakening.)¹³ Imagery of modalities other than kinesthetic probably does not lead so readily to overt movement as does the kinesthetic. Visual imagery, auditory imagery, and

⁴ *Op. cit.*, 95 f.

⁵ *Op. cit.*, 226.

⁶ J. Sadger, *Sleep Walking and Moon Walking*, 1920, vii.

⁷ Cf. H. C. Warren, *Dictionary of Psychology*, 1934, 255.

⁸ M. R. Fernald, The diagnosis of mental imagery, *Psychol. Monog.*, 14, 1912, (no. 58), 137.

⁹ M. F. Washburn, *Movement and Mental Imagery*, 1916, 50.

¹⁰ E. C. Tolman, *Purposive Behavior in Animals and Men*, 1932, 254.

¹¹ F. C. Davis, The functional significance of imagery differences, *J. Exper. Psychol.*, 15, 1932, 630-661.

¹² Washburn, *op. cit.*, 36.

¹³ *Loc. cit.*

the less common types of imagery, when they occur in dreams, are less likely to lead to overt action.¹⁴

If the foregoing hypothesis is valid and if we may grant that the waking imagery of an individual corresponds to his dream imagery, so far as modality and relative intensities are concerned,¹⁵ then laboratory studies of intensity of imagery should show the kinesthetic imagery of somnambulists to be more intense (vivid, clear) than that of non-somnambulists. In order to test this hypothesis we examined the data given by 138 students on "Experiment 3: Introspective Method: A Study in Images" in Guilford's laboratory manual.¹⁶ In this exercise the students, after preliminary instruction and trial rating of the vividness of their imagery, rate the vividness of their imagery in response to each of 50 words read aloud by the instructor. The manual provides a table on which each modality of imagery for each word may be rated for intensity (vividness, clearness) on a scale from 0 to 6, inclusive. Of the 138 records upon which this part of the study was based, approximately 100 were drawn from the elementary laboratory course; the remaining were obtained from members of a class in abnormal psychology and from several persons who were known to walk in their sleep. Upon the completion of the exercise, it was explained that a study of imagery and dreams was being made and that in order to complete the study it would be necessary to know which persons walked or talked in their sleep. The Os were asked to cooperate by writing their names and answers to the questions "Do you ever walk in your sleep?" and "Do you ever talk in your sleep?" on separate sheets of paper.¹⁷ They were informed that this information would be kept strictly confidential.

Of the 138 Os, 80 reported that they neither walked nor talked in sleep. For convenience we classed with these, 12 individuals on whom no information regarding somnambulism was available, making in all a control group of 92. Thirty-four Os stated that they talked in their sleep, but did not walk. The remaining Os (12 in number) said that they walked in sleep, but only 11 of these reported talking. For convenience in reporting the data, we have treated these 12 Os together. It would have been desirable to have had more somnambulists for study, but we believe the results from these 12 are sufficient to indicate a trend which would be likely to occur in a larger group.

The distribution of the mean ratings of intensity of the various modalities of imagery for those who report they neither walk nor talk in sleep, plus the 12 on

¹⁴ Ellis, (*op. cit.*, 102) refers to Aliotta (*Il Pensiero e la Personalita nei Sogni*) as believing "that dreamers differ according to their nervous type, the person of visual type assisting passively at the spectacle of his dreams, while the person of motor type takes actual part in them."

¹⁵ Thompson found that in 5 Os, dreams tended to reproduce the relative proportions of the imagery of waking life. (E. R. Thompson, An inquiry into some questions connected with imagery in dreams, *Brit. J. Psychol.*, 7, 1914, 301-318.) Cf. E. S. Conklin, *Principles of Abnormal Psychology*, 1935, 375.

¹⁶ J. P. Guilford, *Laboratory Studies in Psychology*, 1934, 21-23.

¹⁷ We do not believe these reports to be entirely truthful, but we have no other criterion by which to classify the Os. We are reasonably certain that all who reported walking in sleep have actually done so, though none of them is a pronounced somnambulist. Several of the latter reported that they had not walked within the past year.

whom no information is available, is shown in Table II. The average of the ratings made by an individual in a given modality is treated as his *score* for that modality. The means presented in summary of this and succeeding tables are therefore the averages of the *scores* for the individuals, not *weighted averages*.

TABLE II
DISTRIBUTIONS OF MEAN RATINGS OF INTENSITY OF VARIOUS MODALITIES OF IMAGERY FOR
PERSONS WHO NEITHER WALK NOR TALK IN THEIR SLEEP
(92 cases)

Intensity	Modality							
	Vis.	Aud.	Kin.	Tac.	Ol.	Gus.	Ther.	Pain
0.0-0.9	1	2	2	1	7	8	3	6
1.0-1.9	3	6	8	12	15	9	13	13
2.0-2.9	28	27	34	24	21	13	20	22
3.0-3.9	40	45	35	39	25	27	34	36
4.0-4.9	19	8	9	11	7	13	12	12
5.0-5.9	1	3	3	2	8	4	2	3
6.0	0	1	0	2	0	1	2	0
Mean	3.33	3.20	3.05	3.17	2.91	3.09	3.12	3.01
Median	3.35	3.24	3.04	3.22	2.93	3.28	3.21	3.14
SD _{vis.}	.87	.99	.98	1.09	1.36	1.40	1.20	1.17
SD _{m.}	.09	.10	.10	.11	.15	.16	.13	.12

The relative ranks in intensity of the various types of imagery are the same as those usually found in such a group.¹⁸ Since the *Os* were relatively naïve, these ratings are probably subject to numerous forms of error. Coefficients of correlation between scores in the various pairs of modalities are all positive, ranging from +0.26 (visual-gustatory) to +0.76 (olfactory-pain) and averaging +0.51. As these positive correlations suggest, many of the *Os* had a tendency to rate all modalities of imagery higher or lower than others rated them. This might indicate that some *Os* have stronger imagery than others in *all* modalities, but it may also be regarded as evidence of individual constant errors in rating. Unfortunately, distributions of average ratings (scores), such as those in Table II and succeeding tables, provide no clue to the prevalence of the various 'imagery types' among the individual *Os*. If the ratings are to be taken at face value, visual imagery predominated in many of the *Os*, though there seemed to be clear cases of the predominance of auditory, kinesthetic, and tactile imagery, respectively. No instance occurred in which gustatory or olfactory imagery predominated. The greater number of 'visualizers' is in part responsible for the higher average ratings of intensity of visual imagery shown in this and succeeding tables. The differences between the average ratings of the various modalities for the whole group show tendencies toward statistical significance in some instances, but not in others. Critical ratios of the difference between these averages for the group (Table II) ranged from 0.18 (gustatory-thermal) to 2.85 (visual-olfactory) and averaged 1.23.¹⁹

¹⁸ Cf. C. H. Griffitts, Individual differences in imagery, *Psychol. Monog.*, 37, 1927, (no. 172), 1-91. Except for pain, the order is the same as that reported by Guilford (*op. cit.*) for 52 *Os*, but Guilford's figures are considerably higher than ours.

¹⁹ All critical ratios presented are $\text{diff.} / \sigma \text{ diff.}$

Table III contains the data for the 34 *O*s who stated that they talked but did not walk during sleep; Table IV, for the 12 *O*s who admitted walking, 11 of whom also admitted talking. The scores of the latter were consistently lower than those of

TABLE III

DISTRIBUTIONS OF MEAN RATINGS OF INTENSITY OF VARIOUS MODALITIES OF IMAGERY FOR PERSONS WHO TALK BUT DO NOT WALK IN THEIR SLEEP
(34 cases)

Intensity	Modality							
	Vis.	Aud.	Kin.	Tac.	Ol.	Gus.	Ther.	Pain
0.0-0.9	0	0	1	1	3	0	1	1
1.0-1.9	0	2	2	2	4	4	3	3
2.0-2.9	6	6	8	2	3	5	5	9
3.0-3.9	17	18	18	22	6	10	11	11
4.0-4.9	11	8	4	5	8	10	5	6
5.0-5.9	0	0	0	0	2	0	1	4
6.0	0	0	0	1	1	0	0	0
Mean	3.65	3.44	3.17	3.45	3.31	3.40	3.23	3.38
Median	3.65	3.50	3.31	3.52	3.58	3.55	3.36	3.36
SD _{dis.}	.69	.80	.88	1.00	1.61	1.03	1.13	1.23
SD _{m.}	.12	.14	.15	.17	.31	.19	.22	.21

all the other *O*s, except in the gustatory category, where three persons had slightly higher average ratings. The *qs* for the various pairs of modalities for those who talk but do not walk are not as high as the *rs* for the group previously cited. They range from 0 (visual-gustatory) to +0.62, averaging +0.34. The critical ratios of dif-

TABLE IV

DISTRIBUTIONS OF MEAN RATINGS OF INTENSITY OF VARIOUS MODALITIES OF IMAGERY FOR PERSONS WHO WALK AND TALK IN THEIR SLEEP
(12 cases)

Intensity	Modality							
	Vis.	Aud.	Kin.	Tac.	Ol.	Gus.	Ther.	Pain
0.0-0.9	0	0	0	0	0	0	0	0
1.0-1.9	0	0	0	0	3	1	3	4
2.0-2.9	2	2	3	4	0	4	1	1
3.0-3.9	3	5	5	5	5	1	3	2
4.0-4.9	4	4	2	2	3	4	2	4
5.0-5.9	3	1	1	1	1	0	3	1
6.0	0	0	1	0	0	1	0	0
Mean	4.17	3.83	3.83	3.50	3.42	3.59	3.58	3.25
Median	4.25	3.80	3.60	3.40	3.60	3.50	3.67	3.50
SD _{dis.}	1.03	.85	1.18	.91	1.26	1.38	1.50	1.42
SD _{m.}	.30	.25	.34	.26	.36	.42	.43	.41

ference between the average scores of the various pairs of modalities for this group, range from 0.06 (auditory-tactile) to 3.75 (visual-kinesthetic), averaging 0.88. The *qs* for the smaller group of those who both walk and talk are generally higher, ranging from 0.12 (tactile-gustatory) to 0.85 (auditory-olfactory, kinesthetic-pain, tactile-thermal) and averaging 0.61. Critical ratios of the differences between the

average scores of the various pairs of modalities for this group range from 0 (auditory-kinesthetic) to 2.69 (kinesthetic-pain), averaging 1.19.

A glance at the tables shows that although the distributions are not radically different, all the average ratings in Table III are higher than those in Table II, and those in Table IV, with the exception of the average for pain, are higher than those in Table III. This would seem to indicate that the imagery of somnambulists

TABLE V

DIFFERENCES AND CRITICAL RATIOS OF DIFFERENCES BETWEEN AVERAGE RATINGS OF VIVIDNESS⁸ OF IMAGERY: CONTROL GROUP COMPARED WITH THOSE WHO WALK BUT DO NOT TALK; AND CONTROL GROUP COMPARED WITH THOSE WHO WALK AND TALK IN THEIR SLEEP

Modality	Talk but not walk		Talk and walk	
	Diff.	CR.	Diff.	CR.
Vis.	.32	2.17	.84	2.71
Aud.	.24	1.42	.64	2.40
Kin.	.12	.64	.78	2.21
Tac.	.28	1.35	.33	1.15
Ol.	.40	1.16	.51	1.29
Gus.	.31	1.23	.50	1.12
Ther.	.11	.42	.46	1.02
Pain	.37	1.52	.24	.56

TABLE VI

CHI SQUARES AND CONTINGENCY COEFFICIENTS OBTAINED IN COMPARING CONTROL OS, SLEEP-TALKERS, AND SLEEP-WALKERS AND TALKERS, WITH RESPECT TO THEIR RATINGS OF VIVIDNESS OF VARIOUS MODALITIES OF IMAGERY

Modality	χ^2	P*	C	mC†	P.E. _o
Vis.	29.32	.01	.42	.41	.11
Aud.	13.49	.46	.30	.13	.05
Kin.	17.62	.17	.34	.25	.12
Tac.	15.03	.25	.31	.18	.04
Ol.	18.40	.10	.36	.27	.06
Gus.	14.81	.24	.34	.19	.05
Ther.	15.77	.20	.34	.21	.09
Pain	13.39	.46	.30	.12	.06

* Probability that the obtained χ^2 or a greater value could have arisen by chance.

† Contingency coefficient corrected by Yule's method, and comparable to r .

tended to be more vivid than that of non-somnambulists. When the differences are examined closely (Table V), it is seen that they are all small. However, the critical ratios of the differences between the averages of those who neither walk nor talk and of those who walk and talk are greater than 2.20 in the case of the three modalities usually rated most reliably, viz., visual, auditory and kinesthetic.²⁰

A further attempt to determine the statistical significance of these data involved the construction of a 3 x 7 contingency table for each modality of imagery. The three categories of somnambulism previously adopted were used, with seven class intervals of ratings of vividness. The principal results of this procedure are shown in Table VI. While the Chi squares and contingency coefficients are not high,

²⁰ Practically 99 chances in 100 that the true difference is greater than zero.

especially when the probable errors of the latter are taken into account, the fact that all the coefficients are positive is significant. Considering that there were only 12 Os in the category of those who walked during their sleep, the findings in the case of kinesthetic imagery (about one chance in six that the obtained distribution might have arisen from the theoretical series) are sufficiently remarkable to support our hypothesis. The results are more striking in the case of visual imagery, a finding which was not anticipated in our original hypothesis.

It should be emphasized that the figures presented here refer to *ratings of vividness* of imagery; no mention has been made of the relative frequency of the various modalities. As Griffitts has pointed out, the various criteria of imagery types do not yield the same results; an individual classified in one type on the basis of the frequency or dominance of his images may fall into a distinctly different category when judged by the vividness or intensity of imagery.²¹ Not only was this the case among our Os, but the correlations between *number* of images and *average rating of vividness* within each modality were largely *negative*. The range of the 24 coefficients (each of the three groups of Os taken separately for each modality) was from -0.64 to $+0.15$, only four of them being positive. The highest positive correlation and one of $+0.06$, represented the data for visual imagery from those who neither walked nor talked and those who talked but did not walk, respectively. The more significant negative correlations were found in the tactile, olfactory, gustatory and thermal categories.

While these data hardly support the hypothesis that somnambulists are persons whose imagery is *predominantly kinesthetic*, they suggest that somnambulists tend in general to have stronger imagery than non-somnambulists. To test the hypothesis further, it would be necessary to have trained introspectors, more accurate knowledge of the Os and a larger number of somnambulists. Our chief justification for publishing this study lies in the hope that it will bring our hypothesis to the attention of those who have greater opportunity to observe somnambulists than we have.

SUMMARY AND CONCLUSIONS

(1) Data from 1808 persons on Thurstone's Personality Schedule indicate that about nine-tenths of the approximately 5% who admit that they walk in sleep, also state that they talk. About one-fourth of the 92% who did not admit walking in sleep stated that they talked.

(2) The hypothesis was proposed that somnambulists are likely to be persons whose imagery is predominantly kinesthetic. Preliminary data indicate that both sleep-talkers and sleep-walkers tend to estimate their waking imagery as more vivid than that of non-somnambulists in all modalities, the differences being most significant in the visual, auditory and kinesthetic categories.

(3) These conclusions hold for ratings of vividness of imagery, but not for frequency of imagery, as most of the correlations between *numbers* of images reported and average ratings of vividness were negative.

²¹ Griffitts, *op. cit.*, 73-74.

APPARATUS

THE CONTINUOUS MEASUREMENT OF STRENGTH OF PULL BY RATS

By CLAUDE E. BUXTON, Northwestern University

In the investigation of motivation in rats, various indexes of the strength of motives have been utilized. Among these are the frequency with which the rat crosses a charged grid, the speed of digging through obstructing shavings or sand, the rate of bar-pressing, the rate of string-pulling, and the speed of running. One obvious index, which has not yet been explored thoroughly, is the effort exerted by a rat in pulling against a restraining force in order to reach its goal. An apparatus designed for the measurement of this effort is described here.¹

The influence of a large number of variables on the strength of motivation (using strength of pull as the indicator) may be studied with the aid of this device. Examples are: (1) kind and strength of need; (2) desirability of goal object; (3) distance to the goal; (4) effects of past experience in the situation, in conjunction with the more momentary influences; (5) speed with which the animal is permitted to progress; (6) amount of resistance to be overcome before locomotion is possible; and (7) the structure of the pathway. A simplified sketch of the instrument is presented in Fig. 1.

Runway. The alley traversed by the rate may be of various types; Fig 1 shows merely the kind we have used. It is constructed of two 6-ft. sections (S_1 and S_2) with 3-in. sides and 4-in. floor, painted white inside and covered with a wire screen. To give the rats good footing, $\frac{1}{4}$ -in. wire mesh was stapled to the floor. This was done before the sides of the run-way were attached, hence the floor was uniformly and smoothly covered. The starting-box is 1 ft. long and is separated from the rest of the run-way by a guillotine-gate, string-operated by E . The food-box (F) is 18 in. long and is entered through a light 'push-up' door.

Polygraph. The polygraph is shown at the left in Fig. 1. It was built to carry wide paper. The speed at which the paper passed was varied by means of gear shifts. About 2 in. above the paper there is a horizontal iron bracket on which is mounted the writing-arm (W_1). This arm moves on a hub (H) to which there is attached beneath an horizontal bakelite disk. A sector (C_2) of this disk is covered with thin sheet brass. Over the bakelite disk and this brass sector moves a spring tongue (C_1) that is attached to the long side of the writing-arm. This tongue (slider) makes contact with the brass sector (slider contact) and closes the circuit to the motor (M) when the writing-arm is moved. A pulley block (P_2) is attached to the end of the long side of the writing-arm. A long, light coil-spring is fastened to the pulley and through the pulley is run the fish-line leash (L) that is attached to the rat.

Drum and accessories. The drum, to which the other end of the fish-line leash

¹ This apparatus was built at Swarthmore College.

(L) is attached, is driven by the constant-speed motor (M) by means of a matched pair of speed-reducer pulleys (P₁ and P₂). At the free end of the drum there is a metal pin (shorted through the drum-mounting) which closes the battery-circuit to the revolution-marked (W₂) on the polygraph whenever it touches the spring-brass contact (C₆). To permit rewinding of the fish-line leash after a run, the drum is mounted on a hinged base which may be tilted back toward the motor pulleys,

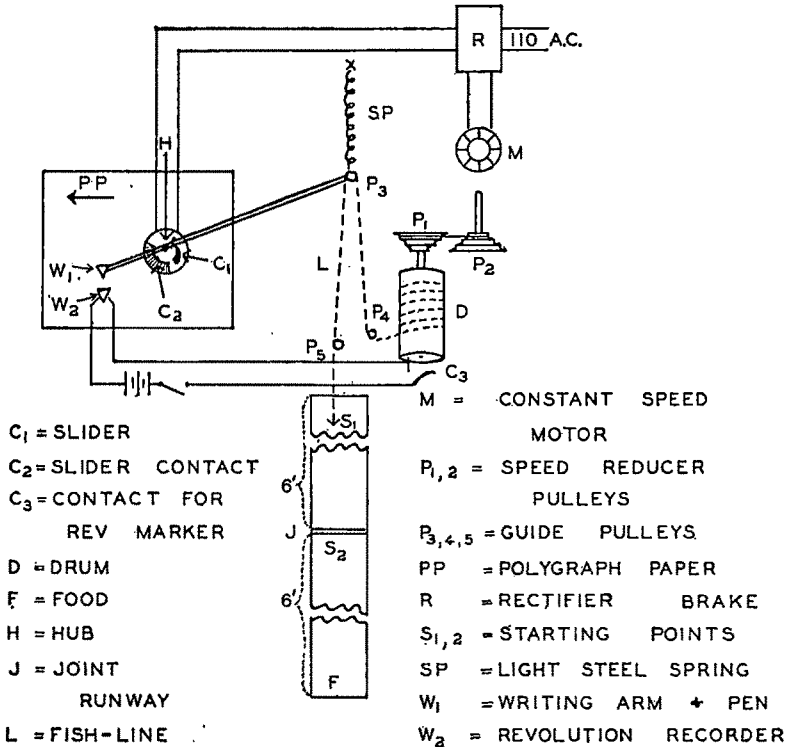


FIG. 1. SIMPLIFIED SKETCH OF THE APPARATUS

thus loosening the drive belt. The rectifier (R) is so constructed that a direct current passes through the motor when the tongue-slider (C_1) on the writing-arm and the slider-contact (C_2) on the bakelite disk do not touch. This acts as a brake on the motor and prevents any coasting of the drum.

Writing-arm. The writing-arm, a 1/4-in. aluminum rod (see Fig. 2) is made in two sections both threaded into the center hub (H) which turns on the shaft (SH). The short section of this arm, 4 in. in length, carries the recording-pen (W_1); the long section, 16 in. in length, carries the slider-tongue (C_1) and the pulley block (P_1). The slider-tongue, made of spring-brass, is fastened to the long lever-arm by means of the block (BL). It is shaped so that it touches the bakelite plate (PL)

and makes contact with C_2 when the writing arm is turned. Small rubber-covered wires led from C_1 and C_2 to the motor and complete the circuit whenever those poles are brought into contact.

Revolution-marker. Every revolution of the drum is recorded on the polygraph by means of a device made from an ordinary door-bell. The breaker mechanism of the bell was removed so that only the make and break of the electrical circuit moved the buzzer arm. On this arm a recording-pen (W_2) is fastened. This device was mounted on a bakelite plate and clamped into position on the polygraph where the two recording-pens (W_1 and W_2) would be in perfect alignment.

Recording-pens. The pens used on the polygraph (W_1 and W_2) were Wrico No. 7. They ride vertically in 1-in. holders made of brass tubing, the internal diameter of which was slightly larger than that of the pens. The pens could consequently

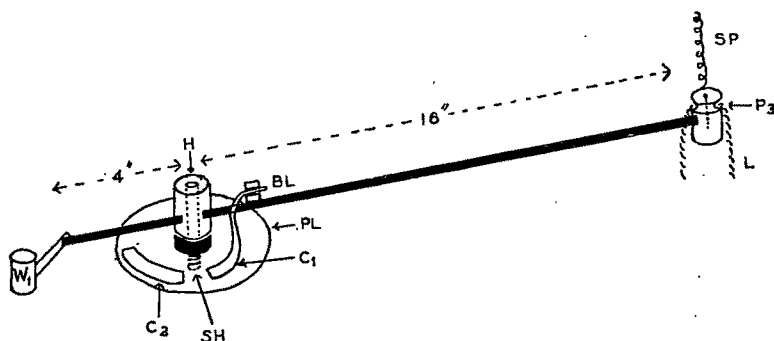


FIG. 2. WRITING ARM

move freely in the vertical direction which allowed for irregularities in the paper-surface.

Harness for the rat. A harness must be made specially for every experimental animal. As it is fairly difficult to construct a harness that will stay on a rat for any length of time without irritating its skin, we shall describe in detail the construction of the tape-and-chamois harness that we have found to be the most successful.

An H-shaped piece of chamois is cut with one side of the H—the side that goes around the chest of the animal—a little longer than the other. Over the cross-bar of the H is slipped an ordinary paper clip by means of which the leash is attached to the harness. After the clip is in place the chamois is covered with strips of adhesive tape cut to width and length. The tape is placed on both sides of the cross bar but only on the external side of the rest of the harness. Holes are then punched in the ends of the strips and short pieces of fish-line drawn through them. The harness is then ready to be placed on the rat. This is best done when the animal is lightly etherized.

A leather leash which runs behind between the rat's legs is attached to the paper clip that is on the belly band at the cross-bar of the H. This point of attachment proves more satisfactory than the shoulders. The fish-line leash which ran to the drum was fastened in turn to the leather leash.

Spring. The spring (SP), Figs. 1 and 2, was closely wound from No. 32 spring wire. It is $\frac{1}{2}$ in. in diameter and, unstretched, 12 in. long. This strength of spring permits long extension with relatively small amounts of effort.

Motor. The motor (M) that we used was manufactured by the Bodine Company. Its rating is 1,600 r.p.m., $\frac{1}{400}$ horse power, 60-cycle, shaded-pole induction, reduction gear ration of 17-1. A more powerful motor is desirable.

Rectifier. The wiring diagram of the rectifier, (R), is shown in Fig. 3.² The operating switch (OS) is the contact (C_1 and C_2) in Fig. 1. The power switch (PS) was so located that the filament heating transformer (FHT) operated con-

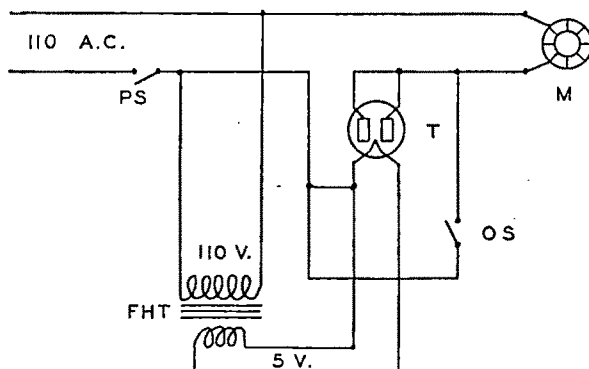


FIG. 3. WIRING DIAGRAM OF THE RECTIFIER

tinuously through the course of a day's experiments. The tube (T) was a Cunningham 83V. In operation, the rectifier passed 110 v. A.C. straight to the motor when the operating switch (OP) was closed; if that switch was open the tube was in series and it passed a pulsating 5 v. D.C. to the motor.

The course of a run. In order to show how the various parts of the apparatus function it may be well to trace the sequence of events in a sample run. At the beginning of the run the writing-arm, held by the spring, is in its resting position (Fig. 1). The rat begins to move and the line feeds through pulley P_3 . Since the drum is motionless, the pulley moves and stretches the long steel spring, turning the writing-arm horizontally. After a certain minimum displacement the slider (C_1) touches the semi-circular contact (C_2) thus starting the motor. Thereafter, so long as the animal moves as fast as the line is released, the motor will keep on turning. If he slows down, or stops, he then serves as a fixed point, the drum releases more line, and pulley P_3 (and the writing-arm of which it is a part) moves back toward its resting point until the circuit through the slider is broken and the motor stops. As long as the rat keeps pulling, the line is released from the drum at a constant rate. The faster he moves, the more he stretches the spring. The displacement of the recording pen thus indicates in a continuous manner how much the spring is stretched as the rat moves toward food. The unit of measurement most

² The rectifier was designed by Dr. Edwin B. Newman to fill the need for a braking device.

easily used with the present arrangement is the average strength of pull during each separate revolution of the drum, the revolutions being recorded by the marker circuit through the contacts on the drum and its base.

Training procedure. Because pulling is a new and strange activity for rats, preliminary training must be carried on until they are adapted to the situation. Lunging is characteristically the first response. After a relatively small number of trials, however, the animals settle down to reasonably steady pulling and almost no retracing. A training series finally found to work with black rats about 100-105 days old was the following: two days of three *free* runs each; three days of unrecorded *pulling* trials, two the first day, three the second, and five the third. Then recorded runs may be made, although more practice trials may be necessary, depending upon the temper of the animals. It is necessary to use moderate motivation during the adaptation period, in order to avoid the extreme lunging which tends to occur in very hungry animals. Practice should be planned with a view to the later experimental

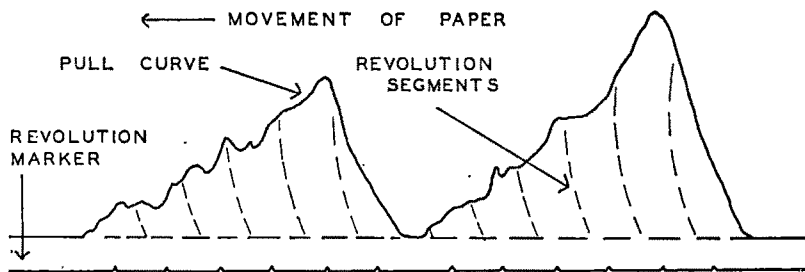


FIG. 4. A SAMPLE RECORD

procedure. Individual caging is necessary and care must be taken that the animals are not weakened so that they are incapable of pulling strongly.

Analysis of records. A curve of an animal's pull for one run can be split into segments corresponding to successive revolutions of the drum (see Fig. 4). This may be done by erecting arcs, like those described by the writing-arm, above the marks made by the revolution indicator. These arcs cross the ink record at a point corresponding to where the revolution-marker *would* have been if it had been moved also. Some sections of the record on the polygraph tape will show no displacement of the pen from the resting position, simply because the animal has stopped pulling. These 'blanks' may be disregarded and the actual successive revolutions of the drum during pulling, as indicated by the marker, used as a basis for computations. The areas of each of these segments can be determined with a planimeter. Each area is then divided by the length of the baseline unit as automatically marked off during *continuous* operation, the result being an average height of a segment of the curve during one revolution of the drum. This measure may be transmuted into the number of grams necessary to stretch the spring this average amount, by means of an empirical calibration of the whole instrument. In the latter procedure, a rough but workable measure may be reached by simply supporting different known weights over a pulley at the end of the line in the runway, letting the polygraph run with

the motor-drum circuit broken. The displacements thus caused and recorded may be used in constructing a curve from which the average heights of experimentally obtained pull segments can be translated into grams.

A word should be said about the final part of the pull-record, when the recording-pen moves toward its point of rest because the rat has stopped at the food-box. Pulling, or at least forward movement, has ceased. It is possible to compute the point at which the animal actually reached food, but it would be better to utilize a treadle at the food-box door and an additional marker on the polygraph. Furthermore, treadles placed systematically throughout the runway would greatly assist in making analyses of the actual amount of work done.

Two corrections have been applied to individual records. The first is for certain defects in the apparatus. With the present set-up, as the rat pulls near his maximum, friction in the pulleys and other parts lessens the relative increase in amount of stretch of the spring. The rat does not, therefore, 'get credit' for all the work done. If the average height of a segment of the curve is translated on an empirical basis into the average number of grams pulled, as described above, the necessary correction is automatically made. The second correction is for weight of animals. It may be desirable to permit pull-values for lighter animals, which pull smaller absolute amounts, to have as much influence in the computations as values for heavier animals. The values for lighter rats may, therefore, be stepped up in proportion to the lack of weight. The desirability of this correction will depend upon the kind of experiment attempted.

The apparatus described, and the procedures as well, have been arrived at in large measure by way of trial-and-error. Particularly difficult to solve are such details as the proper length and tension of the spring, the proper motor-control, and the practical type of training-period. These details must be adapted to the kind of problem to be studied and animals to be used. Experience will suggest modifications of the whole apparatus to fit particular research needs.

APPARATUS NOTES

RECORDING ACTION-POTENTIALS WITHOUT PHOTOGRAPHY

In various psychological investigations it is desirable to secure quantitative records concerning contraction in one or more sets of muscles for a period of time. This need arises no less in studies concerning 'mental work' than in clinical experiments on neuromuscular hypertension. Electrical methods of measurement have proved least cumbersome as a rule, employing fine wires in place of mechanical connections which often restrict the posture or movements of the subject. As an added advantage, if the electrodes are wires or needles, contact is made directly with muscular tissues, so that virtually direct recording of muscular activities is made, in contrast with the data secured mechanically, when displacements and movements afford a less direct record. In many studies, however, records have proved more nearly complete when secured by both routes, mechanical as well as electrical.

When data concerning action-potentials are secured by whatever apparatus, it is always necessary to bear in mind the character, size, location and contact of the electrodes; for what is measured is (at least) the difference in potential between the two electrodes. Whether or not this is also a precise measure of the electrical state of the tissue depends upon the nature of this state and upon the electrical impedance of the measuring system in relation to the impedance of the tissue surrounding the electrodes. Apparently, electrical measurements of muscular and of nervous states up to date are always to be regarded as relative to the individual and to the particular set-up. Within these limits, recording of muscular contraction can be accomplished quantitatively.

Electrical measurement of muscular contraction can be accomplished photographically with the string galvanometer or oscillograph, but the difficulties generally increase with the duration of the time period which it is desired to cover. This is not true if concentric electrodes are employed providing that successive impulses are of equal amplitude, but with most other types of electrodes where successive values of action-potentials are unequal and fluctuating, the photographic record is commonly difficult to quantify, requiring specific and arbitrary measures.¹ Even so, the measurement of selected data on the photographic records along with the corrections of constant errors and the translation of this data so as to yield a plot of action-potential energy against time has often required many hours of labor on the part of a trained technician.

In order to circumvent some of these difficulties, I could think of no procedure more promising than the rectification of the action-potentials to be measured. This procedure had not previously been employed in biology and was described in full at the June 1937 meeting of the American Association for the Advancement of Science and printed subsequently.² A circuit was designed so that the rectified action-potentials were quantified, insofar as their average value at any instant could be read

¹Edmund Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man: Studies on arm flexors, *Amer. J. Physiol.*, 107, 1934, 230-248.

²Jacobson, The neurovoltmeter, this JOURNAL, 52, 1939, 620-624.

on a meter. Obviously, the next step toward a non-photographic registration of action-potentials required only that some means be employed of storing up the action currents over a desired unit of time and then proceeding to measure the accumulated charge. This was readily accomplished with the use of a condenser. The measurement of the condenser charges from time to time can be performed most accurately by means of a ballistic galvanometer.³ Accordingly, an integrating instrument, including a condenser and a galvanometer, was developed and employed for a considerable period of time, being finally reported in full before the American Association for the Advancement of Science on December 28, 1939.⁴ At that time, circuit diagrams were available to investigators and were supplied upon request. Through the Publicity Department, the lay press proceeded to widely publicize this new method of measuring neuromuscular states, and two journals of engineering also contained brief descriptions of the instrument.⁵ A report of the new non-photographic method of recording action-potentials was made also before the American Physiological Society in March, 1940.⁶

The new apparatus served to integrate the values of action-potentials over time insofar as charges were accumulated on a condenser. Accordingly, it seemed appropriate to name it the "Integrating microvoltmeter" or "Myovoltmeter." Another name employed was the "Integrating voltmeter" for the study of nerve and muscle potentials. The apparatus was shown at a meeting of the Mid-Western Psychological Association in this Laboratory, as a part of a demonstration of the measurement of action-potentials on May 2, 1940.

In an integrating instrument of the type mentioned, there are various other ways to measure the condenser charge from time to time, but they are likely to be less accurate than the ballistic galvanometer. One such method is the use of a thyatron relay device, another a gas-filled tube which activates an impulse counter of the Klopsteg or other type. The objection to such devices is that they depend upon the condenser reaching a state of "full charge," thereupon discharging automatically. Unfortunately, the condenser will discharge irregularly from time to time with a great percentage of discrepancy between successive accumulated charges. Discrepancies in such a method of testing condenser charges are so great as to raise the question of scientific adequacy, unless errors chance to cancel out on a statistical basis.

However this may be, Freeman and Hoffman recently described "a technique for integrating directly and without resort to translation from photographic records the total electrical disturbances developed by body tissues."⁷ "In our integrator," they

³ The galvanometer used is a highly damped D'Arsonval instrument of high moment of inertia, with no appreciable restoring spring so that the deflection is proportional to the product of the current and the time. These requirements are met in a fluxmeter but the sensitivity must be high for present purposes.

⁴ Jacobson, The direct measurement of nervous and muscular states with the integrating neurovoltmeter (action-potential integrator), *Amer. J. Psychiat.*, 97, 1940, 513-523.

⁵ Jacobson, Machine measures nervousness and tension, *Electronics*, 1940 (June), 83; Measuring nervousness with the integrating neurovoltmeter, *Radio & Television*, 11, 1940, 200.

⁶ Jacobson, Variations of muscular tension (action-potentials) in man, *Amer. J. Physiol.*, 192, 1940, 388.

⁷ G. L. Freeman and E. L. Hoffman, Electrical integrator for "action currents," *Rev. Sci. Instr.*, 11, 1940, 283-284.

continue, "the amplified action currents accumulate as charges on a condenser until a given amount is reached, whereupon they are discharged by a gas-filled tube to activate an impulse counter." Evidently, the statement quoted would apply equally to the "Integrating neurovoltmeter" described the year previously, deleting what follows the word "condenser" and substituting the phrase, "which are measured with a ballistic galvanometer."

Obviously, the apparatus of Freeman and Hoffman is a modification of the "Neurovoltmeter" and "Integrating neurovoltmeter," employing the principle of integration first developed in this apparatus as devised and described in June 1937. Landis, in coöperation with this Laboratory, is even now seeking to develop such a modification in the hope that it may work roughly, but with reasonable accuracy. In any event, the instrument of Freeman and Hoffman must rest its claims to novelty merely on greater portability and on its convenient use of a counter. It presents no new improvement on the earlier devised "Integrating neurovoltmeter" in point of scientific measurements; on the contrary, their instrument, as described, is much less accurate and considerably less sensitive and should not be used if it is desired to measure the lower range of residual tension. With the "Integrating neurovoltmeter" it is possible to measure accurately, making corrections for constant errors, to small fractions of a microvolt.⁸

Several additional criticisms of Hoffman and Freeman's device remain to be mentioned.

(1) There is no assurance that the charging current to the condenser is proportional to electrode voltage. (a) The impedance in the charging circuit, primarily of the Tube 56, is far from constant at low values of current so that the relation between electrode voltage and charging current would not be linear for such a condition. (b) Apparently most of the charging current derives from amplifier noise as indicated by the authors' statement that the system is adjusted to discharge 120 times per min. with no tissue voltage and that in a certain test with tissue voltage the rate was 170. One might inquire just what it is that is measured.

(2) Variations that inevitably occur in the ignition and extinction voltages of the gas tube evidently may lead to marked errors in measurement. Nothing that the authors say excludes the possibility that the ignition voltage and the extinction voltage might each vary as much as 2 v. from one discharge to the next. While they do not indicate the range between ignition and extinction, it would appear that it would probably be low, perhaps not more than three or four times the combined variation in the ignition and extinction voltages. If such should be the case, the device can be regarded as unscientific and workable if at all only because of the statistical leveling process. Incidentally the inductance of the Klopsteg counter may influence the extinction component because of the $-L di/dt$ component.

(3) The stability of the electrical system seems dubious. (a) Since amplifier noise voltages are important components of the potential giving rise to the condenser charge, it would appear that any adjustment of the amplifier gain during the normal use of the equipment would upset the base line rate. (b) The plate potential versus grid potential characteristic of the gas tube for the condition of breakdown is so steep that circuit contact potentials, to say nothing of variations in

⁸ Jacobson, An integrating voltmeter for the study of nerve and muscle potentials, *Rev. Sci. Instr.*, 11, 1940, 415-418.

the grid biasing battery, would suggest that the base line rate would have to be adjusted or calibrated after each observation.

(4) The correlation coefficient is inadequate from the standpoint of a research instrument. The correlation coefficient of 0.76 signifies that less than 60% of all causes of operation are common to the two systems under comparison. Such a high degree of independence (40%) of operation of the Hoffman and Freeman device as compared with the reference suggests unreliability. The lack of suitable correlation would seem to lend weight to the criticisms offered above.

In summary, it has become possible to record action-potentials without photography, thanks to the principle of rectification described in the present studies in June 1937. In the first instrument, the value of the rectified action-potentials at any instant could be read on a meter. The next step consisted of averaging or integrating these potentials over periods of time such as a minute. This was accomplished by means of the "Integrating neurovoltmeter" (Action-potential integrator), which was described for the first time in 1939, although developed in years previously. It affords data for accurate graphs of action-potentials against time with great convenience.

Obviously, the claims of Freeman and Hoffman regarding the development of a method for integrating action-potentials without photography can not rest on priority, unless a number of papers presented previously at scientific meetings or published in scientific journals are ignored; nor can their claims be based on improvement on the apparatus previously described as the integrating neurovoltmeter, unless voltage sensitivity, stability, and scientific accuracy are regarded with indifference.

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THE CORNELL SUMMER RESEARCH STATION IN PSYCHOLOGY

The Department of Psychology of Cornell University wishes to announce that the Summer Research Station which was established on trial during the past two summers will be continued this year. The psychological laboratories, seminary, library, and the behavior farm will be open for research and study. Psychologists with the doctorate will not be charged tuition or other fees. They will be given the status of Resident Doctor with all the rights and privileges of University membership. All others will be charged the usual fees of the Cornell Summer Session.

The Research Station will be open from June 17 to September 1. Attendance may begin and end at any time within those limits. The Cornell Summer Session runs from July 7 to August 15 and will probably be the most advantageous time in which to attend.

Application for admission to the Research Station should be made to the Secretary, Department of Psychology, Morrill Hall, Cornell University, Ithaca, N.Y..

K. M. D.

NOTES AND DISCUSSIONS

PROBLEMS IN THE PLANNING OF PSYCHOLOGICAL EXPERIMENTS

Psychologists are beginning to borrow and adapt methods of statistical analysis from experimental agriculture; particularly the methods of the analysis of variance and covariance, as developed by R. A. Fisher.¹ These methods have appeared frequently, especially in the journals for agriculture and biology, but it is only recently that reports of studies using these methods have been published in psychological journals,² and that a special symposium has been devoted to them at a psychological meeting.³ Similarly, psychological articles are now appearing in which special designs of experiments, also developed by Fisher and his students, have been employed.⁴ Certain problems and difficulties arise, however, whenever a method used in one field is brought over to another, especially where these fields are as different as agriculture and psychology. This article will attempt a discussion of some of these problems as they arise in the design of experiments.

Perhaps one of the greatest difficulties is in the new vocabulary. These terms, developed as statisticians applied their knowledge to agricultural problems, tend to be more pertinent in that field of investigation. In order to become truly meaningful when used in their new field, they must be clearly understood in relation to psychology and its ways of dealing with problems. Among the more important of these words are treatment, level, variety, combination, replication, block, plot, confounding, experimental error, and heterogeneity. Let us examine briefly the meanings for all of the words and then deal more extensively with the problem of heterogeneity as faced by the psychologist.

It is important in this reorientation to understand the following analogy. Treatment : Soil :: Experimental Variables : Organism (or group of organisms). Whereas in agriculture various seeds, fertilizers, and spacings, are applied to the soil, in psychology we apply various shocks, directions, study methods, and emotional situations, to our subjects. Treatments do not include every part of the experimental situation, such as the variables held constant (*i.e.* temperature, quiet, number of people present), but only the variables under direct investigation. Treatment corresponds to the traditional term independent variable.

Level refers to the aspects of the variable as applied in the experiment. In its simplest form it refers to the presence of the variable in one group (experimental) and its absence in another group (control). This would mean that the variable

¹ R. A. Fisher, *Statistical Methods for Research Workers*, 1936, 1-339.

² J. W. Dunlap, Applications of analysis of variance to educational problems, *J. Educ. Res.*, 33, 1940, 434-442; K. H. Baker, Item validity by the analysis of variance, *Psychol. Record*, 3, 1939, 242-248.

³ At the meetings of the Midwestern Psychological Association in Chicago in May, 1940.

⁴ R. S. Crutchfield, Efficient factorial design and analysis of variance illustrated in psychological experimentation, *J. Psychol.*, 5, 1938, 339-346; Brent Baxter, The application of factorial design to a psychological problem, *Psychol. Rev.*, 47, 1940, 494-500.

has two levels. As the term more or less implies, level means different degrees or amounts of a variable that may be expressed on a quantitative scale. For example, the variable might be length of study period and the levels, 1 hr., 2 hr., and 3 hr. (three levels); or if the variables were the size of the interacting social group, it might have four levels, or 4, 8, 12, and 16 Ss in the experimental groups. If the variable is of a discontinuous or qualitative nature, the "divisions" of the variable have been called varieties instead of levels. The term arose from the comparison of several different seedings, but psychologists may use it in referring to varieties such as red *vs.* green *vs.* blue or economy *vs.* social prestige (as in a study of advertising appeals). Level is sometimes loosely used in referring to varieties. The number of

		HOURS SPENT IN RECITATION (4 levels)			
		0	1	2	4
TYPE OF MATERIAL (2 varieties)	NS	1 N S 0	2 N S 1	3 N S 2	4 N S 4
	MF	5 M F 0	6 M F 1	7 M F 2	8 M F 4

FIG. 1. A LOGICAL ARRANGEMENT OF THE COMBINATIONS
(2 x 4 experiment)

levels or varieties chosen in any case depends upon the questions that the experimenter seeks to answer. If he wishes to make only a single comparison, two levels are adequate; if he believes that the factor deserves a more detailed examination, more levels may be selected.

Let us suppose that we wish to perform an experiment in learning involving the factors, kind of study material and hours spent in recitation. The first variable has two varieties, nonsense material *vs.* meaningful material, and the second has several levels, perhaps four, *i.e.* 0 hr., 1 hr., 2 hr., and 4 hr. in a study period. Since both variables (type of material with two varieties and hours spent in recitation at four levels) are to be studied simultaneously, we have a factorial design,⁵ and our Ss would be given *combinations* of the variables, *i.e.* 2 hr. of recitation with nonsense material, or no recitation with meaningful material, etc. In this case there would be eight possible combinations of the two treatments (2 x 4).

The terms, replication, block, and plot, refer to the plans or scheme for presenting the variables to the Ss. For example, let us refer to the experiment suggested above. Fig. 1 shows the eight combinations in a logical arrangement (not as they

⁵ A design or experiment is said to be factorial when it deals with two or more factors, or variables, in all of their combinations.

would probably be arranged for an experiment where randomization is desired). NS and MF refer to nonsense and meaningful material, respectively. The numbers in the center of each square refer to the number of hours spent in recitation. The numbers in the upper left-hand corner merely count the eight combinations and will be used in referring to them.

The entire rectangle represents one replication. Each *S*, or groups of *S*s, who receives all of the possible combinations (in our experiment, eight), is regarded as a unit, a replication. An experiment usually has more than one replication. A replication is added each time a new *S* or group of *S*s is subjected to all of the combinations. In a simple experiment⁶ a replication is also known as a "block." The rectangle, therefore, also represents a complete "block." The smaller squares, or subdivisions of the block, are termed "plots." Therefore, each combination is a plot, and eight plots make up our block.

An example of block and plot where factorial design is not used may be helpful.

		TYPE SIZES			
		1	2	3	4
SUBJECTS	A				
	B				
	C				
	D				
	E				

FIG. 2. BLOCK AND PLOT WHERE FACTORIAL DESIGN IS NOT USED

We might be interested in the problem of seeing whether different type sizes (four) affect the speed of reading. With *S*s A-E being tested for each of the four type sizes, the unrandomized plan would be as in Fig. 2.⁷ Every *S* represents a block and complete replication, while each separate square is a plot. There are 20 plots and 5 blocks. It is difficult to give a definition of "block" that will hold for all designs. Perhaps the following is a close approximation. The block is that basis of comparison that arises from the design of the experiment; *i.e.* those comparisons that are a product of the plan of applying the treatments to the *S*s rather than giving answers to the specific hypotheses of the experiment. For Fig. 1 all interplot comparisons are meaningful in terms of the experimental hypotheses; it is only when we add more groups of plots of the same type that block comparisons arise. Similarly,

⁶ "Simple" here means unconfounded, which will be explained later.

⁷ In a real design the order of type size would be randomized for each individual.

in Fig. 2 the experimental questions are answered by comparing the responses in terms of the columns. The addition of Ss, each contributing information for those questions, presents us with further comparisons that are really outside of our real problem (effects of type size). In the arrangement of Fig. 2, however, the block comparisons are interesting for they are an estimate of individual differences.

It is easy to picture the rectangle as a piece of land, but how is it related to, for instance, 20 sophomores in a laboratory course in psychology? To understand better the meaning of the experimental group⁸ in terms of blocks and plots, let us examine the two possible arrangements: (1) where a single individual or a group is a block, and (2) where a single individual or a group is a plot.⁹ In the first case the individual would receive all of the eight combinations in a randomized or chance order; he may be regarded as the entire rectangle of Fig. 1, and said to represent a block. He is also a complete replication. In the second case the individual receives only a single combination, perhaps number seven. He is regarded as a single plot. We will need eight such individuals to form a complete block or replication. Either of these arrangements is applicable to psychological experiments. No hard and fast rules apply to their choice; the one selected for use will depend upon many factors, some of which will be discussed later.

It was implied above that the block may sometimes not be the same as a complete replication. This occurs only when confounding is applied to factorial design. Confounding is a device which obviates the necessity of exposing each block to every treatment combination. In this device the total number of combinations is divided into parts, each one being a block and all together being a replication. Referring again to Fig 1, we might divide the eight combinations by the horizontal middle line, thereby separating the replication into two blocks, one containing plots 1, 2, 3, and 4; the other containing 5, 6, 7, and 8. We may then follow either of the two possible methods for assigning individuals to blocks. Let us suppose that one individual is represented in the four plots above the line and another individual is represented in the four plots below the line, and further, that we have collected the data for both of them. The differences between their total scores is a measure of individual differences. It happens, however, that as the result of the way we divided the replication, the difference between the two blocks is also equal to the difference between the effects of nonsense syllables and meaningful material. It is said, therefore, that the effect of the factor (type of material) is "confounded" or confused with the block or individual differences. A particular effect is confounded if its source of variance is also an estimate of the effect of some other source. In other words, effects have been confounded if an experiment is so designed that a given comparison estimates the contribution of more than one source of variation. The concept might well be applied to any situation where the data is the effect of more than one cause, and we are unable to isolate that portion of the effect that is

⁸ In this type of design of experiments the control group is not a separate group as we usually conceive it. All Ss are 'experimental;' controls are represented in the experimental variable as one of the levels as mentioned above.

⁹ In the remainder of this discussion "individual" will be used but "or group" is to be understood with it. In either case only a single score would be used to represent the plot.

due to the single source. Psychologists are still struggling to unconfound the effects of heredity and environment, but have not yet found the perfect design.

If the horizontal middle line of Fig. 1 represented our method of confounding, the accurate estimation of the effects of our factor would be impossible. It is unlikely, however, that we would in a real experiment divide our replication as we did in this instance. We would probably divide the replication so that the information that was confounded with block differences would be relatively unimportant. It may be noted that the divisions of the confounded replication have been called blocks, for the difference between the two halves does not help us in answering our experimental questions. The difference is really a comparison arising from the special design we have used and is called a *block* difference.

Factorial design yields information concerning interactions. Where the relationships between the levels for one factor vary with levels in another, the factors interact. If the increment from one level to the next in one variable depends upon the level at which another variable is measured, the two variables are said to interact. The relation is one of discrepancy rather than covariance. For example, if the relative influence of type of material changes with the number of hours of recitation, *i.e.* meaningful material is easier to learn when 4 hr. are spent but nonsense material is easier when 2 hr. are spent, an interaction exists. In some psychological experiments, we might not be interested in interaction, and the replication could be divided so that the block difference would equal the interaction.¹⁰ If we wished to have some estimate of the interaction and yet wished to use confounding, we might use partial confounding. In this case, different treatment effects are confounded in each replication. For example, the interaction might be confounded in two replications, but in two other replications it would be unconfounded. From the latter two we would obtain our desired information. The information that is lost in the first two replications is partially compensated for by the information gained in the other two. In the final analysis we will have information concerning all of the treatment effects, though some effects will be based on fewer replications. Other advantages offset this loss of information. For a thorough discussion of confounding, the reader is referred to Fisher.¹¹

Experimental error is a purely mathematical term. It is the residual variance or that variance (variability) about the grand mean which cannot be accounted for by the factors or controls of the experiment. The experimenter designs his problem so that portions of the total variance may be said to be the result of certain known factors,¹² such as the treatments, differences between blocks, etc. In our problem we would first find the variability for all of the scores (total variance or variance about the grand mean) and then subtract the variation attributable to (1) the difference in the type of material used, (2) the difference in the number of hours spent in recitation, (3) the interaction of the two variables, and (4) the difference between blocks. The remainder would be our estimate of the experimental error. The unassignable portion is the basis for tests of significance; it is analogous to the standard

¹⁰ For example, the difference between the effects of plots 2, 3, 5 and 8, and the plots 1, 4, 6, and 7 corresponds to one of the interaction effects (the quadratic).

¹¹ Fisher, *Design of Experiments*, 1937, 1-258.

¹² Actually in the analysis of variance it is the sum of squares of the deviations from the grand mean that is broken down into its several components.

deviation from which, in the older forms of statistics, the standard error of the difference might be calculated.¹³ An efficient design seeks to get a precise estimate of the experimental error so that all of the variables will have a sound basis for evaluation. An essential condition of a properly designed experiment is that it must provide its own estimate of experimental error.

Experimenters in agriculture found that a great obstacle in their search for a precise estimate of error was soil heterogeneity.¹⁴ The designers discovered that whenever there was a variation in the quality of the soil within a block, the treatments in that block would be affected differently. Since there were no means available to evaluate accurately the differences due to the heterogeneity (no subtraction of those differences from the total variance was possible), whatever increase in variation was due to it, would, therefore, become part of the experimental error; this would decrease the precision of the judgments concerning the factors being investigated. The designers have spent considerable effort in devising techniques of avoiding this reduction in precision.

The question is asked: How are these conditions related to the design of psychological experiments and how do they affect the selection of experimental groups? In psychological experiments the problem of heterogeneity is present, though in a somewhat complex manner. In agriculture the problem is always related to the spatial aspects of the block. In psychology, however, the block cannot be said literally to have space, since it refers to individuals who are not fixed in location. The exact relationship is more easily seen by recalling that a block may refer to a single individual, or may be made up of several individuals, each a plot. The latter case is more truly spatial heterogeneity in so far as there are differences from individual to individual in the block. That is, if there were an individual in each of our eight plots, there would be differences (individual) in our measures of the plots regardless of the treatments. The greater the differences, the greater will be the effect upon the experimental error. It is important to ask wherein lie the differences. Since in the above example, we are taking measures of speed of learning, individual differences in physical characteristics, reaction-time, etc., would have little effect on those measures. We would more likely be interested in the differences in learning ability: If, therefore, we selected a group for our experiment who were very homogeneous in regard to learning ability, the experimental error would be decreased. The conclusions reached from a design using selected homogeneous blocks, however, would be limited to the particular level of learning ability of the Ss, unless it should be assumed that there is no interaction between learning ability and the factors being investigated. Where each plot is a group, however, the differences within each group will not affect the error, but only the differences between groups.

In the design where an individual is a complete block, the heterogeneity assumes

¹³ If the sum of squares assigned to error is divided by the appropriate number of degrees of freedom, we have the mean square. The square root of this is the standard deviation. This, in turn, when divided by the square root of the number of plots upon which the mean is based, gives the standard error of the mean. To get the standard error of the difference between two such means, the standard error of the mean is multiplied by 1.414.

¹⁴ J. Wishart and H. G. Sanders, *Principles and Practice of Field Experimentation*, 1936, 1-100.

a temporal aspect. In giving a series of combinations, such as our eight, to the individuals, we are forced to do so at different times. Therefore, in so far as the individual differs from one time to another, heterogeneity is introduced. In agriculture all of the treatments operate at the same time, and this aspect of heterogeneity is to a large extent avoided. In addition to differences in the individual and his responses resulting from time to time variations, there are differences in some cases that might be classified as due to practice. If, as the result solely of being given the first treatment, say number four, *S* is more (or less) proficient in dealing with the subsequent experimental material, his responses to the second and successive treatments will be affected by a factor other than that being investigated. When dealing with some psychological material, these differences might be very large; with others the practice effects are fairly negligible. For example, if the experiment dealt with the influence of certain factors on the speed of typing, *S* would probably tend to receive a higher score on the second treatment regardless of the nature of that particular treatment. If the experiment, however, dealt with the adaptation to darkness, little heterogeneity would be introduced by practice effects.

There are other temporal aspects that may be classified as due to fatigue. If several treatments are applied to the same individual in one period of observation, the individual may become tired and therefore his responses to the later treatments will not be on the same basis as to the first treatments. This likewise will increase the experimental error. Similarly, with some psychological measurements, there is little variation when the measures are taken only a few hours apart, yet the scores may be highly variable when measured on different days. Also, a series of stimuli presented to the senses, especially the olfactory sense, might be accompanied by adaptive phenomena. Thus it is clear that the temporal aspect of heterogeneity may vary with the length of the experimental period, with the arrangement of the treatments, and with the kind of psychological measurements involved in the experiment. These sources of error will not come out in the "statistical wash;" all of these factors must be carefully considered in the design of the experiment if precision is to be attained.

No exhaustive list of solutions to these problems will be presented here. The number of possible useful solutions will depend upon the skill and ingenuity of the experimenter. Only a few will be suggested.

If factorial design is being used, confounding will help to reduce the temporal heterogeneity. This is especially applicable where a single individual represents a block, since only a fraction of the total combinations need be given in the parts of the confounded replication. As noted in our experiment, only four combinations need be given to the individual instead of eight. Differences between blocks may be isolated as individual differences³⁵ and with the shortening of the period of observation, fatigue effects will be avoided. Confounding will be of less help where each individual is a plot, for the differences between blocks are group differences, which are only a portion of the individual differences. The heterogeneity introduced by the difference in individuals will be only partially isolated. This solution is of use only with factorial design.

³⁵ The block differences will be estimates of the particular effect confounded as well as individual differences.

If the designer suspects that individual differences will contribute significantly to the errors while temporal heterogeneity (due to fatigue, practice, or trait variability) is not important, the simplest design is the randomized block. Every individual, representing a complete replication, is given all of the treatments, such as our eight, in a random order. The block differences (replication differences) are separated from the total as individual differences.

A design often employed in agriculture is that called the Latin Square. This design takes into account the variations in soil that occur in two directions, both along and across the field. Each treatment appears once and only once in each row and column of a square. Since the spatial aspect does not fit psychological subjects, this design will perhaps not be as useful when taken over to its new field. Fig. 3 presents a Latin Square for an experiment testing the effect upon learning of dif-

		COLUMNS			
		0	1	2	4
ROWS	1	1	4	0	2
	4	4	2	1	0
	2	0	4	1	

FIG. 3. A 4 x 4 LATIN SQUARE

ferent lengths of time spent in recitation.¹⁰ If each column were an individual to whom four combinations were given, the differences between columns would then be an estimate of individual differences which can be "removed" from the experimental error. The comparisons between rows are differences between order of presentation of the learning material. Since the size of this type of difference will vary with the factors and the problem, the value of the cross randomization will depend upon the particular conditions of each experiment, but usually it will not be very large.

Fig. 4 presents a Latin Square design which would be efficient for our 2 x 4 experiment. The numbers refer to the treatments as given in Fig. 1. The columns are individuals; the rows are days. The arrangement shows that every individual receives each of the eight treatments only once and that every treatment appears once on each day. From this design we would get clear estimates of individual differences, day to day effects, and treatment effects. This arrangement, however, is limited in so far as it requires the same number of days as there are combinations. For some experiments this limitation would be serious. In experiments where it would be possible to administer all eight combinations on one day, the row differences would give an estimate of trial differences or practice effects.

¹⁰ The number of different 4 x 4 Latin Squares is 576. The one used in an experiment should be chosen at random from these.

There is another scheme, a modified randomized block, which approximates the direction of the Latin Square and is more appropriate to psychology. Fig. 5 shows it as applied to the 2 x 4 experiment. The numbers again refer to the treatments counted in Fig. 1. Each of the 4 Ss (*A*, *B*, *C*, and *D*) receive all of the treatments in a random order. This randomization is subject to the restriction that on the first day there will be given the same number of each combination as on the second day (two of each combination on each day here). With this design the experiment

		INDIVIDUALS							
DAYS		1	2	3	4	5	6	7	8
		5	6	7	8	1	2	3	4
		2	1	4	3	6	5	8	7
		6	5	8	7	2	1	4	3
		7	8	5	6	3	4	1	2
		3	4	1	2	7	8	5	6
		8	7	6	5	4	3	2	1
		4	3	2	1	8	7	6	5

FIG. 4. AN 8 x 8 LATIN SQUARE

covers only two days as compared with the eight days for the Latin Square. Where the replication is divided into two, the number of Ss used must be some power of two; where it is divided into three, the number of Ss must be some power of three, etc. The reason for this rule is that only in this way can the same number of each combination be given on each day. By this randomized block estimates may be obtained of the day to day performance which may be isolated from the total. Likewise the individual differences will not be part of the experimental error. If trial to trial effects within each day were of considerable magnitude, heterogeneity would, however, be introduced.

Perhaps the design that will be most useful in the study of certain problems in psychology is the incomplete randomized block.¹⁷ This design enables the experimenter, as in confounding, to assign only a portion of the total combinations or treatments to a block. It will be of special advantage in research in which the number

¹⁷ The author is indebted to Dr. Palmer O. Johnson for suggesting the inclusion of this design and for the examples accompanying it.

of Ss is limited. For example, if we were studying monozygotic twins, our very homogeneous block would be limited to two plots. If, therefore, we wished to study more than two combinations, we would have to employ some design permitting a reduction in the number of combinations per block to two. Similarly, in studies of animals, the number of plots in the block would often be limited to the number of animals in the litter. Where there were only four animals in a litter, (the litter being regarded as a block) the number of combinations per block is limited to four. The construction and analysis of the incomplete randomized block is complex. The

		INDIVIDUALS			
		A	B	C	D
FIRST DAY		4	1	6	3
		6	7	3	8
		2	8	1	4
		7	2	5	5
SECOND DAY		3	6	8	6
		8	4	7	7
		1	5	4	1
		5	3	2	2

FIG. 5. RANDOMIZED BLOCK DESIGN, SPECIAL RANDOMIZATION

reader who is attracted by this interesting and efficient design is referred to the discussion by Goulden.¹⁸

There are two further suggestions which may have already been inferred from the earlier discussion. If practice effects are to be reckoned with, it may be well to practice the Ss until no further improvement is shown, before the treatments are given. The nature of the material will indicate whether this is feasible. If day to day variations are large, all of the treatments should be given on one day, or the randomization suggested above used.

SUMMARY. With the borrowing of statistics and experimental designs as used in agriculture, psychology faces the problem of adapting them to its own peculiar inquiries. The terms, treatment, level, variety, combination, replication, block, plot,

¹⁸ C. H. Goulden, *Methods of Statistical Analysis*, 1939, 1-277.

confounding, and experimental error are given psychological meaning. The relation of heterogeneity of the experimental block to the precision of the tests for significance, which caused so much difficulty in agricultural designs, is discussed with particular reference to psychology. Some solutions as seen by the psychologist are offered.

University of Minnesota

BRENT BAXTER

COMMUNALITY IN RELATION TO PROACTION AND RETROACTION

It is the purpose of this note to discuss the theoretical nature of the interaction between two successively learned materials when the degree of communality between the materials is variant.

Those who reduce similarity to partial identity or to functional substitutivity will consider that communality provides an operational meaning to similarity, but there is no need to enter upon this moot question here. It is obvious that such research on learning and memory as is interpreted in terms of interference presupposes some kind of analysis of the total organizations that are being learned, an analysis which implies the possibility of reinforcement when a part in the first material is repeated intact in the second, and associative or retroactive inhibition when the first organization is contradicted by the second.

Robinson dealt with the problem in 1927 and plotted the theoretical curve for retroactive reinforcement and inhibition as a function of similarity, drawing upon experimental results in which he took communality to mean similarity.¹ The curve that he drew somewhat resembles the curve of Fig. 1, at least in respect of the positive and negative values and of the maxima and minimum. This note is concerned with the rationale of this function.

Let us assume that an organization or pattern A is learned first, and that afterward another organization B is learned. The facts of positive and negative proaction or transfer are discovered by studying the effect of the learning of A on the learning of B. The facts of retroactive facilitation or inhibition are found by studying the effects of the learning of B on the reproduction of A. Let us assume further that the patterns for A and B are equivalent in complication and range.

We can not discuss the communality of A and B without analysis, but the mistake that has too often been made is to regard as adequate the analysis of an organization into a congeries of parts. Is there any way in which an organization can have its communality with another organization expressed except by counting the number of identical elements? Perhaps there can be no safe general rule, but certainly we shall be nearer the truth if we say that a pattern consists of terms in relation and the attempt to enumerate both the terms and the relations. Such a procedure keeps the attention upon the organizational nature of what is learned, and also makes it quite clear, even with the simplest method of counting, that there are bound to be many more relations than there are terms related. It is with such assumptions in mind that I offer a simple analysis of Robinson's function.

Let us suppose that A and B are each analyzed into n terms and that some of

¹ E. S. Robinson, The 'similarity' factor in retroaction, this JOURNAL, 39, 1927, 297-312, esp. Fig. 1, 299.

the terms in B are 'old' terms which occurred in A, and that the others are 'new,' not having been in A.² Let:

n = number of terms in A and in B.

n_1 = number of terms from A repeated in B.

n_2 = number of new terms in B (and thus the number abandoned in A).

$n = n_1 + n_2$

Now let us make the simplest possible assumption about the relations, counting a relation as existing between every pair of terms. There are thus three kinds of relations: repeated relations where both terms that make the relation are repeated from A in B, unrepeated relations where both terms are changed from A in B, and altered relations where one term is changed and the other not. It is in this third class that interference and inhibition occur, since a relation has to be disorganized in order that the new organization be formed. Let:

r = number of (paired) relations between n terms.

r_1 = number of repeated relations between n_1 terms.

r_2 = number of entirely new relations between n_2 terms in B (and thus the number entirely abandoned in A).

r_3 = number of altered relations, where one term is repeated and the other changed.

$r = r_1 + r_2 + r_3$

This schema, being much too simple for reality, can not lead to any results that are more than grossly approximative, but a simple mathematical model of this sort helps nevertheless to clarify thought about fundamentals. It follows by theory of combinations and algebraic substitution that:

$$r = n(n-1)/2$$

$$r_1 = n_1(n_1-1)/2$$

$$r_2 = n_2(n_2-1)/2$$

$$r_3 = r - r_1 - r_2 = nn_1 - n_1^2$$

This function is symmetrical, that is to say, r_3 is maximal when $n_1 = n/2$; the greatest number of altered relations occurs when half the terms are changed from A in B and the other half are repeated. This situation is where the most interference and inhibition should occur.

Perhaps we should go no further, but it is obvious that we have here reëforcements working against interferences and inhibitions and some sort of algebraic addition may be possible. Let us assume then that every r_1 acts equally for reëforcement, that every r_3 acts equally in the opposite direction for associative or retroactive inhibition, and that no r_2 has any interactive effect, since an r_2 -relation in A has no relation at all to any relation in B, and conversely. Let F be the total facilitation (reëforcement). Then:

$$F = r_1 - r_3 = n_1(3n_1 - 2n - 1)/2$$

That function is the curve plotted in Fig. 1. It is obvious that, when $n_1 = n$, $F = n(n-1)/2 = r$, for all the relations are facilitative. When $n_1 = 0$, $F = 0$, for

² If n is different in A and B, the functions can be worked out in a similar manner, but the curve for proaction is then different from the curve for retroaction.

there can be no interaction between entirely disparate organizations. The minimum of this curve lies at $n_1 = n/3 + 1/6$, where $F = -(n + 1/2)^2/6$. It crosses the axis, $F = 0$, at $n_1 = (2n + 1)/3$.

This curve is not quite the shape of Robinson's. He levelled his curve off at the two maxima and made the minimum sharper. He drew his experimental evidence from different studies, and it would be desirable to try to test this total function throughout its range with only communality variant. It is probably not to be expected that the right end of the curve, $n_1 = 0$, could ever be found, since the evidence from transfer and retroaction is that no two comparable organizations could be completely disparate. On the other hand, the existence of the points *H*, *J* and *K* in Fig. 1 must certainly be regarded as established. Repetition does, when the first learning is not complete, reënforce (*H*). Partial communality leads to negative transfer or retroactive inhibition (*J*). The absence of interaction for completely

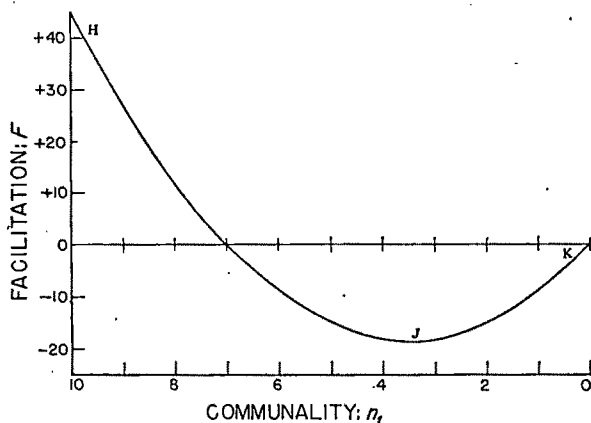


FIG. 1. FACILITATION AS A FUNCTION OF COMMUNALITY

Values of facilitation, F , for communality, n_1 , when n , the maximum communality, is 10. $F = n_1 (3n_1 - 2n - 1)/2$. Thus the shape of the curve changes somewhat with the different values of n .

disparate organizations is a necessary consequence of a useful conception of disparity (*K*).

It is not to be expected that the function of Fig. 1 could be empirically verified as to its exact form, for it is based upon much too simple assumptions. For instance, the various r_{12} s and r_{23} would hardly be equal and subject to algebraic summation. The general principle, however, that interaction, as a function of communality, should vary from reënforcement (*H*), first through no effect and then through inhibition (*J*), to a final no effect at complete disparity (*K*)—Robinson's principle—must hold unless some of our most elementary theses about interaction are wrong, and the foregoing derivation shows what factors would have to be known in order to predict an exact function.

This method of treatment of proaction and retroaction is, of course, most ob-

viously applicable to the learning of serial organizations, like lists of syllables or prose sequences. Here the r s resemble the direct and remote associations, which are known not to be equal, but which might have their strengths measured so that a proper function could be predicted, provided no other interaction would enter in—an improbable assumption.

On the other hand, the general argument seems to me to apply also to many other kinds of learning. Take the case of study, which might first be analyzed into motivation and content. If motivation is attached to a given content, it will transfer to other contents of sufficient communality (H)—from one study to more of the same study, and from one study to another study since both are studies. 'Formal discipline' is partly true for this reason. There will, however, be inhibition if there is too little communality (J). Motivation for play will not help study unless both activities share some common element, like personal achievement, that can be attached to the same motivation. Given one motivation for play and another for study, we might, however, approach the dissociation of complete disparity (K), so that the two learning processes could alternate without any interaction.

The difficulty of applying this logic to discrimination learning, which is now so much in the air, lies in the fact that the nature of the discriminatory response is usually the same (selection and rejection), so that there is always enough communality to make the events lie in the H - J part of the function. If an animal learns to choose a black-triangle-on-the-left and to reject a white-circle-on-the-right, one would expect positive transfer to the task of learning to choose a white-triangle-on-the-left as against a black-circle-on-the-right, and negative transfer to the task of learning to choose a white-triangle-on-the-right as against a black-circle-on-the-left. Here we may know a little about the r s, because for the rat position habits are apt to be strong. We should, however, need also to know about the other r s: is the rat perceiving difference of brightness or of shape at all under the given conditions, or is he ignoring them ($r = 0$)? We should not have disparity unless we could use different reactions, one for a difference of brightness and another for a difference of shape, and even then we should be likely to put some other r into the sum, because of other communalities—like the relation of hunger to jumping from a jumping stand.

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EDWIN G. BORING

THE INTERPRETATION OF EBBINGHAUS'S RETENTION VALUES

In contemporary studies of retention and in textbook treatments of it the question of the absolute amount retained at successive intervals is of secondary importance to that of the relative amount. This attitude is justified because experimentation has shown that the absolute amount retained is influenced by the conditions of learning and by the methods of measurement. Authors writing on this subject have, nevertheless, frequently offered explanations for the differences in the absolute amounts retained in the various experiments. So far as the writer is aware, these explanations have never been brought together but are scattered widely throughout the literature. It seemed worth while, therefore, to collect them and to present them in one place.

Some of these explanations are pertinent but others are in error and of questionable validity. One explanation that we give here is new; it has apparently escaped the attention of earlier writers. All of the explanations given here are concerned with explaining the differences between the results of Ebbinghaus's pioneering experiment¹ and the one conducted by Radossawljewitsch.²

Radossawljewitsch's retention values at all time-intervals were greater than Ebbinghaus's. One explanation of this difference is over-learning. Bills,³ for example, writes that Ebbinghaus learned to but one perfect repetition whereas Radossawljewitsch's Ss learned to two. This explanation is, however, in error as van Ormer and Dallenbach have pointed out.⁴ Radossawljewitsch's Ss learned to two perfect repetitions⁵ and so also did Ebbinghaus.⁶ This interpretation of the differences in absolute retention, which appears widely in the literature, is slowly being dropped though it still is given often enough to be confusing.

A second explanation, offered first by Finkenbinder, is concerned with the method of learning. Radossawljewitsch, according to Finkenbinder, introduced "the factor of accentuated rhythm into the act of learning the syllables"⁷—the inference being that Ebbinghaus did not. As a matter of fact Ebbinghaus did read his lists rhythmically,⁸ hence this explanation is also fallacious.

A third explanation concerns the number of Ss employed in the two studies. Radossawljewitsch used altogether 29 Ss and Ebbinghaus was his own S. The difference between the results of their studies may be due, it has been suggested, to the individual differences between Ebbinghaus and Radossawljewitsch's Ss.⁹ Though Radossawljewitsch used 29 Ss in his study, the greatest number employed at any time-interval was 16, and for the longest time-interval, 120 days, he used but 4. In any event, this 'explanation,' to have point, must mean that Ebbinghaus possessed a poorer memory than the average of Radossawljewitsch's Ss.

A fourth explanation deals with the Ss' relative amounts of practice in the two experiments. It is held that Radossawljewitsch's Ss were relatively unpracticed as compared with Ebbinghaus. The argument then is that since the former were relatively unpracticed at such learning tasks they probably learned the material better and hence would be expected to exhibit better retention.¹⁰ A strange argument! It may be granted that Radossawljewitsch's Ss were relatively unpracticed. The fact remains, however, that they were given preliminary practice of from 1 to 5 days during which they learned different groups of 3 lists of syllables.¹¹ Whether these Ss learned the material better during the experiments than did Ebbinghaus cannot

¹ Hermann Ebbinghaus, *Memory* (trans. by H. A. Ruger and C. E. Bussenius), 1913, 1-123.

² P. R. Radossawljewitsch, *Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen*, 1907, 1-197.

³ A. G. Bills, *General Experimental Psychology*, 1934, 295.

⁴ E. B. van Ormer and K. M. Dallenbach, A frequent error concerning Ebbinghaus' experiments on oblivescence, this JOURNAL, 43, 1931, 706-707.

⁵ Radossawljewitsch, *op. cit.*, 15.

⁶ Ebbinghaus, *op. cit.*, 65.

⁷ E. O. Finkenbinder, The curve of forgetting, this JOURNAL, 24, 1913, 11.

⁸ Ebbinghaus, *op. cit.*, 25.

⁹ Bills, *op. cit.*, 295; J. B. Stroud, *Introduction to General Psychology*, 1938, 544.

¹⁰ H. E. Garrett, *Great Experiments in Psychology*, 1930, 65.

¹¹ Radossawljewitsch, *op. cit.*, 17.

be told surely. Ebbinghaus used the method of complete presentation with attempted recall at irregular intervals—"There was a perfectly free interchange between the reading and the occasionally necessary tests of the capacity to reproduce by heart."¹² In some respects Radossawljewitsch's procedure was similar. He employed a memory drum driven at a constant speed with free recall instead of the method of anticipation. His Ss allowed an attempted recall whenever they felt they could do so. If they failed to give two consecutive errorless recalls they continued learning. Thus an unknown amount of practice took place before the Ss were credited with having reached the criterion.¹³ This means that in both experiments some amount of practice took place over that to be inferred from the objective records. Then too Ebbinghaus used the first syllable as a cue, which means that he actually learned but 12 syllables, and this may have influenced the degree of learning. Radossawljewitsch's Ss learned lists of 8, of 12, and of 16 syllables whose results were averaged in comparison with Ebbinghaus who learned 8 lists of a single length. Just what this difference in methods of conducting the experiments would mean for the suggested difference in degree of learning is impossible to say. Perhaps the degree of learning did differ in the two experiments but this cannot be said certainly from their publications.

There is a fifth explanation dealing with a difference in the methods of the two experiments which may be given. The methodological feature has been mentioned by several writers but it has not been used as it might to account for the differences in the amounts recalled. Neither has this feature been applied in explanation of the differences between Ebbinghaus and more recent investigators. The explanation is based on the fact that different numbers of lists were learned at a single sitting in the two experiments. Ebbinghaus learned 8 lists at a sitting. These 8 were learned one after the other with a pause of 15 sec. between lists for recording results.¹⁴ These 8 lists thus constituted the unit of work for any one interval. The time required to relearn all 8 lists was used as the relearning time. This relearning time was compared with the time required to learn all 8 originally to obtain the saving scores. Under such conditions it is thinkable that part of the forgetting was due not to the mere passage of time as Ebbinghaus thought but to the fact of inhibitions engendered between these several lists. (It may be remarked parenthetically that the retention intervals were measured "from the completion of the first set of first learnings." This meant in the case of the so-called 20-min. interval that "the interval as a whole is so short that the relearning of the first series of a test followed almost immediately or after an interval of one or two minutes upon the learning of the last series of the same test."¹⁵ Radossawljewitsch's Ss, on the other hand learned 3 lists at a sitting instead of 8. These likewise were learned one after the other with an unspecified time-interval between them. Woodworth does say that "Radossawljewitsch on the contrary gave a rest of 3-4 min. after each list, and it is not surprising that his Os forgot more slowly than Ebbinghaus."¹⁶ On this point, however, Radossawljewitsch says the following: "Die Dauer der Ruhepausen,

¹² Ebbinghaus, *op. cit.*, 24.

¹³ Radossawljewitsch, *op. cit.*, 15, 24.

¹⁴ Ebbinghaus, *op. cit.*, 25.

¹⁵ Ebbinghaus, *op. cit.*, 66.

¹⁶ R. S. Woodworth, *Experimental Psychology*, 1938, 57.

zwischen der in einer und derselben Sitzung zu wiedererlernenden und erlernenden Silbenreihen betrug nicht so viel wie bei Ebbinghaus und entsprach auch nicht den Müller-Schumann'schen Versuchen. Die Ruhepausen dauerten etwa 3-4 Minuten."¹⁷

Though the meaning of this statement is somewhat ambiguous, if we compare it with Ebbinghaus's statement given above it seems clear that the 3-4 min. interval occurred between the relearning of the old lists as a group and the learning of the new lists as another group. If this be true then the more rapid forgetting shown by Ebbinghaus's data cannot be explained as a result of this particular difference in time interval. Instead the assumption of a greater amount of inter-list inhibition for Ebbinghaus's 8 lists than for Radossawljewitsch's 3 would seem to be supported. This particular feature of Ebbinghaus's method would likewise account for the differences in absolute amounts retained between his and more recent studies of the curve of retention.

This hypothesis is in harmony with what we now know of retroactive inhibition although it has not been subjected to experiment. In a preliminary investigation it has been tested for the 20-min. interval as measured in Ebbinghaus's study. A single list of 12 syllables, a triple (XXX) X-mark was used as the cue symbol, was learned and then relearned after an interval of 20 min. On alternate days a group of 4 lists of the same length and association value were learned. The 20-min. interval in these cases was measured from the completion of the learning of the first list. Twenty minutes from that time, during which the remaining three lists were learned, all 4 lists were relearned in order. Learning and relearning by the anticipation method were carried to a criterion of 2 errorless presentations. The syllables were pronounced, after a fashion, and not spelled. Six single lists were learned and 5 groups of 4 lists each in the test. The saving scores for presentations were 83 and 47, for errors 93 and 25, for the single lists and groups of 4 lists respectively.¹⁸ These results would support the hypothesis. Whether analogous results would be found for longer intervals is something to be settled by additional experiments. Van Ormer's study,¹⁹ for example, shows the same retention value after one hour as did Ebbinghaus in spite of the fact that van Ormer's Ss learned 3 lists only. It may be that the inhibiting effect of increasing numbers of lists decreases for longer intervals or that the inhibiting effect is somehow related to the number of such lists learned originally.

University of Arkansas

R. H. WATERS

BLACK AND WHITE

Dr. Deane B. Judd has raised again the question of the psychological nature of black and white.¹ He proposes two sets of terms for the so called "achromatic colors" according to whether they appear under the perceptual mode of "surface" or that of "aperture" ("film"). His definitions are as follows: "Lightness [is] the

¹⁷ Radossawljewitsch, *op. cit.*, 17.

¹⁸ This study was conducted by Homer G. Wood, graduate assistant, who was both experimenter and subject.

¹⁹ Van Ormer, Retention after intervals of sleep and waking, *Arch. Psychol.*, 1932, (no. 137), 42.

¹ D. B. Judd, Hue, saturation, and lightness of surface colors with chromatic illumination, *J. Opt. Soc. Amer.*, 30, 1940, 2-32.

attribute of any surface color which permits it to be classed as equivalent to some member of the series of grays ranging between *black* and *white*; "brightness [is] the attribute of any aperture color which permits it to be classed as equivalent to some member of the series of achromatic aperture colors ranging between *very dim* and *very bright*."² Surface colors, then, are said to have the attributes, hue, saturation, and lightness, while aperture colors have the attributes, hue, saturation, and brightness. "Lightness" is to be expressed in terms of black, gray and white; "Brightness," as more or less dim or bright.

Dr. Judd's definitions imply that "black," "gray," and "white" are perceptual terms referable only to the surface mode of appearance. The alternate terms "*very dim*" to "*very bright*" which he would apply to "aperture" colors would make that mode of appearance basically intensive and referable to an illuminating source. These implications have been suggested before in the literature of color as Dr. Judd points out.³ The position has been adequately criticized and the counter arguments stated by Titchener in his answer to Ward's contention that "darkness is simply the zero intensity of the sensible quality 'light'";⁴ The arguments need not be repeated but the conclusion may be restated. "The positive reasons for attributing a sensory character to the blacks . . . are to be found in all the elementary phenomena of vision. . . . They are also phenomena which may be observed, not only by help of black 'body-colors,' but also when these are replaced by the darkness of an unlit space, such as the interior of a black-walled tube; there is no ground for separating the blacks from the darks. In short, black or dark, so far as we are dealing with sheer looks, is no more the zero-point of white than blue is the zero-point of yellow; and when we proceed to experiment, black or dark behaves just as other acknowledged qualities behave. I can not see that further psychological evidence is necessary."⁵ The same conclusion holds for the sensory character of "white." Thus the "achromatic series" becomes for psychology two strictly qualitative series gray to white and gray to black. Gates has demonstrated experimentally that "intensive" judgments in the achromatic series are not fundamentally intensive but are judgments of perceptual "insistence" (*Eindringlichkeit*) based upon qualitative differences.⁶

G. E. Müller⁷ and Titchener⁸ have discussed the consequences of this non-intensive character of vision. Their conclusion that vision is fundamentally qualitative, indicates that the distinctions Judd proposes are perceptual rather than attributive. Judd has, indeed, made his distinctions both perceptual and attributive. Such an identification of attribute with perceptual mode seems confusing and gratuitous, or else it makes the distinction a mere matter of a preference in the choice

² *Op. cit.*, 3 f.

³ Judd, Note on the definition of black and white, *infra*.

⁴ E. B. Titchener, A note on the sensory character of black, *J. Philos. Psychol., & Sci. Methods*, 13, 1916, 113.

⁵ Titchener, *op. cit.*, 120.

⁶ E. J. Gates, On intensive and qualitative judgments of light sensations, this JOURNAL, 26, 1915, 296-299.

⁷ G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Zsch. f. Psych. u. Physiol. d. Sinnesorg.*, 10, 1896, 31; 14, 1897, 60 ff.; Über die Farbenempfindungen, 1930, 22-27.

⁸ Titchener, Visual intensity, this JOURNAL, 34, 1923, 310 f.

of words. Titchener pointed out in his answer to Ward that "black does not reduce to dark, nor dark to black; but both superficial black and roomy dark reduce to an "areal quality."⁹ The "areal quality" is the "film" color of Katz¹⁰ and it is our contention that Judd's definition of the conditions for black and white should refer to this "areal quality" or "film color" because it is the mode of appearance that shows the least perceptual complication. Then we should have all of our experimental conditions, physical and psychological, reduced to their simplest terms and capable of more adequate standardization. There seems to me no impediment to applying Judd's specifications of the stimulus for surface black and white to aperture colors inasmuch as his "Assumption (a)" states that "if the aperture colors derived from two matt, opaque surfaces are identical (in hue, brightness, and saturation) and the surfaces are compared under the same illumination and viewing conditions, it is assumed that the surface colors will be identical; and, conversely, if the aperture colors are different, the surface colors will be different."¹¹ Judd has thus accepted Katz' fundamental observations that all modes of color appearance can be reduced, so far as their color is concerned, to "areal colors" or "film colors" (Flächenfarbe).

The further difficulty seems to be one of observational set and the consequent definition of terms. Judd has preferred the term "aperture" to "film" apparently because the former indicates certain aspects of the conditions which give that perceptual mode. On the other hand, the term "film color" as it is used by Katz and by Martin¹² is not intended to refer to the object source of the color, but to the color itself which has area without defined distance or depth. "Film color" may be obtained from surfaces, transparent objects, and primary sources, but because of its perceptual reduction it is not prejudiced by its object of origin. "Aperture," on the other hand contains the implication of a primary source since "black" which stands at a zero of primary source can not, according to Judd, appear in an aperture. This conclusion is logical, however, rather than psychological. Black can be observed as a "film color" and the surfaces of black objects appear blacker as they lose "hardness" of surface and becomes less sharply localized, or "softer." The best black "surface" is obtained with velvet, and even that is "less black" than, though comparable with, the color of an opening into a lightless chamber.

There seems to be no reason, then for restricting the application of Judd's equation (1), $(L = A(A_r + 1) / (A_r + A))$, to "surface colors."¹³ Under the ordinary conditions for observing "film" or "aperture" colors with a reduction screen, A_r becomes the luminous apparent reflectance of the reduction screen and A the luminous apparent reflectance of the reduced surface. The equation holds only if the illuminations upon A and A_r are identical. If such is not the case or if the "film" or "aperture" color is obtained from a primary source, the ratio which determines the whiteness or blackness of the color will be between the luminance of the object and the luminance of the screen (or the field) under a given illumination. The value of the

⁹ Titchener, A further word on black, *J. Philos. Psychol., & Sci. Methods*, 13, 1916, 650.

¹⁰ David Katz, *Die Erscheinungsweisen der Farben*, 1911, 36 f.

¹¹ Judd, *op. cit.*, 5.

¹² M. F. Martin, Film surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 451-480.

¹³ Judd, A note on the definition of black and white, *infra*.

ratio will change, of course, with the illumination of the field as well as with the luminance in the aperture. Whether we call the colors specified by the two ratios, (reflectances and luminances,) "brightnesses," or "lightnesses," or "white, gray and black" is immaterial since A and A_f under a specified illumination can be stated as luminances,¹⁴ and aperture matches and surface matches are mutually reducible.¹⁵

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FORREST LEE DIMMICK

THE DEFINITION OF BLACK AND WHITE

Professor Dimmick courteously submitted to me in advance of publication a copy of the preceding note. I have consequently an opportunity for an immediate reply. My views on black and white have accordingly been written out more completely than before and I have in turn submitted them to him for a rejoinder.

Tentative definition. Black and white are the two extremes of the series of achromatic surface colors, of zero glossiness and transparency, which differ in lightness alone.

Sufficient conditions for appearance, illumination uniform. A surface color is color perceived as belonging to a surface. If an opaque object be viewed among other objects in a space sufficiently illuminated to provide photopic vision, the normal observer (O) will generally perceive that it is an object with surfaces possessing color.¹ If it be granted that the achromatic color seen by O takes on the surface mode of appearance, the following formula for lightness, L , of the surface color, derived from a formula by Adams and Cobb,² serves to describe stimulus conditions for black and white, in which, for $L = 0$, the color is black, and for $L = 1$, the color is white.³

$$L = A(A_f + 1)/(A_f + A), A_f > 0 \dots \dots \dots [1]$$

where: A = luminous apparent reflectance of the surface under consideration,⁴ and A_f = luminous apparent reflectance of the field, an average of the luminous apparent reflectances of the surfaces in the field, weighted according to proximity in space and in past time to the fixation point.⁵ The surfaces must be, and be per-

¹⁴ Judd, Hue, saturation and lightness of surface colors with chromatic illumination, *J. Opt. Soc. Amer.*, 30, 1940, 4.

¹⁵ Judd, *ibid.*, 5.

¹ Factors tending to prevent such perception are objects providing visual fields insufficiently elaborated in gross structure, or unsupplied with visible microstructure. Cf. R. B. MacLeod, An experimental investigation of brightness constancy, *Arch. Psychol.*, 21, 1932, (no. 135), 1-101.

² E. Q. Adams and P. W. Cobb, The effect on foveal vision of bright (and dark) surroundings, *J. Exper. Psychol.*, 5, 1922, 41.

³ No case of failure of these conditions to produce black and white colors is known; other conditions have, however, been known to produce black and white colors, and these will be discussed presently.

⁴ H. J. McNicholas, Absolute methods in reflectometry, *Bur. Stand. J. Res.*, 1, 1928, 33.

⁵ D. B. Judd, Hue, saturation and lightness of surface colors with chromatic illumination, *J. Opt. Soc. Amer.*, 30, 1940, 4; also *J. Res. Nat. Bur. Stand.*, 24, 1940, 296.

ceived as if, uniformly illuminated. A_f must be greater than zero because for $A_f = 0$ there exists no illumination which will yield photopic vision.

It will be noted that the only way for a surface to yield a black color according to Equation [1] is to have a luminous apparent reflectance $A = 0$; and the only way for it to yield a white color is to have $A = 1$. Intermediate values of A correspond to surface colors of lightness intermediate between 0 and 1; values of A higher than 1 correspond to glossy surface colors; and there is, of course, no surface for which A is less than zero.

It is interesting to note how lightness by [1] varies for $A = A_f$ as both approach zero. For $A = A_f$, we have:

$$L = (A + 1)/2, A > 0. \dots \dots \dots [1a]$$

which approaches 0.5 as A approaches zero. The interpretation is that a surface of luminous apparent reflectance the same as that of the field of which it forms a part always yields a lightness higher than 0.5; and if it is a gray, it approaches middle gray through lighter grays as the luminous apparent reflectance of the field approaches zero.

This interpretation bears a superficial resemblance to the Hering mid-gray corresponding to the intrinsic light of the retina.⁶ If A be taken equal to zero, however, instead of equal to A_f , the value of lightness by Equation [1] is always zero. This result bears a superficial resemblance to the Ladd-Franklin view that black is "the psychical correlate of a cortical condition of inactivity in correspondence to a non-stimulated retinal area."⁷ The Ladd-Franklin view has been shown to be inadequate by Knox.⁸ Equation [1] does not really suggest either the Hering or the Ladd-Franklin view, first because it refers to photopic vision only, and secondly because analysis shows that the limiting value of lightness varies according to Equation [1] between 0 and 1 depending on the ratio of A to A_f as they approach zero. The requirement in Equation [1] that A_f be greater than zero suggests on the contrary that perception of neither black nor white nor, indeed, of a surface color of any intermediate lightness can result from zero stimulus. This impossibility should be related to the difficulty of obtaining the perception of a surface from a field which does not supply sufficient light to the observer to yield photopic vision, and to the impossibility of obtaining surface perception from a field supplying no light whatever.⁹

Necessary and sufficient conditions for surfaces, illumination uniform. We have said that Equation [1] describes sufficient conditions for the appearance of black or white in a uniformly illuminated scene; that is, whenever $A = 1$, the corresponding surface color, if achromatic, matte, and opaque, is always reported as white, and whenever $A = 0$ (A_f sufficiently greater than zero to yield photopic vision), the

⁶ E. Hering, *Grundzüge der Lehre vom Lichtsinn*, 1920, 214.

⁷ M. R. Neifeld, The Ladd-Franklin theory of the black-sensation, *Psychol. Rev.*, 31, 1924, 498-502, also C. Ladd-Franklin, *Colour and Colour Theories*, 1929, 241-246.

⁸ G. W. Knox, A contradiction to the Ladd-Franklin theory of blackness, *J. Psychol.*, 8, 1939, 13-21.

⁹ J. Ward, Is 'black' a sensation?, *Brit. J. Psychol.*, 1, 1905, 408.

corresponding surface color is always reported as black.¹⁰ If, however, there are no surfaces in the field having luminous apparent reflectances as high as 1 or as low as 0, it is common, though not inevitable, for an *O* to report black or white for the terminal members of the series of achromatic colors actually experienced in the scene, provided the maximum luminous apparent reflectance, A_z , of any surface present in the field, at the time of observation or not too much previously, exceed the minimum, A_n , by a factor sufficiently large (probably between 10 and 100 for most *O*s). It is possible to expand Equation [1] into a more general form which embraces these common conditions for obtaining black and white. This general statement of the stimulus conditions for a given lightness yields the necessary and sufficient conditions for the appearance of black and white colors perceived as belonging to uniformly illuminated surfaces in accord with the tentative definition given,

$$L = \frac{(A - A_n)(A_f + A_z)}{(A_z - A_n)(A_f + A)}, \quad A_f > 0, 0 < A_z \leq 1, A_n < kA_z \dots [2]$$

where k is a constant, perhaps characteristic of every *O* probably contained within the limits of 0.10 and 0.01. Knox has suggested the value of $1/60$ for k ,¹¹ basing his suggestion on the work of Gelb, Koffka, and Harrower.¹²

Examination of Equation [2] shows that it is in accord with the tentative definition of black and white given at the beginning of this note. The only way for L to assume the value of zero (corresponding to black) is to have the surface take on the luminous apparent reflectance, A_n , that is, to be the darkest surface in the field of view. Similarly white, $L = 1$, corresponds to the lightest matte, opaque surface color in the field of view provided the luminous apparent reflectance, A_z , of the corresponding surface exceeds A_n by the factor of more than $1/k$; that is, provided $A_n < kA_z$.

Equation [1] becomes the special case of Equation [2] obtained by setting $A_n = 0$ and $A_z = 1$. Another special case of Equation [2], that obtained by setting $A_n = 0.03$ and $A_z = 1$, has been compared with the visual estimates of 8 *O*s.¹³ The reports of some of these *O*s would have agreed better with Equation [2] if the value of A_z had been set closer to that actually fulfilled by the experimental conditions ($A_z = 0.80$); that is, some *O*s called surfaces white which had luminous apparent reflectances about equal to 0.80; others called them light gray.

¹⁰ If the surface does not give rise to a surface color, but, as is frequently the case for $A = 0$, to an aperture color, the descriptive term, black, is less likely to be used; in fact it is exceptional. Thus, E. B. Titchener says, "The distinction drawn in everyday life between black and dark, so far as it is drawn at all, seems rather to suggest the psychological distinction of superficial and roomy colors; black is superficial, like the color of colored paper; dark is roomy, like the color of a transparent liquid. (A further word on black, *J. Philos., Psychol., & Sci. Methods*, 13, 1916, 649.)"

¹¹ Knox, *op. cit.*, 15.

¹² Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 251-252.

¹³ Judd, *op. cit.*, *J. Opt. Soc. Amer.*, 30, 1940, 24; also *J. Research Nat. Bur. Stand.*, 24, 1940, 319.

Scenes involving surfaces perceived as unequally illuminated. A normal O in an unknown room filled with unknown objects illuminated by an unknown light is nevertheless able to report on the amount and approximate direction of the maximum illumination and, therefore, upon the amount of illumination pertaining to surfaces in various orientations relative to this direction. The lightness of the colors of such surfaces is primarily dependent upon the impression of illumination pertaining to them. The factors entering into the impression of illumination are complicated and are probably too poorly understood to justify an immediate attempt at formulation (compare Katz,¹⁴ Koffka,¹⁵ Pikler,¹⁶ and Bühler¹⁷).

Equation [2] succeeds without specific reference to amount of illumination because in the simple case of a number of surfaces perceived as equally illuminated the average brightness of the scene (total insistence according to Katz¹⁸) is almost the sole determinant of the impression of illumination. The average brightness of the scene is not mentioned explicitly in Equation [2], but it is measured by the average luminous apparent reflectance of the field, A_f . This principle has been stated by Kirschmann,¹⁹ Von Kries,²⁰ Bocksch,²¹ Kohlrausch,²² and Kardos.²³ Attention should be drawn to an aspect of the definition of A_f (and A_n and A_s also) which may be overlooked; this definition refers to surfaces which have been in the scene before observation as well as those at the time of observation. Often surfaces previously but no longer present in the field have supplied the information upon which the impression of illumination depends. In such cases the O s are sometimes said to have a mental criterion or standard of white which is subject to complicated instantaneous changes. Equation [2] shows the pertinent variables, and its hyperbolic form essentially approximates the true relation; but there is no guarantee that a solution can be found for every O even for the simple case of colors of surfaces perceived in uniform illumination, because it is frequently impossible to evaluate A_n and A_s for a given O or to know how to weight the various surfaces in computing the average luminous apparent reflectance of the field, A_f . It is probable that the most important considerations bearing on such weighting relate to retinal adaptation. Values of A_n and A_s , on the other hand, may refer to surfaces which have been presented in the scene too far in the past to bear at all

¹⁴ D. Katz, *Der Aufbau der Farbwelt*, 1930, 460 ff.

¹⁵ Koffka, *op. cit.*, 258 ff.

¹⁶ J. Pikler, Das Augenhüllenlicht als Mass der Farben, *Zsch. f. Psychol.*, 120, 1931, 189; 125, 1932, 90.

¹⁷ K. Bühler, *Die Erscheinungsweisen der Farben*, 1922, 1-209.

¹⁸ Katz, *op. cit.*, 462.

¹⁹ A. Kirschmann, Die psychologisch-ästhetische Bedeutung des Licht und Farbenkontrastes, *Philos. Stud.*, 7, 1891, 363.

²⁰ J. von Kries, Die Gesichtsempfindungen, in W. A. Nagel's *Handb. d. Physiol. des Menschen*, 3, 1905, 239.

²¹ H. Bocksch, Duplizitätstheorie und Farbenkonstanz, *Zsch. f. Psychol.*, 102, 1927, 343.

²² A. Kohlrausch, Allgemeines über Umstimmung und 'Farbenkonstanz der Sehdinge,' in A. Bethe's *Handb. d. norm. u. pathol. Physiol.* 12/2, Receptionsorgane II, 1931, 1501.

²³ L. Kardos, Versuch einer mathematischen Analyse von Gesetzen der Farbensehens. Nähere Bestimmung des funktionalen Verhältnisses zwischen Farbenerlebnis und Reizgesamtheit, *Zsch. f. Sinnesphysiol.*, 66, 1935, 188.

importantly on retinal adaptation. For very marked local variations in illumination, such as provided by a spotlight in exact registration with a surface, the impression of the illumination according to Gelb is determined by the surfaces actually present.²⁴ For such cases, at least, there is some hope of writing a definition of lightness by extension of Equation [2].

Black or white from nonsurface modes of appearance. This note started with a tentative definition of black and white as qualities of surface color; as far as can be ascertained, no serious attempt has ever been made to deny that surface colors may have these qualities. It is now pertinent to inquire whether black and white are commonly or ever experienced as qualities of color in other modes of appearance. That is, can there be a black or white volume color, a black or white film (aperture) color, or a black or white luminous color?

From casual observation it seems safe to conclude that volumic blacks and whites are exceptional but not impossible, that some trained *Os* experience black and white film colors while naïve *Os* commonly experience them only as dim or bright in various degrees, and that a white luminous color is similarly possible; but a black luminous color is self-contradictory. I venture to suggest without being able to prove it that these reports for other modes of appearance are derived from the surface mode which characteristically yields the black and white quality. Other writers have adopted similar views.²⁵ Helmholtz has asserted that black and white in common parlance refer only to the colors of objects.²⁶ Troland has stated, "Thus, good blacks and other dark colors, such as browns and olives, are found only as surface colors or in a surface color environment."²⁷ Neifeld, even in explaining the Ladd-Franklin view that black is a sensation, states, "Black is the psychic correlate of the absorption, *by objects* (italics mine), of all the visible light-ray frequencies."²⁸

Others have adopted the divergent view that it is not only possible but useful to abstract from these different kinds of percepts the black and white quality. They assert that there is a black sensation and a white sensation.²⁹ It would be impossible within a reasonable space to review the various arguments that have been

²⁴ Adhémar Gelb, Die 'Farbenkonstanz' der Sehdinge, in A. Bethe's *Handb. d. norm. u. patol. Physiol.*, 12/1, Receptionsorgane, II, 1929, 674.

²⁵ A. Fick, Gesichtssinn, in L. Hermann's *Handb. d. Physiol. d. Sinnesorgane*, 1879, 204 f.; A. Kirschmann, Der Metallglanz und die Farbe der Metalle, *Arch. f. d. ges. Psychol.*, 41, 1921, 94; J. Ward, Is 'black' a sensation?, *Brit. J. Psychol.*, 1, 1905; A further note on the sensory character of black, *Brit. J. Psychol.*, 8, 1916, 212; K. Fiedler, Das Schwarz-Weiss Problem, *Neue Psychol. Stud.*, 2, 1926, 343-408.

²⁶ H. v. Helmholtz, *Physiological Optics*, Eng. trans., 2, 1924, 130.

²⁷ L. T. Troland, Visual phenomena and their stimulus correlations, in C. Murchisons' *Handbook of General Experimental Psychology*, 1934, 657.

²⁸ M. R. Neifeld, The Ladd-Franklin theory of the black-sensation, *Psychol. Rev.*, 31, 1924, 498; also C. Ladd-Franklin, *Colour and Colour Theories*, 1929, 241.

²⁹ E. Hering, *Zur Lehre vom Lichtsinn*, 1874, 88. G. E. Müller, Über die Farbenempfindungen, *Zsch. f. Psychol.*, 17, 1930, 16 f. E. B. Titchener, A note on the sensory character of black, *J. Phil. Psychol. and Sci. Methods*, 13, 1916, 113; A further word on black, *J. Phil. Psychol. and Sci. Methods*, 13, 1916, 649; Visual intensity, this JOURNAL 34, 1923, 310. C. Stumpf, Die Attribute der Gesichtsempfindungen, *Abb. d. kgl. preuss. Akad. d. Wiss., phil.-hist. Klass.*, 8, 1917; see also this JOURNAL, 32, 1921, 155. W. Wundt, *Grundzüge der physiologischen Psychologie*, 5th ed., 2, 1905, 147-193. A. Tschermak, Licht und Farbensinn, in A. Bethe's *Handb. d. norm. u. patol. Physiol.*, 12/1, Receptionsorgane II, 1929, 296. H.

framed, and it would be difficult to add anything appreciable to them. It is interesting to note, however, that Titchener,³⁰ having reviewed the evidence and taken his stand, says that we would all welcome a crucial experiment.

It would be worthwhile, I believe, to study the methods of describing color as a function of the mode of appearance, and to search for pertinent stimulus-variables and functions of them correlating with reports of the colors by direct visual estimation. Such a study would constitute a whole series of crucial experiments. Let all abstractions, including those of black and white sensations, be tested for usefulness in color description for all modes of appearance. On this account, we should watch with particular interest the further development of Professor Dimmick's proposal to specify color in all modes of appearance by the seven primaries: red, yellow, green, blue, white, gray, and black, taken four at a time, two chromatic and two achromatic.³¹ In the meantime, the tentative definition of black and white with which this note was introduced may as well be taken. Its usefulness is exemplified by the psychophysical relation given in Equation [2], and its validity has not been questioned by anyone. Whether it be possible to extend with similar validity and usefulness the definition of black and white to modes of appearance other than surface remains to be decided by further study.

National Bureau of Standards
Washington, D.C.

DEANE B. JUDD

A REJOINDER

Due to the courtesy of Dr. Judd and of the editors of the JOURNAL, I am privileged to append a rejoinder to Dr. Judd's reply to my note. A rejoinder, however, is hardly necessary as I am in agreement with most of Dr. Judd's facts. Dr. Judd is concerned with the specifications of the physical conditions sufficient to obtain a perception of a 'black,' 'gray,' or 'white' surface; I am concerned with the visual qualities, 'black,' 'gray,' and 'white' regardless of the perceptual mode under which they appear. It seems to me unfortunate to confuse the two concepts.

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FORREST LEE DIMMICK

Educational and Psychological Measurement

The first number of *Educational and Psychological Measurement* (Vol. 1, no. 1), dated, January, 1941, has been received. It is published by Science Research Associates, 1700 Prairie Ave., Chicago, Illinois, and is edited by G. Frederic Kuder, of the Social Security Board, and a board of three assistant and fifteen coöperating editors. The new journal was founded in the interest of measurement and its pages are "open to reports of research on the development and use of tests and measurements in education, government, and industry; to descriptions of testing programs;

Bocksch, Duplizitätstheorie und Farbenkonstanz, *Zsch. f. Psychol.*, 102, 1927, 343 ff, 430. G. M. Michaels, Black: A non-light sensation, *Psychol. Rev.*, 32, 1925, 248. M. R. Neifeld, The Ladd-Franklin theory of the black-sensation, *Psychol. Rev.*, 31, 1924, 498-502; also C. Ladd-Franklin, *Colour and Colour Theories*, 1929, 241-246. F. L. Dimmick, The series of blacks, grays, and whites, *Psychol. Rev.*, 32, 1925, 334; A reinterpretation of the color-pyramid, *Psychol. Rev.*, 36, 1929, 83; Black and white, this JOURNAL, *supra*, 286-289; F. L. Dimmick and C. H. Holt, Gray and the color pyramid, this JOURNAL, 39, 1928, 284.

³⁰ Titchener, A note on the sensory character of black, *J. Philos.*, 13, 1916, 120.

³¹ F. L. Dimmick and M. R. Hubbard, The spectral location of psychologically unique yellow, green, and blue, this JOURNAL, 52, 1939, 242-254; The spectral components of psychologically unique red, *ibid.*, 52, 1939, 348-353.

to discussions of problems of measurement; and to miscellaneous notes pertinent to the measurement field."

The present number contains eight articles (The evaluation of vocational and educational counseling; a critique of the methodology of experiments, by E. G. Williamson and E. S. Bordin; The logic of age-scales, by M. W. Richardson; Counseling on the basis of interest measurement, by John G. Darley; The course in self-appraisal and careers offered to seniors in the Chicago Public High Schools, by Grace Munson; Primary mental abilities and aviation maintenance courses, by Willard Harrell and Richard Faubion; A comparison of the original and revised Stanford-Binet intelligence scales, by Martin L. Reymert and Ralph K. Meister; The prediction of scholastic success in a college of medicine, by Dewey B. Struit; A comparative study of freshman-week tests given at the University of Chicago, by William M. Shanner and G. Frederic Kuder); a note on a simplified method of computing test reliability by C. J. Hoyt; a section of abstracts; and a section of notes and news.

The new journal, its editors hope, will facilitate the interchange of ideas and techniques in measurement. The first issue promises well for the future numbers.

K. M. D.

THE NEW BRUNSWICK MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The thirty-seventh annual meeting of the Society of Experimental Psychologists was held on Thursday and Friday, March 27 and 28, at Rutgers University, New Brunswick, New Jersey, under the chairmanship of Professor Carroll C. Pratt. It was attended by 30 members—the largest attendance since the reorganization of the Society in 1929—from 19 institutions, as follows: Brown (Graham, Hunter, Schlosberg); Columbia (Poffenberger, Woodworth); Cornell (Dallenbach, Weld); Harvard (Beebe-Center, Boring, Lashley, Stevens); Iowa (McGeoch); New York Psychiatric Institute (Landis); New School of Social Research (Wertheimer); Pennsylvania (Fernberger); Princeton (Bray, Langfeld, Wever); Rochester (Culler); Rutgers (Pratt); Smith (Gibson, Koffka); Stanford (Hilgard); Swarthmore (Köhler); Tufts (Carmichael); Vassar (Lanier); Virginia (Geldard); Wesleyan (Wendt); Yale (Hull, Yerkes). In addition to these members, the scientific meetings were attended by a number of guests invited by the Chairman.

On Thursday evening, at a banquet given the Society by Rutgers University, Dean Walter T. Marvin gave the address of welcome and Professor Robert M. Yerkes reminisced regarding the early days of the Society, telling in particular of the 1917 meeting at Harvard, the final session of which was held on the day America entered World War I. He related how the group turned immediately upon the reception of the news of the declaration of war to consider what American psychology could do to assist the Government in the emergency and how from that small beginning the psychological services in the Army developed. Following these addresses, Chairman Pratt presented the Howard Crosby Warren Medal for "distinguished research" to Dr. Clarence H. Graham, associate professor of psychology at Brown University, for his outstanding research in the psychology of vision.

The 1942 meeting of the Society is to be held at the University of Virginia at a date to be announced later. Professor Frank A. Geldard was elected chairman for the ensuing year, and Professor Samuel W. Fernberger was reelected secretary-treasurer for another three-year term.

K. M. D.

Edouard Claparède: 1873-1940

Professor Claparède, who died in Geneva on September 29, 1940,¹ was a man whose personal history and professional career were thoroughly integrated: his scientific work clearly reflected his personality, just as his everyday orientation was essentially scientific.

Son of a Genevan preacher, born on March 24, 1873, he was the youngest of five children. Since the nearest sibling was eight years his senior, he was considered "the little one"—a fact which, on his own view, had clear Adlerian implications for the development of his personality. His early cravings for positions of authority were, however, shortlived, for they went counter to his basically mild and sweet temperament. More lasting was his cumulative 'reaction formation'—a life-long perseverance against dogmatic authority of any sort, intellectual or practical. Likely constitutional and familial factors were reënforced here by the traditions of hardy refugee ancestors who, after the revocation of the Edict of Nantes, emigrated to the *Suisse Romande* from the French *Midi*. The Huguenot spirit of liberal and pragmatic Protestantism, vividly identified by him with the principles and method of free inquiry, remained an all-pervasive life-force in his professional and social activities in the city of Calvin and Rousseau. Being born and living all his life "in the same old house in Champel" in which some of his ancestors also had lived, he gained from the continuity a powerful spiritual foothold for his humbly proud sense of intellectual autonomy. His financial independence, too, (though also a source of overmodest feelings of inadequacy about achievement in proportion to advantages, or of humane guilt feelings about the iniquities of social welfare) primarily served to sustain his forthright spirit of eclectic empiricism.

Namesake of a paternal uncle, a noted zoölogist (who, with Haeckel, was among the first to spread Darwin's ideas in Europe), the young Claparède early determined to live up to the obligation imposed by this identity of name. Another distinguished relative, Theodore Flournoy, the neurologist, was instrumental in the post-adolescent materialization of his early ego-ideals. In 1892, having established, as a department of the science faculty at the University of Geneva, the first psychological laboratory divorced from the faculty of philosophy, Flournoy, through his own work on synopsies, inspired his young cousin to undertake a questionnaire on colored hearing. The resultant report, published in the following year in Flournoy's *Phénomènes de synopsie*, received favorable critical comments by Binet, then engaged in similar studies in Paris. Claparède's career as a psychologist dates from then on. Medicine, however, seemed to him the best preparation for the psychological study of man. After one semester at Leipzig, he returned to Geneva to complete his medical studies, with a doctoral thesis on *Du sens musculaire, à propos de quelques cas d'hémiataxie posthémiplegique* (1897). After a year at the Salpêtrière with Dejerine

¹ This date has been positively established by letter from Dr. A. Rey in Geneva who is taking over some of Claparède's clinical work. *Science* was wrong in giving the date as September 2, and the *Psychological Bulletin* copied *Science*. Claparède himself was at pains to get necrological dates correct: he published 419 of them from 1915 to 1934 in his *Archives de Psychologie*. Although it has not been easy to communicate with Geneva in these times, it is fortunate that this account can continue Claparède's tradition of accuracy in this respect.

(where the then acute Dreyfus affair almost led him to more pro-Dreyfus debating than neurological work on ataxia and sensory disturbances), he returned to become Privat-docent under Flournoy, teaching a laboratory course on sensation. While he decided not to give up neurology and kept up his part-time clinical practice until 1920, his therapeutic work on stereognostic perception and its disturbances, agnosia, and ataxia, only served to strengthen his interest in the properly psychological study of human behavior. In 1904 he took over the laboratory, becoming associate professor in 1908 and a full professor in 1915. Other dates to record are: 1901, when, with Flournoy, he founded *Archives de Psychologie*, and 1912, when, with the assistance of Pierre Bovet, he established the Rousseau Institute.

At first glance, Claparède would seem to have ranged too widely in his research to be able to dig in intensively at any one point. He left behind him creative exploratory studies which were never really completed or worked through to the more refined stages of experimental inquiry. He never completed his major book on animal psychology (announced long ago in the Ebbinghaus collection), his studies in child psychology of which *Psychologie de l'enfant et pédagogie expérimentale* (1909, translated into ten languages) was to be only the first volume, nor his work on will and on testimony. It was partly that, as teacher, reformer, citizen and family man, he led a life which was too full and rich to allow time for more intensive research. As his own severe critic, he saw in himself a basic predisposition for preliminary sketching of plans, rather than seeing them through in minute detail. Largely, however, these seemingly scattered activities were the natural consequences of a definite point of view: he wanted to work and to initiate work in a variety of psychological fields (neurophysiological, animal, child, educational, social, industrial). He had a considered fear of prematurely specialized points of view which might keep him from that *vue d'ensemble* which young and narrow psychological specialties could not be expected to afford him as yet. "I would rather not understand than arbitrarily adopt simple schemes which do not faithfully reflect reality." If the neurological study of cerebro-cortical localizations (of everpresent interest to him) was relatively but a rigid, "clumsy scheme," so too were the more grossly behavioristic and mentalistic approaches to human behavior. Such is the orientation back of his efforts to clear up terminological squabbles between introspectionists and behaviorists regarding empirical work in animal psychology at a time when the possibility of such work was a befuddled issue.² In his early critical attack on associationism,³ he was concerned with clarifying what necessarily limited associationist methods could and could not explain, stressing the direction of ideas and the different patterns of association, points on which the Würzburg school were to focus a little later and, eventually, the Gestalt school.

Deeply influenced by Karl Groos' *Die Spiele der Tiere*, Claparède became converted to a zoölogical and functional-purposive conception of conduct, in opposition to the more mechanistic neurological and behavioristic approaches and to the mentalistic emphasis of introspectionists. With classical simplicity, his "law of momentary interest" expressed the functionalist notion that, in intelligently directed adap-

² *La Psychologie animale de Charles Bonnet*, 1909, 1-96; and *Tierpsychologie*, in E. Korschelt's *Handwörterbuch der Naturwissenschaften*, 9, 1913.

³ *L'association des idées*, 1903, 1-426.

tive response to the momentary (concrete) situation, the underlying need (interest) and the capacity for adequate reaction were primary postulates for psychology—much as the postulate of gravity is 'given' for physics. Dynamogenization of response was then determined by the stimulus plus the need of the moment; the intensity of response being proportionate to the extent to which a given stimulus seemed capable of satisfying the need. Invoking here a long or short list of instincts seemed no more necessary or helpful to him than reducing the problem to reflexes, nerve impulses, or other neurophysiological mechanisms; nor was he impressed with Gestaltist nomenclature in such matters. It is indeed a challenging question whether so far, at the present stage of our ignorance, much more, if any, theoretical elaboration is indicated beyond the unpretentious formulations of this "determined empiricist." Such simple functional formulations may help research thinking, for the time being, in an economical, uncomplicated and uncomplicating manner, without involving premature theoretical escapades or conceptual fixations.

Claparède's functional conception of sleep is a case in point (a man may sleep, not because he is intoxicated or fatigued, but so as not become intoxicated).⁴ Its force is to bring current antagonistic conceptions of frustration potentially into balance, for it calls attention to responses to frustration which range all the way from 'boredom' to slumber. Such reactions are being overlooked in contemporary studies in favor of more dramatic and obvious ego-defensive responses (ranging from clearcut hostility to ambivalent fits or hysterical amnesias and anesthetics).

Claparède felt that general psychology as a young experimental science would benefit from what child behavior (*i.e.* child psychology and educational psychology) can teach the investigator in keeping his feet operationally on the ground. It is the nearest approach to supplying him with the equivalent of what the medical investigator has at his disposal through clinical patients. It was for similar reasons that Claparède tried to initiate and encourage work in social and applied psychology, beginning with *Expériences collectives sur le témoignage*,⁵ his early study of "agreement in error" in testimony.

Of such research efforts, his exploratory findings on the prior appearance of awareness-of-difference *vs.* similarity in the face of seemingly contrary behavior of children in problem solving, merit special mention. These (including his well-known formulation of *prise de conscience* or Law of Becoming Conscious),⁶ as also his incompleting studies of syncretic perception and related aspects of selective attention and awareness, had a direct influence on the whole series of investigations by Piaget and his collaborators, and on the work of Rey and other investigators.⁷ In this connection, Claparède's technique of *réflexion parlée* (calling for continuous reporting by Ss in problem solving) provides a research tool, the

⁴ Cf. his chapter (Le sommeil et la veille) in G. Dumas' *Traité de psychologie*, 2nd ed., 1929, and *Esquisse d'une théorie biologique du sommeil*, *Extrait des archives de psychologie*, 1905.

⁵ *Arch. de psychol.*, 5, 1906, 344-387.

⁶ La conscience de la ressemblance et de la différence chez l'enfant, *Arch. de psychol.*, 17, 1918, 67-77; La psychologie de l'intelligence, *Scientia*, 11, 1917, 353-367.

⁷ J. Piaget, *The Language and Thought of the Child*, 1923, and the other studies in child logic and child cosmogony; A. Rey, *L'intelligence pratique chez l'enfant*, 1935.

possibilities of which are not adequately indicated in his exploratory studies of hypothesizing as a special phase of reasoning.⁸

Even allowing for a 'differential of trail blazing' for preferential weighting of such exploratory rescarches, it would seem that they do not represent his most important contributions to psychology. On the other hand, they probably rank as high as do few other comparable studies by pioneers of his caliber—precisely because they were not utilized for topheavy terminological fantasies or theoretical variations on basic themes. Claparède did not deem such exploratory work strong enough to support or warrant self-contained systems of mysticism. A lot more exploratory widening might still be preferable to systematic elaborations within psychological specialties—in keeping with his methodological emphasis on the need for preliminary explorations or *tâtonnements* and in patient quest for valid horizons.⁹

As a living embodiment of Claparède's most significant achievements, the Rousseau Institute largely serves to concretize and symbolize his work record.¹⁰ Here and at the university (with which the *Institut des sciences de l'éducation* eventually became affiliated) he gathered and inspired a noted group of fellow investigators, teachers and students, including Descoeudres, Bovet, Piaget, Baudouin, Antipoff, Loosli-Usteri, Rey, Meili. In addition to psychology proper, it is from this seat of his labors of love that he diffused his influence in the field of progressive education—an influence exerted upon a great number of undergraduate and graduate students from all over Europe, the Near East, and North and South America.¹¹

In addition to the Rousseau Institute, it was in connection with international and regional congresses of psychologists that he made his most significant practical efforts, by means of his ever continuing, zestful interest in methodological and terminological classification. He had no peer in his inspiringly unaffected approach to the problem of conceptual rubbish burning, utilizing to this end his function as permanent secretary of international congresses. That his deliberate plans for clearing up and 'getting past' the by-products of psychological "phrase mongering" have not yet materialized is indeed to be regretted. The failure of his fellow workers to sit down and to try to clear up overlapping equivalences in method and terminology probably represents the single major frustration of the need for closure in Claparède's life as a psychologist.

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⁸ La genèse de l'hypothèse, *Arch. de psychol.*, 27, 1933, 2-155.

⁹ Claparède left a manuscript entitled "Morale et politique," which is to be published soon.

¹⁰ Basically a normal school or teachers' training institution, specializing in the pedagogy of pre-school, kindergarten, and grade school children, the Rousseau Institute has also been from the first a center of child research. It includes the well-known *Maison des petits* (experimental nursery school and kindergarten). Its students had an opportunity for field practice and research in several demonstration grade schools affiliated with the Institute. Numerous studies in child and educational psychology were published from it (besides the *Archives de psychologie*) in the *Collection d'actualités pédagogiques* (published under the joint auspices of the Institute and the *Société belge de pédotechnie*) and in the *Cahiers de pédagogie expérimentale et de psychologie de l'enfant*.

¹¹ In this connection may be mentioned the following works: *L'école sur mesure*, 1920; *L'éducation fonctionnelle*, 1921; *Comment diagnostiquer les aptitudes chez les écoliers*, rev. ed., 1933.

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

Mathematico-Deductive Theory of Rote Learning. A study in Scientific Methodology. By C. L. HULL, C. I. HOVLAND, R. T. ROSS, M. HALL, D. T. PERKINS, and F. B. FITCH. New Haven, Yale University Press, 1940. Pp. viii, 329.

Though this volume is concerned with rote learning—a phenomenon which may or may not be highly valued by comparison with other psychological activities—it assumes an importance entirely out of proportion to its subject-matter. First, it brings into focus the need and importance of theory in psychology as well as in all other sciences. Secondly, it stimulates the reader to consider the nature and place of symbology, mathematics, and logic in psychology and science in general.

The following is the essential purpose of this work as described by Hull, the senior and primary author. First, to build up a deductive logical system. The desirability of doing this was suggested by the work of Lepley, which indicated that an underlying identity exists between rote learning and conditioned reflexes. Secondly, the utilization of ordinary mathematics for the derivation of the theories of a system. Thirdly, to supplement the mathematics by the methods and symbolism of symbolic logic. Finally, experimentally to check the evolved theories.

The volume, then, consists of an exposition of a system composed of (a) 16 undefined and 86 defined terms (significantly called concepts), (b) a set of postulates (primitive propositions), and (c) the demonstration or proof of 54 theorems and 110 corollaries. The theorems and corollaries are only mathematically and not symbolologically demonstrated, except that a symbolologic proof of the corollary to the first postulate is offered as an illustration. This constitutes Appendix B, while Appendix A offers some supplementary remarks concerning symbolic logic by Dr. Fitch. A third appendix, C, consists of an index of logical signs.

In addition to the symbolologically presented definitions and postulates, the authors, out of consideration for those unacquainted with symbolic logic, formulate the system in ordinary English. The volume is not printed from type, but produced by a photo-offset technique from a typewritten manuscript.

In his forward Dr. May writes that the Institute of Human Relations has supported its preparation mainly because it furnishes a demonstration of the logic-empirical method of science in psychology. Since deductive logic is also emphasized by the senior author, the fact is not stressed that the volume contains an excellent monograph upon the subject of rote learning. This material admirably describes the experimental procedures employed in the study of such learning and the technique of analyzing the results as a general psychological enterprise, with the generous complement of 4 figures and 44 tables.

Because the authors regard the present work simply as an exercise in system-making, a critical estimate of their systematizing results is hardly called for. Despite the fact that the system is intended to be a theory of actual psychological phenomena, Hull points out that in a number of cases the undefined notions do not represent observable objects, processes or operations, whether logical or experimental. Again,

Dr. Fitch says: "It is not known whether all the postulates are consistent and independent (p. 310)," though it is supposed they are. Furthermore, the senior author literally flaunts the trials and especially the errors of this and a former attempt to create a deductive system. Accordingly, it is only proper that an appraisal of the volume should be concerned with the more comprehensive issues of scientific methodology. In turn, then, we consider the place of deduction, general logic and symbology in science.

The attempt to develop a deductive system in psychology raises a question as to how far deduction is a fundamental process in science. Whether one regards deduction as the subsumptive classification of traditional syllogisms, which presumed to exhibit the power of abstract reason, or the organization of tautological systems in more recent formal logic, the chasm between such deductive systems and scientific work is uncrossable. The search for such systems is symptomatic of a reversion in thought to an early stage of rationalistic science when absolutistic Euclidean geometry held exclusive sway. Hull, of course, realizes the sharp separation between this type of logic and the investigative work of science which he calls the empirical component, but he regards the latter as only a stepping stone to rigorous systematization.

Is there any other method in science than the fundamental investigative procedure in which the phenomena involved are studied under specific conditions? Has physics, for example, advanced by deducing theorems from undefinables or by interrupting a beam of light with a prism, analyzing pitchblend, discharging electricity through gases, passing sparks across a resonator wire, producing fogs in dust-containing and dust-free chambers, etc? Can deductive systems in science be anything more than descriptive models set up by way of symbolizing investigative results after the work is done? For instance, setting up at a certain stage in chemical history the following deductive system: No atom weighs more than 240; Uranium is an atom; therefore

The history of science is clear in its verdict concerning the effect of closed and fixed systems upon scientific thinking and investigation. The transition in science from Scholastic authority to modern experimentation is a progressive deviation from deductive proof toward free hypothesis and manipulative investigation. If it ever seemed at all feasible to reduce particles and motions to points and lines in a deductive geometry, it was only because workers simplified their problem by reducing their phenomena to abstracted relations between static things. Even then the plausibility of the scheme lay in the fact that it made possible the operations of elementary calculation. With the earliest development of dynamics new types of calculation (calculus) had to be originated, so that even though Newton threw his *Principia* into a Euclidean-deductive framework he employed the calculus to develop it. Today, of course, no one regards even abstract geometries as other than hypothetico-deductive systems, that is, systems of deliberately chosen elements interrelated upon the basis of operations and criteria deliberately assumed.

It is a serious error to confuse the work of quantitative symbolization and calculative operation with the formalistic structure of deductive systematization. Genuine scientific deduction consists merely of hypothetically bridging the gap from one set of observed events to another. This implies tentative orientation with respect to partially known happenings and not any closed circular system. The difference between the two is illustrated by Maxwell's production of his famous electromagnetic

equations. Those who think rationalistically like to believe that Maxwell merely mathematically deduced the radiation of electromagnetic waves which was later experimentally confirmed by Hertz. Actually, however, he built with meticulous detail upon the experiments of Faraday and in no sense did he start with undefined concepts. It is all very well to regard equations as purely formal structures, but as the study of mathematical processes indicates, symbols and equations are always derived from interbehavioral operations and therefore imply the concrete materials and situations from which they are abstracted as well as the unrepresented residues.

So much for deductive logic. What about logic in general and its place in scientific work? Although Hull recognizes the difference between (a) logic as a scientific tool and as "a subtle distillation of the human spirit held in religious awe" (p. 7), as well as (b) the gap between the closed and finalistic systems of theology and metaphysics and the approximative systems of science (p. 4), he still regards "logic" as a powerful, autonomous and unique agency in scientific work. He writes of the "use of logic" in system construction (p. 11) as though logic were something other than the actual work of system-construction. Moreover, as we have already indicated, he regards logic as something different from actual contacts with the problems and data of science.

All this suggests the necessity of distinguishing between at least three different referents of the term *logic*; namely, inferential behavior, general system-building, and deductive systematization, each resulting in different products. Obviously, logic as actual reasoning, concrete inferential operations, is an indispensable factor in all complex activities, including scientific investigation as a matter of course. A similar statement may be made about general system-building. On the other hand, when logic means deductive system-making, it is questionable how important it is for scientific enterprises. Hull certainly does not overlook the discrepancy between deductive systematization and scientific investigation in psychology because he is tremendously disturbed by the fact that many of the theorems of his system do not accord with and are even opposed to experimental evidence (p. 306). Accordingly, his exposition is thoroughly permeated with the conflict between a closed deductive system on the one hand, and the claims of empirical science on the other.

When one really regards logic as processes and operations in science or other intellectual or non-intellectual enterprises, is it not obvious that there are many logics and types of logic? Logic, that is, logicizing, is essentially an enterprise of specific and particular system-construction. As such the procedures and operations and the criteria for these constructive operations depend upon purposes or ends in view and the material worked upon. A perfect logical system may be constructed by arbitrarily choosing elements and setting up manipulatory rules without regard to anything else than a willfully accepted criterion of consistency.

When logic is employed for systematizing psychological phenomena and processes as in the present instance, the question arises how to justify the inclusion of arbitrary and even false elements. What sort of scientific system can be built out of neural traces and inhibitory and excitatory potentials? It is highly significant that the present system-building, which does include such materials, is implemented with the rationalization that these materials represent unobservable entities like energy, and that sometimes theorems concerned with unobservables aid in developing theorems concerning observables.

As to the former point, it is highly questionable whether energy is unobservable in any sense other than non-directly visible. As to the latter, does nonsense-syllable learning involve any difficulties of observation and theory serious enough to prevent building an adequate and satisfactory scientific system upon the basis, say, of the excellent analysis to be found in the present volume? We need merely substitute for the interest in deduction an interest in the specific conditions observable in the particular behavior-field or frame of reference.

The difficulties involved in employing "logical" methods in the study of psychological phenomena, even rote learning, are in no sense mitigated by resorting to symbolic logic. Here as elsewhere the authors of this volume start with a perfectly justifiable proposition that they wish to secure accuracy of terms. That such accuracy is necessary requires no argument, but can symbolic logic provide it? How the attempt works out may be illustrated by the results. In following out the procedure of symbolic logic the authors present two series of formulations, one called undefined concepts and the other definitions. Here is the first of each series:

U1. *Syllable exposure* (slex): A class of events each of which may be described as the stationary presence in the window of a memory machine of a syllable consisting of a vowel placed between consonants in a combination not used as a word by the subject. The syllable is supposed to be printed in such a way as to reflect clearly a characteristic pattern of light rays. The subject may or may not be present.

D1. The *duration* (du) of an event is the length of time between its beginning and end.

No prompting is necessary to observe that the only difference between the two is that the undefined concept, *syllable exposure*, is more adequately and more precisely defined than the defined *duration*. Hull himself suggests that there is no proper division between his U and D terms (p. 306). Furthermore, the symbolization in this particular instance of a defined concept, D1', $du \equiv t' \hat{a} [t \equiv nd 'a - bg a']$, is much more complicated than the verbal sentence. This, despite the fact that we expect symbolization to make for simplification as well as clarification.

There is, however, a deeper question than style of symbolization. Granted that proper symbols can materially aid in describing phenomena accurately, we must still insist that where science is concerned the phenomena must be available. To make such phenomena available is obviously the work of observation and experimentation. No amount or quality of symbolizing behavior can produce either data or science. To make a symbolic system for scientific psychology one should at least consider the many experiments that cast doubt upon traces and cumulative organizations within the learner's body. Not until we settle the problem of the propriety of symbols—their indication of eventual structure or function—are we concerned with their accuracy. Moreover, the accuracy or serviceability of symbols depends entirely upon the scientist's interbehavior with events and his freedom from unrecognized presuppositions.

In general, linguistic and symbolic science is unambiguous in demonstrating that words or symbols, whether in mathematics (calculating), logic (system-making) or natural-science (describing) must be derived from actual interbehavior with the problems and data initiating the work. Only when our symbols perform specific functions in particular interbehavioral situations can we avoid the difficulties of overrefining our terms or formalizing them beyond the point of diminishing returns,

and, what is more important, prevent them from obstructing our investigative labors. Perhaps this explains why the physicist has not turned to symbolic logic rather than Hull's suggestion that the physicist does not need this method because his concepts are less elusive (p. 306).

Again, symbolic systematization or any similar form of careful system-building can aid the scientist by a proper relating of propositions. But in the present case the type of logic is that of the *Principia Mathematica*, which is hardly the best for the purpose, if only because it was developed as the systematization of mathematics. Moreover, Dr. Fitch adds the axiom of infinity, the proof of which the original proposer declares to be fallacious, and retains the rejected axiom of reducibility, a procedure which hardly strengthens one's confidence in using symbolic logic to solve psychological problems.

As a final reaction to the present enterprise the reviewer wonders whether this is another one of the long series of attempts by psychologists to compensate for their inferiority-feeling because of the presumed necessity to deal with intangible and unobservable things. But even if this be the case, the present authors must be credited with the originality of their attempt to advance psychological theory. Traditionally, psychological theory was promoted by transforming the psychic into what appeared to be more dependable material; namely, neural functions. The present authors, realizing that neural functions or traces are themselves unobservable and in general unreliable, adopt the symbolological method. Retaining the neural traces, they proceed to remodel scientific methodology by introducing a substitute or an additional technique; namely, deductive and symbolic logic.

As we have already noticed, Hull appears extremely sensitive concerning the ineptness of the details of this enterprise. In a similar way he is aware of the possible failure of his general effort to improve psychological theory. In this connection he attempts to ward off objections by saying that if mathematico-logical psychology does not work very well, neither has physical theory attained perfection (p. 6). As to possible scepticism concerning symbolological psychology, he remarks that at one time experimentation in psychology had been doubted and is now accepted (p. 12). To the reviewer both of these defenses appear to be excellent examples of *non-sequitor*.

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J. R. KANTOR

Emotions: Their Psychological, Physiological, and Educative Implications. By FREDERICK H. LUND. New York, Ronald Press Co., 1939. Pp. xiii, 305.

This book is a result of a subsidy granted by the Josiah Macy, Jr., Foundation to a Committee on the Relation of Emotion to the Educative Process as organized by the American Council on Education. The immediate aim, therefore, was to bring together in a serviceable relationship the main facts concerning the emotional life that were pertinent to education.

Taking the lead from his own definition of the main "business of science" as the identification and description or the definition and classification of "objects and sequences," the author proceeds in the first chapter with the identification of emotions. Besides showing how complex this problem is, he involves his exegesis with additional complications by referring to the mentalist as an "individual who accepts philosophic dualism"—which is a statement contrary to fact—and by stating that

"the physiological changes which follow upon [sic] such perception are conceived merely as effects, not of the inciting stimulus but of the emotional state." Even the qualifying statement which follows does not do justice to William James. The consultation of Warren's *Dictionary of Psychology* in the proper places would have done much to prevent the erection of 'straw men.' While the term 'emotion' is still somewhat ambiguous, especially in its reference to the mind-body relationship, in the reviewer's opinion it is no longer important to raise the mentalist-behaviorist specter especially when a brief survey is in order. Considering the limitations of space, the identification of emotion through facial expression is very well done.

In the succeeding two chapters on the neuro-glandular basis of emotional reactions the author sails forth on smoother waters and citations to this literature are authentic and well selected. The evidence here marshalled is, however, preponderantly on the Cannon side and the telling assaults of Wheeler, Stewart, and Rogoff are omitted. When the mind-body problem is repeatedly implied, as in the experiments of Marañon, Cantril, and Hunt, it is cautiously avoided. The author ends the discussion, however, by calling attention to the impossibility of ascribing exclusively to the thalamic processes the conditions that are identifiable with emotion.

The following chapters deal with cardio-vascular and respiratory and with gastrointestinal and sexual changes. A considerable amount of material is summarized in this section, but a disproportionate number of pages (20) covers the topic of sexual emotions, in view of the author's admission that in human Ss "very few attempts have been made to measure the changes associated with sex excitement" and that "such material as is available is too descriptive and general to have much scientific value."

Chapter 6 discusses metabolic and skin changes accompanying emotional experiences. The broader aspects of basal metabolism are not considered—probably for want of space—and statements concerning the electrodermal responses are likewise often suspiciously succinct. A typical example is the statement that "physiologically the response may be traced to increased sympathetic outflow and to increased sweat secretion, producing higher permeability of the tissues and reduced resistance to the passage of the current." That interpretation does not lend a favorable ear to the widely accepted theory of Gildemeister and others which depends upon a change of polarization or upon some phase of capacitance.

The final chapters (7, 8, 9) begin to bear down on the real problem of the book; namely, the relation of the emotions to the educative process. They are entitled "Development and Control of Emotions," "Emotions and Motivation," and "Conclusion." The work on children's emotions, their expression and control, is presented in a well organized and clear manner. The chapter on motivation consumes a surprising amount of space devoted to experiments on the lower animals and the concluding chapter has very little to say about education.

When we thus view the book as a whole we find it easy to suggest improvements chiefly because we did not write the book. For one thing, the reviewer has been warned that the omission of references to recent important publications may be accounted for by the fact that the manuscript was delayed several years in publication without the chance for revision, which is always an unfortunate procedure. This can, however, hardly explain the complete avoidance of any reference to Beebe-Center's important treatise on the *Psychology of Pleasantness and Unpleasantness* (1932) and the dismissal of Wundt with what amounts to a single reference. It is

presumptuous, however, to criticize an author on his sins of commission and omission. The fact remains that the bibliographies at the end of each chapter are uncommonly replete with authoritative references. The only observation which we would seriously make is this: in a book conceived to serve the educational profession, no great effort appears to be made to coördinate the scientific data available with educational theory and practice. A welter of physiological and psychological facts permits the average educator to continue his floundering in the sea of scientific research.

Chicago, Illinois

CHRISTIAN A. RUCKMICK

Cognitive Psychology. By THOMAS VERNER MOORE. Philadelphia, J. B. Lippincott Co., 1939. Pp. viii, 636.

Animism dies hard. This new text is a delight to the student whose interests include the historical development of the scientific point of view and its perennial battle with the verbal habits of the past—more commonly known as ritual and dogma. The student should note the similarity of approach to the problem of the higher thought processes in this volume and that of human motivation in the author's *Dynamic Psychology* (same publisher, 1924). In both volumes, short shrift is made of any evidence of interpretations that fail to provide for the necessity of the animistic agents and concepts used by the early Greek philosophers and primitive cultures generally in their attempts to grapple with the problems of the activity of man.

Moore can see neither the humor nor the logic of his own observation: "Materialism exists mainly among . . . the modern physicians." To him it seems fitting that astronomers should not be animistic about planets and stars, chemists about compounds, nor aviators about airplanes, but somehow, the men who know most about the biochemistry of living matter should still be animistic about that! Psychology is admittedly the last science to break way from the animistic terminology and point of view. The divorce is far from complete today; witness this volume. When psychologists, however, are brought up on biochemistry, neurology, and physiology instead of religious dogma and ancient and medieval philosophy, statements such as this, "The human person, the conscious, psychic ego, is the primary fact in all experience," will begin to sound to them like "sunrise" and "sunset"—convenient and habitual expressions handed down from the past but hardly to be confused with workable, instrumental, scientific concepts.

One need not then be surprised at the general treatment of the subject. The first section on Consciousness and the Nervous System attempts to sum up all the evidence from neurology and pathology in such a way as to make inevitable the acceptance of the Aristotelian doctrine of formal causes and the Christian hypothesis of the immortal soul. Out of the rich literature on conditioning, now numbering well over a thousand titles, he has found nothing more relevant than the unfortunate Pavlovian theory of "analyzers." The basic phenomena of conditioning are dismissed as being caused by "psychic reflexes" or conscious processes in the face of all the excellent experimental evidence to the contrary. There are a hundred pages of history of theories of perception. Throughout the volume one encounters generous evidence that Theology is still putting up a battle against the inroads of the scientific point of view, a struggle that has lost none of its vigor because it is reduced to a rear-guard action. One of the ingenious techniques used in this sanguine warfare is

that of "solving" various difficult problems by "applying philosophy." What this means, of course, is that the ordinary rules of evidence and science are abandoned in favor of some more acceptable traditions of the past. One can be sure that such devices always result in conclusions highly satisfactory to the dogmas of medieval Christianity. But why bother? We already know that such must be the character of any psychology text that hopes for a *Nihil obstat* and an *Imprimatur* on its title pages.

New York University

GEO. B. VETTER

Critiques of Research in the Social Sciences: I. An Appraisal of Thomas and Znaniecki's The Polish Peasant in Europe and America. By HERBERT BLUMER. New York, Social Science Research Council, 1939. Pp. xv, 210.

This is the first of six critiques planned by the committee on appraisal of research of the Social Science Research Council. Each of the six works was selected on the basis of reports from a score or more of "outstanding workers in the discipline," who were asked "to submit a list of three to six works, published in the United States since the Great War, which in the informant's judgment had made the most significant contributions to knowledge in the particular discipline." Besides Blumer's analysis, this particular book contains short rejoinders by both Thomas and Znaniecki, a digest of a round table discussion on the critique, and summary comments by several members of the round table. The report of the round table discussion is in itself an interesting document, and social psychologists will find it an addition to their material on behavior in committee situations, although its value is diminished by the fact that the discussion had to be digested and could not be reported in its entirety.

Blumer's primary concern is with methodology. He believes the authors, Thomas and Znaniecki, have shown conclusively the need of considering "subjective factors" if social life is to be properly understood; but he does not think their methods—analyses of letters, life histories, newspaper accounts, institutional records—have uncovered these subjective factors in adequately scientific fashion; and he even doubts whether such methods can be satisfactorily used for such a purpose. "My impression is," says Blumer, "that the larger part of their theoretical interpretation, and particularly its more abstract features, cannot be tested by their documentary materials. Instead, I found Thomas and Znaniecki presenting a document and making comments on it in the nature of an analysis. These comments are usually exciting and very plausible, but there is nothing in the nature of the document which enables one to declare whether or not their analysis is correct."

With Blumer's methodological objections most psychologists will find themselves in complete agreement. The limitations and uncertainties of autobiographical material as ordinarily obtained have been recognized in psychology for quite some time, at least by that large body of men who were brought up in the experimental tradition. Unhappily, there is still here and there a social psychologist who has not yet seen the light in this respect, and to him the reading of Blumer's able analysis is recommended as a desirable, if troublous, experience.

The reviewer feels that the critique would be much more valuable if it contained a thorough analysis of Thomas's concept of attitude. That this concept has been extremely useful, there is no doubt; that it is disgracefully ambiguous and vague,

is also true. The committee had an excellent opportunity to do something about this state of affairs, but it missed fire. A psychologist appointed to work with Blumer on *The Polish Peasant* might have gone far towards clearing up some of the confusion. (Blumer discusses "attitudes," but inadequately; the task is obviously one for a psychologist rather than a sociologist.) If this "subjective factor" is so extremely important, as seemed generally agreed among the members of the round-table, we ought to know what present-day psychology has to say about it.

Western Reserve University

HERBERT GURNEE

L'éducation de demain: La biologie de l'esprit et ses applications pédagogiques. By J.-E. MARCAULT and TH. BROSSE. Paris, Felix Alcan, 1939. Pp. xi, 308.

We are again confronted with an eclectic systematization from the *Bibliothèque de Philosophie Contemporaine*, this time on the thesis: what is essentially human in man, the *moi conscient*, is an objective fact occupying the center of human biology and as such to be studied in relation to psychophysical activity and the natural-social milieu. "The *moi*, transcending psychic and psychological functions alike, acts as a biological level *sui generis*; in it are integrated all mental, affective, and organic functions."

The authors (one an academician, the other a physician) adduce considerable material from biological evolution and from neural anatomy and physiology to sustain two cardinal principles underlying psychophysical unity: (a) integration (structural), (b) subordination and control (functional). Studies in neuropathology, particularly in differential chronaxies (central connections intact), are cited to establish the presence and role of the *moi conscient*.

Into an onto-phylogenetic notion of the individual's psychological structure are fitted six developmental levels (*niveaux*): *sensoriel, motrice, affectif, mental, social, intuitif*. The first four constitute a sequence from birth to maturity. At each *niveau* the *moi* is alleged to exercise an autonomous integrative function further schematized into three temporal phases: contact, analysis, and control. The point of view is genetic and, one suspects from some of the machinery introduced, psychoanalytic.

The latter part of the treatise (Livre III) is concerned with methodological concepts for *l'éducation de demain* (*demain* is defined in a footnote as an "era," not a "date"). In the matter of contemporary realization of their thesis, the authors favor progressive education and the conceptualism underlying the Montessori system and the Dalton plan. A psychological palm is bestowed upon the Boy Scouts, a movement which has afforded many a *moi conscient*—at the proper *niveau* (v.s.)—free expression in the natural milieu. Despite the dialectic severity which the authors have imposed upon their task, critical psychologists will be disturbed by the absence of quantitative data anent the efficacy of progressive education. The authors propose as tractable to their basic theme several long-standing problems: progressive *vs.* traditional education, the primacy of the individual over the social institution, democracy *vs.* dictatorship.

On the literary side, the style is clear and sprinkled with fetching epigrams, e.g. "Medicine is education for health, education is the medicine of health." Occasional passages reminiscent of William James's more homely muse relieve the impressive logic. Less concern with the device of repetition would, however, have materially reduced the bulk of the piece (pp. xi, 308, in actavo).

In summary, if psychologists, by reason of their training, will be regaled by the provocative insights abundant in the first part of the treatise, the pedagogical brotherhood will find much that is at once inspiring and contentious in *L'education de demain*. In lesser hands the scope of the *moi conscient* thesis might conceivably fall into mere heterogeneity and the material adduced constitute a pot-pourri of psychological chestnuts.

University of Maryland

WALTER M. SPARKS

Minor Mental Maladjustments in Normal People. By J. E. W. WALLIN. Durham, Duke University Press, 1939. Pp. vi, 298.

The material in this book is derived from personality inventories of graduate and undergraduate students in psychology who were asked "to describe their personal maladjustments as accurately as possible and without exaggeration." These descriptions, as edited by Dr. Wallin, are intended as case histories to show "that the sources of many maladjustment and personality traits can be discovered by reasonably intelligent people without the use of any intricate, esoteric, and diagnostic procedures." They are also presented as true life stories of human interest and as illustrative material for courses in abnormal psychology. The case histories, for the most part, are rather short, covering usually not more than one half to three quarters of a printed page. They have been conveniently grouped to illustrate various types of neurotic symptoms and certain behavioral difficulties, such as fears, phobias, reactions to unfavorable comparison, and daydreaming.

In appraising the book, it is necessary to distinguish between its factual contents and its stated purposes or claims. As regards the former, Wallin's volume furnishes a compendium of illustrative case material which should prove useful supplementary reading for courses in abnormal psychology; but as regards the latter, there is little in the presentation of material of the case histories which supports the author's general thesis that many emotional abnormalities and mental quirks can be cured without the use of intensive psychiatric therapy. In the first place, many of the cases cited strike even a non-psychiatrist as being far from mere minor mental maladjustment. While it is true that the "biographies" are from college students, it is very questionable whether those sampled in the case studies represent a normal population in the ordinary sense of the term. The fact that the subjects were not under treatment is, of course, no proof that they did not need psychiatric attention.

In any event, the easy distinction which the author makes between major and ostensibly minor mental maladjustments, as well as his general attitude toward self and lay therapy is open to serious strictures. The most obvious one is the immediate danger that many individuals needing psychiatric attention will gain the impression that mental symptoms and difficulties can be cured by reading a book or taking a course in abnormal psychology. This may have been far from the author's intention, but that the danger is real is evidenced by statements made in many of the biographical sketches cited. Thus, the subject of the first "biography," an individual who has obviously been suffering from a deep neurosis of long standing, makes the following statement: "My only reason for entering the course was the hope of securing a solution of my annoyances;" and a few lines later adds, "it is with a great deal of satisfaction that I can report that the solution has been found in the reading I have done from suggested sources." It is to be hoped that no considerable

number of students taking abnormal psychology have similar reasons for pursuing the course. Wallin's exposition of minor mental adjustments, however, is hardly calculated to discourage them from such a point of view.

Bellevue Hospital

DAVID WECHSLER

Analysis of Handwriting: An Introduction into Scientific Graphology. By H. J. JACOBY. London, George Allen & Unwin, 1939, Pp. 238.

The author does attempt to disarm the skeptic by pointing out that scientific graphology is not to be confused with fortune-telling, palmistry, and mind-reading. He does not take the extreme position of saying that there are no limitations to the work of the graphologist. He is perfectly willing, however, to apply graphology to everything from mentality to matrimony, and from personnel to psychotherapy.

The book is intended as an introduction to the *science* of graphology, and the scientific method is employed by analyzing specimens of handwriting according to size of letters, difference of length, writing angle, degree of width, spacing, and degree of pressure. Science, however, flies out the window in at least four respects. First of all, one gets the feeling that the principles evolved would not necessarily be agreed upon by other graphologists; and this suspicion is confirmed when the author says, "The method expounded in this book is that of the author" (p. 10). In the second place, handwriting is said to reveal certain personality characteristics, but how these characteristics are to be determined *independent* of the handwriting analysis is not stated. Thirdly, many of the terms used in the analysis cannot be operationally defined; for example, "the libido of the writer is very onesidedly directed to goals which are in the outside world and in the future" (p. 207). Finally, the analyses are made up in large part of such glib generalities that they could easily apply to almost anyone: "The writer is a sensitive, alert, and unsettled person . . . susceptible to many impressions, able quickly to adjust . . . active and striving in many respects and ways . . . equally interested in the destinies of other people . . . penetrating intellect . . . adroitness . . . quick grasp . . . skill in combining things" (p. 218).

George Washington University

STEUART H. BRITT

What Church People Think About Social and Economic Issues. By N. L. TROTT and R. W. SANDERSON. New York, Association Press, 1938. Pp. 79.

This is an interesting attempt to measure social attitudes among the white Protestant Church members of greater Baltimore under the sponsorship of the Council of Churches and Christian Education of Maryland and Delaware. The subjects were 1100 adolescents and adults and comprised about 2% of the active white Protestants of the area. Reasonable care seems to have been exercised in sampling. Scales were constructed by the Thurstone method of equal-appearing intervals on three topics: (1) attitudes towards the participation of the church in the social order; (2) attitudes towards private property; and (3) attitudes towards labor. The results are of more sociological than psychological import, and the treatment is rather non-technical. There are numerous graphs.

Comparisons are made between clergy and laity, among three age groups, between the sexes, and among educational, income, and property groups. Unhappily

these comparisons are of limited scientific value because no sigmas or critical ratios are reported; thus the clergy are shown to be more liberal than the laity, but whether this is a reliable difference is not indicated. It also seems regrettable that the authors, after going to the trouble of scaling attitude items by the Thurstone method, subsequently abandoned the scale values thus obtained for a simple rank order scale. Their excuse for this is that "the statements were spread at reasonably equal intervals along the length of the scale." Examination of the chart on page 60 shows, however, that the intervals obtained by the Thurstone method were by no means "reasonably equal." Withal the study is a valuable supplement to the mass of collegiate material by which our knowledge of attitudes is now so heavily biased.

Western Reserve University

HERBERT GURNEE

The Language of Gesture. By MACDONALD CRITCHLEY. London, Edward Arnold & Co., 1939. Pp. i, 128.

The author of this little book is a physician and neurologist on the staffs of King's College Hospital and the National Hospital for Nervous Diseases (England). He presents a survey of general information concerning the sign-languages of the North American Indians, Australian Aborigines, various types of secret societies, and of the various forms employed by deaf-mutes. Among these different sign-systems there appear to be significant similarities, and the apparent universality of much of the language of gesture is discussed in some detail. The author includes a consideration of the neurology of gesture and offers a discussion of the significance of gesture in its relation to the origin of spoken and written language.

In the concluding chapter, certain advantages of gesture over articulate speech are discussed. Among these are the international comprehensibility of gestures, the rapidity with which they can be executed, and the fine shades of meaning which can be expressed with them. In this general connection, Critchley refers to Paget's work designed to develop a universal sign-language based upon the use of Ogden's Basic English. Paget estimates that between 500 and 600 signs would suffice for his purpose. These could be selected from an extraordinarily large number of signs. Paget has stated that with the upper arm, lower arm, wrist and fingers 700,000 distinct signs can be made. "In this way the human hand can be regarded as 20,000 times as versatile as the mouth."

The Language of Gesture serves as a fascinating introduction to a field of investigation which has been extraordinarily neglected. With the growing interest in the psychology of language, semantics, and general semantics, the scientific study of gesture should be expected to undergo the development which its importance warrants.

State University of Iowa

WENDELL JOHNSON

Speech Development of a Bilingual Child: A Linguistic Record. By WERNER F. LEOPOLD. Evanston, Northwestern University, 1939. Pp. 188.

The author's daughter learned German from hearing this language always from her father and under the influence of a short period spent in Germany. She learned English from her mother and from her other environment. She was not by nature a "talkative" child. This fact probably explains that gradually the use of the German

language during the early years became more and more passive, whereas the English language became the actively used, in as much as the use of the latter was more strongly forced upon her. The author's record of the child's speech development is excellent. His use of the international phonetic symbols removes the ambiguities which in such studies make the record considerably unreliable. If one looks for a result of his work in the nature of some "laws" of bilingual speech development, one looks in vain. That is not the author's fault. He points out himself that many features of the child's speech development were caused by the child's individuality or by accidents and peculiarities of her human surroundings. The reviewer concludes from reading the book that there are no specific psychological laws which apply to "bilingual" speech acquisition. The very term "bilingual" is unclear, as the author mentions. For example, in Luxemburg all children must learn both French and German when they go to school if not earlier. In Switzerland children learn both a German dialect and the German standard language. What is the difference? In the latter case most words are the same, in the former most words are different. Psychologically, bilingual speech development does not differ from the acquisition of the spoken and written language in general.

University of Miami

MAX F. MEYER

Love Problems of Adolescence. By OLIVER M. BUTTERFIELD. New York, Emerson Books, 1939. Pp. viii, 212.

From several standpoints an adequate survey of the "love problems" of adolescents in our culture would be an important contribution. The reader who looks for such a survey in this volume will be disappointed. The author describes the "love problems" of 1,169 American adolescents between the ages of 13 and 25 yrs. as seen while he was conducting discussion courses on boy-and-girl relationships for Protestant churches, Y.M.C.A. camps, and college organizations. He had the members of his 24 groups either write anonymous questions or check the problems they wished discussed on a check-list. Such a method has all the defects of a questionnaire plus lack of systematic coverage of a set of pertinent questions. Tabulations of questions checked along with supplementary material from the literature and the author's comments on the source and solution of the various problems are described without benefit of theoretical orientation under 5 chapter headings: "starting boy-and-girl friendships," "making a good impression," "keeping steady company," "engagement problems," and "problems concerning marriage."

Social psychologists, cultural anthropologists, and others will find interesting material in this book, though much of it is obvious from casual observation and the quantitative statements have little meaning. Systematic treatment and theoretical orientation are almost entirely absent. In the author's defense it should be said that the project was not originally intended as research, and he may deserve some credit for attempting to make his discussion groups contribute to knowledge.

Brown University

J. McV. HUNT

Mind Explorers. By JOHN K. WINKLER and WALTER BROMBERG. New York, Reynal & Hitchcock, 1939. Pp. 378.

One's first impression in examining this book is that it is a "popularization." A thorough reading, however, confirms this opinion only to the extent that it is written

in a very readable fashion. The "mind explorers" include contributors from such divergent fields of endeavor as Gall and Mesmer at one extreme, and Cattell, Terman, and Lashley at the other. The chapter headings are frequently catchy, for example, "Phrenology—A Scientific Miscarriage," "The Sportsman-Scientist" (Galton), and "A Psychologic Prima Donna" (James), but the content of the chapters is usually an interesting and accurate biography and survey of the contributions of the investigators considered. Throughout the book one is continually impressed by the impartiality of the authors in describing those with whom they have chosen to deal. It is probable that many readers will wonder why so much space is given to certain individuals and so little to others; for instance, Titchener is not mentioned, whereas G. Stanley Hall is allocated thirty and William Tuke eight pages. The selection is heavily weighted by applied psychologists, by mental healers, and by men whose lives and works show "dash." On the whole, the book, though making some minor errors of fact, is one that will serve a useful purpose. Its content should not be scorned by the professional psychologist, while its style will make it attractive to the erstwhile student of psychology and to the general public.

University of Newark

FREDERICK J. GAUDET

Current Psychologies. By ALBERT J. LEVINE. Cambridge, Sci-Art Publishers, 1940. Pp. 270.

This book is a grouping together of the various older schools of psychology into the author's concept of present day psychology. He lists as the major psychological schools of today the Neurological, the Gestalt, the Purposivistic, and the Freudian. The author describes the Neurological school as follows: "Its continually broad catholicity makes for an unarticulated heterogeneity which, at times, approaches a highly diversified miscellany."

Throughout the entire book there is considerable emphasis on the problem of personality, so much so that the reader may come to the conclusion that it is a treatise on this subject. In discussing this field the author states that psychology has accepted the scientific method but has failed to make allowance for the infinitely greater influence of probability in the interplay of calculable and incalculable elements. The continuing argument leaves the reader in doubt as to the author's regard for experimental procedure in psychology. His Freudian leanings in the field of personality lead one to believe that he prefers a non-scientific interpretation of data, especially in this field.

The ordinary student will find the book confusing; and it is doubtful if the serious student will gain any clarification on the subject of contemporary schools of psychology.

Ohio State University

ROBERT Y. WALKER

Bernadette of Lourdes. By MARGARET GRAY BLANTON. New York, Longmans, Green & Co., 1939. Pp. xi, 265.

The chief value of this book for psychologists is to furnish raw material for the study of strange yet typical social behavior—both on the part of the good people of Lourdes and of the author of the book. It is a dramatic but uncritical story of events starting in 1858 with private visions of the Virgin by Bernadette Soubirous, a forlorn girl of fourteen years, and climaxing with her beatification as a saint in

1925. Crowds, which at first gathered to watch her while she had her visions, have increased until in recent years there have been over 25,000 pilgrims annually who have sought healing by faith at the remote rock shrine on the French slope of the Pyrénées. Since 1926 about 100 cures have been accredited by the official board of reviewing physicians, or about four-hundredths of one percentage of the sick pilgrims. It would be interesting to know whether the general average of unexplained recoveries throughout the world is that high.

The presentation is appealing in its simplicity. The fact that apparent sympathy for the dogmatic interpretation has influenced the author to refrain from psychological speculation does not prevent the reader from making some very interesting interpretations. It is significant that such a renowned drama of healing could be founded on such an idyllic phantasy.

University of Colorado

THOMAS H. HOWELLS

Emotion and Conduct in Adolescence. By CAROLINE B. ZACHRY, in collaboration with MARGARET LIGHTY. New York, D. Appleton-Century, 1940. Pp. xv, 563.

This volume summarizes the findings of a five-year survey of adolescent behavior undertaken by the Commission on Secondary School Curriculum of the Progressive Education Association. The aims of the study were the discovery of the kinds of problems with which adolescents are faced in our American society and the possible rôle which the school can play in facilitating the youngster's solution. The survey was made through actual school counselling practice, class-assigned anonymous diaries and essays, and extensive consultation with teachers. Little new was discovered concerning the nature of adolescent adjustment problems, but an enormous quantity of case material has been excellently organized and discussed in this extensive report.

The book is in essence a text for secondary school teachers; it describes in detail the specific motives, conflicts, anxieties, frustrations and insecurities which *real* adolescents suffer in a *real* society. There is nothing theoretical in the descriptions, and the educational recommendations are specific to American public high schools at their present stage of growth. There is an atmosphere of saneness and practicality pervading all the interpretations and suggestions. Catch words and phrases like "organism as a whole" and "adjustment" have been avoided with unusual care. The special physiology of adolescence has been subordinated to its proper place as one of many variables which influence behavior, and, happily enough, far more space is devoted to heterosexuality as a challenge to the growing child's social habits than to it as a kind of homuncloid excrecence on infantile sexuality.

Yale University

ROBERT R. SEARS

La Verdad, la Ciencia y la Filosofía. By FRANCISCO JAVIER A. BELGODERE. Mexico, D. F., Francisco Marruenda, 1939. Pp. 261.

After a survey of the positions of philosophers, ancient to contemporary, with reference to the question of truth, the author discusses the relation of truth to science, psychology, history, sociology, ethics and religion. He concludes with a statement of his belief that "truth is not of this world."

University of Maryland

ROGER M. BELLOWES



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THEORY OF THE NEURAL QUANTUM IN THE DISCRIMINATION OF LOUDNESS AND PITCH

By S. S. STEVENS and C. T. MORGAN, Harvard University,
and J. VOLKMANN, Columbia University

The advantage of a *quantal theory* of sensory discrimination lies not solely in the fact that it makes explicit the rôle of neural processes that are all-or-none, but also in the fact that it enables us to predict the form and the slope of certain psychometric functions. The *classical theory* left these matters to the operation of a multitude of unknown factors combining in random formations to help or hinder discrimination as chance might have it. This older theory is indeed quite adequate to the data of most conventional experiments in psychophysics, but results of the sort to be presented later seem definitely to elude the classical assumptions.¹ Instead of psychometric functions resembling the probability integral, we find rectilinear functions. Instead of curves of unpredictable slope, we find lines whose slopes are proportional to the differential sensitivity of the *O*. Instead of failure of even the smallest stimulus-increments to produce zero perceptions of increase, we find a critical value below which no increment is ever perceived. Faced with such data, the classical theory helps our understanding not at all, but the quantal theory appears quite capable of taking these results in stride and of accounting, as well, for the traditional psychometric functions in those experiments where rectilinear functions are not obtained. The form of an empirical function depends, of course, upon methods and procedures, but before discussing this vital aspect

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¹This paper reports the results of two experiments. The first, dealing with the problem of intensity discrimination, was carried out by Stevens and Volkman. A brief report of this work has already appeared: S. S. Stevens and J. Volkman, The quantum of sensory discrimination, *Science*, 92, 1940, 583-585. The later work on the quantum of frequency discrimination was carried through by Stevens and Morgan.

of the problem let us first review the salient features of the classical and the quantal theories.

THE CLASSICAL THEORY

What we have taken the liberty of calling the 'classical theory' refers to the assumptions commonly invoked to explain the behavior of an *O* whose task it is to report whenever a detectable increment is added to a stimulus. The human organism can detect a change in the loudness of a tonal stimulus when its intensity is increased by a sufficient increment. But always we find a range of increments which, upon repeated presentation, are sometimes perceived and sometimes not. If we plot the percentage of presentations the *O* hears against the size of the increment presented, the result is a psychometric function. Now, in the usual experiment, where a standard stimulus, *I*, is followed after a short interval of time by a comparison stimulus, $I + \Delta I$, the psychometric function tends to resemble the S-shaped normal probability integral (the phi-function of gamma). The result is similar to the curve we obtain when we shake repeatedly a handful of coins and plot the frequency of the throws on which the percentage of heads exceeds different proportions of the number of coins.² This resemblance of the psychometric function to a cumulative 'chance' distribution leads to the argument "that the stimulated organs, including the brain with its momentary states of equilibrium, are variously disposed toward a given impression. These dispositions are dependent upon a great many different factors ('coins'), each one of which can be favorable or unfavorable for a judgment. . . . These dispositions, being independent, are favorable or unfavorable in chance combinations. For a weaker, or smaller, stimulus [increment] more of them must be favorable than for a stronger, or larger, stimulus [increment]."³

The convention then is to define as the difference-limen that increment which is noticed 50% of the time. It is sometimes argued that when the difference limen is small the slope of the psychometric function must be steep—its *b* must be high; but precisely what slope we are to expect, the theory is unable to disclose. As to whether we should expect ever to perceive *no* increments, or *all* of them, the classical theory, in assuming the phi-function of gamma, says *never*.

THE QUANTAL THEORY

The quantal theory derives from the assumption that the basic neural processes mediating a discrimination are of an all-or-none character. That our knowledge of the nervous system calls for the existence of "sensory quanta" was evident to Boring⁴ in 1926, and he hoped as evi-

² E. G. Boring, A chart of the psychometric function, this JOURNAL, 28, 1917, 281-285.

³ J. P. Guilford, *Psychometric Methods*, 1936, 173.

⁴ E. G. Boring, Auditory theory with special reference to intensity, volume and localization, this JOURNAL, 37, 1926, 157-188. In discussing the all-or-none principle, Troland also speaks of "neural quanta:" L. T. Troland, *Psychophysiology*, II, 1930, 17. Further on, in describing the nerve process (p. 37) he introduces us to the adjective *quantal*.

dence for them to be able to find discontinuities, or steps, in such psychological continua as pitch and loudness. The discontinuities he sought have not appeared, but our researches now show that, if we look for proof in experiments of the proper design, we find sensory quanta revealing themselves precisely as scheduled by theory. The theoretical argument is as follows.

We assume that the neural structures initially involved in the perception of a sensory continuum are divided into functionally distinct units. Békésy⁵ thought of these units as single afferent fibers, but, as we shall see later, the evidence indicates that the functional units are 'larger' than fibers and that they are probably centrally located. A stimulus of a given magnitude excites, at a particular instant, a certain number of these quantal units, and, in order for an increment to be noticeable, it must excite at least one additional quantum. That is the basic picture; but here enter some additional considerations. The stimulus which excites a certain number of quanta will ordinarily do so with a little to spare—it will excite these quanta and leave a small 'surplus' insufficient to excite some additional quantum. This surplus stimulation will contribute, along with the increment, ΔI , to bring into activity the added quantum needed for discrimination. Consequently, at any instant, the size of the increment necessary to add another quantum to the total number excited must depend upon the amount of 'left-over' stimulation.

The next problem is: how much of this left-over stimulation or surplus excitation are we to expect? Here we must raise the question of the over-all fluctuation in the sensitivity of the organism. From the behavior of the absolute threshold of hearing,⁶ for example, we know that this over-all sensitivity fluctuates in 'random' fashion—due perhaps to breathing, heart beat, etc. If such fluctuation is large compared to the size of an individual quantum; it is evident that over the course of time all values of the surplus stimulation occur equally often. In other words, in the presence of a 'steady' stimulus one amount of surplus stimulation contributing toward the excitation of an additional quantum is, at each instant, as likely as any other.

Now, the frequency with which a given stimulus-increment will excite an additional quantum depends upon the frequency with which the surplus

⁵ G. von Békésy, Über das Fechner'sche Gesetz und seine Bedeutung für die Theorie der akustischen Beobachtungsfehler und die Theorie des Hörens, *Ann. d. Phys.*, 7, 1930, 329-359. See also S. S. Stevens and H. Davis, *Hearing: Its Psychology and Physiology*, 1938, 145-147.

⁶ S. Lifschitz, Fluctuation of the hearing threshold, *J. Acoust. Soc. Amer.*, 11, 1939, 118-121.

stimulation exceeds a certain crucial amount, and this occurs a proportion of the time which is dependent directly upon the amount to be exceeded. From these considerations it follows that, if the increment is added instantaneously to the stimulus, it will be perceived a certain fraction of the time, and this fraction is directly proportional to the size of the increment itself.

This argument can be rendered more precise with the aid of a little mathematics. As already stated, we assume that at a given moment a steady stimulus excites completely a certain number of quanta and leaves a small surplus, p , which goes part way toward exciting the quantum next in line; and that the stimulus increment, ΔI , which is required to complete the excitation of this quantum is smaller when the surplus, p , is larger. Now, let us measure the size of a quantum in terms of the increment, Q , which will just succeed *always* in exciting it. Then, the ΔI just sufficient to complement the surplus, p , and thereby excite an additional quantum is given by

$$\Delta I = Q - p \dots \dots \dots [1]$$

A given ΔI will excite an additional quantum whenever $\Delta I \geq Q - p$. Since p fluctuates at random (due to the large over-all fluctuation in sensitivity) between $0 \leq p \leq Q$, this condition will obtain a proportion of the time given by

$$r_1 = \Delta I / Q \dots \dots \dots [2]$$

where r_1 is the relative frequency of the instants during which ΔI excites one additional quantum. The value of r_1 varies between zero and one.

Equation [2] tells us that under certain conditions the psychometric function should be a straight line and that zero increment in the stimulus should produce no responses. Békésy was able to produce data satisfying this equation to a fair approximation, but before we consider the necessary experimental conditions, let us examine another case.

Suppose we provide conditions under which the O is able to report a change whenever *two* additional quanta are excited, but is unable to detect a single quantum. Then equation [2] becomes

$$r_2 = (\Delta I - Q) / Q = \Delta I / Q - 1 \dots \dots \dots [3]$$

and again r_2 varies only between zero and one.

Or, in terms of the percentage, R of the increments which an O should be able to detect

$$R = (\Delta I / Q - 1) \times 100 \dots \dots \dots [4]$$

and R varies between zero and 100.

This equation, derived from the assumption that the addition of *two* quanta is required for a discrimination, also calls for a rectilinear psychometric function, and it is this equation which best describes the data reported below. Its graph is shown by the rectilinear function in Fig. 1. There we have plotted equation [4], using the value of Q as the unit for measuring the stimulus-increment. In these units the slope of the straight psychometric function is exactly determined. Furthermore, when a discrimination requires the addition of two quanta, we note that stimulus-increments of less than one quantum are never detected, whereas those greater than two quanta are always detected.

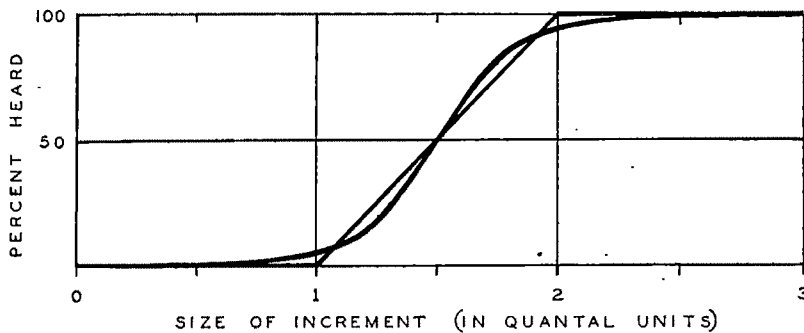


FIG. 1. THE QUANTAL AND PHI-GAMMA FUNCTIONS

The straight line shows the results expected on the basis of the quantal theory. It is the graph of equation [4] of the text. The S-shaped curve was constructed by fitting the phi-function of gamma to the rectilinear quantal function.

EXPERIMENTAL CONDITIONS

It is, of course, one thing to derive an equation but quite another to satisfy the experimental conditions it presupposes. If we are to devise an adequate experimental technique, we must bear in mind particularly the two major assumptions we have made: (1) the existence of fixed neural units or quanta; and (2) the fact of a relatively large over-all fluctuation in the sensitivity of the organism. Since this fluctuation is always in process, it is evident that if we are to determine the effect of a given ΔI , as of a particular instant in time, we must add ΔI instantaneously, and remove it before the organism is able to change in sensitivity by more than a negligible amount. No time-interval between the standard stimulus and the augmented stimulus, and a very brief duration for the latter! Although other precautions are also necessary, it is in these particulars that we must depart most completely from the traditional methodology if we are to obtain rectilinear psychometric functions. A time-interval between stimuli would allow the random fluctuations in over-all sensitivity to manifest themselves in the form of a non-rectilinear psychometric function.

In order to test the quantal theory, Békésy presented a tone lasting 0.3 sec.

followed immediately by a second tone of the same duration but of variable intensity. He recorded the percentage of times *O* heard the second tone as different in loudness from the first one. After what he describes as a month's practice, he was able to obtain results satisfying equation [2], except for a constant difference resembling a time error; but mostly he got data fitting equation [4] again with a constant error. Some of these data, corrected for the error, are shown in Fig. 4. Békésy's *O* usually required the addition of two quanta in order to perform a discrimination.

The design of our experiment was such that the addition of two quanta was always required to invoke a discrimination. The *O* listened to a continuous 1000- \sim tone, one of whose parameters, either intensity or frequency, was momentarily increased at intervals of 3 sec. and his task was simply to press a key whenever he heard an increase and to refrain from pressing when no change was detectable.

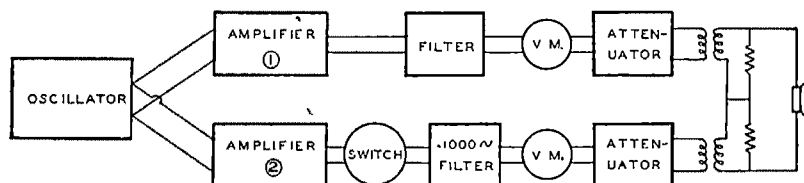


FIG. 2. SCHEMA OF THE APPARATUS

The increment in intensity was supplied to the earphone through the circuit following amplifier 2. Increments of frequency were obtained with a modification of this arrangement in which the switch was used to introduce a tuning capacitance into the oscillator circuit.

(This procedure eliminates the constant error that troubled Békésy.) Under these conditions a discrimination *always* requires two quanta, because the excitation due to the continuous tone, against which the *O* must make his judgment, fluctuates up and down at random by single quantal jumps. From this 'steady'-tone condition only double quantal jumps can be discriminated. Hence, equation [4] always applies, and it is this equation which, in practice, offers the best means of measuring the size of the sensory quantum.

APPARATUS

Only certain crucial aspects of the apparatus need be described in detail. As already indicated, the technical problem was to generate a steady pure tone whose intensity (or frequency) could be increased for a brief period at regular intervals. The apparatus for working with variable increments of intensity is shown in Fig. 2. An oscillator fed a 1000- \sim current into two amplifiers in parallel. The output of each amplifier was led through filters and attenuators to the primary of a transformer. The secondaries of the transformers were connected in series with each other and with an earphone in such a way as to keep the currents in phase. Thus the currents from the two amplifiers summated in the secondary circuit, a fact carefully checked with the aid of a cathode-ray oscillograph and with an electrical wave-analyzer. (The resistors in the earphone circuit were for impedance matching.) The current from the second amplifier was controlled by a rotary switch and was

allowed to pass for a period of 0.15 sec. at 3-sec. intervals. Thus a known increment was added every 3 sec. to the steady tone from the first amplifier.

The 1000- \sim filter in the circuit of the second amplifier calls for special mention. It is a well known fact that the intensity (or frequency) of a tone can not be changed abruptly without the scattering of energy into other regions of the frequency spectrum. The ear hears this scattering as a 'click,' and in an experiment of the sort we are conducting the elimination of this click is imperative. It can be suppressed only by a gradual rather than an abrupt transition from the steady tone to the augmented tone. But, if the transition is too slow, we should no longer be able to measure the quantum, and so we must seek a compromise. From preliminary experimentation we found that, if the transition occupies 0.01 sec., all per-

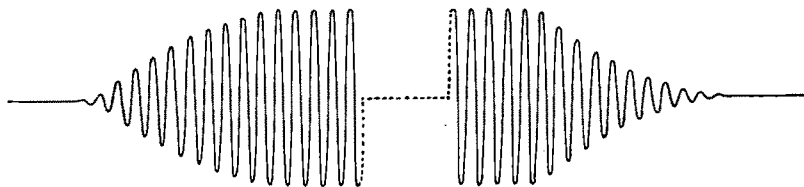


FIG. 3. BUILD-UP AND DECAY OF THE FILTERED INCREMENT

A pulse of a 1000- \sim tone which reaches full amplitude instantaneously and later stops abruptly looks like this after it is passed through a sharply tuned 1000- \sim filter. The dotted portion means that a number of waves have been omitted.

ceptible click is abolished, and the transition-time is still negligible relative to the rate of fluctuation in the over-all sensitivity of the organism.

The transition-time of 0.01 sec. was obtained by passing the increment from the second amplifier through two sharply tuned 1000- \sim filters (General Radio, type 830-R) connected in cascade. These filters make an abrupt transition impossible by eliminating all frequencies except those close to 1000 \sim . Thus, if a train of waves that start and stop instantaneously are sent into these filters, the output at the other end is a train which begins and terminates gradually. Fig. 3 shows this effect. On a cathode-ray oscillograph the increment sent to the earphone is seen to begin and end as shown in Fig. 3. The time-constant for the growth and decay of this wave is 0.005 sec., and the effective time of transition from *on* to *off*, or *off* to *on*, is approximately 0.01 sec.

Fortunately, the same general considerations apply both to a change in frequency and to a change in intensity. Hence, the same filtering arrangement was used for the experiment on frequency. This was possible because the largest increments in frequency used were less than 10 \sim , and 10 \sim is slightly less than the 'width' of the band-pass filters. Because of these filters an abrupt change in frequency was converted into a gradual change, and all perceptible clicks were eliminated. (This method of presenting tones and changes in tones recommends itself on many counts, but its disadvantage is that separate filters are required for different frequencies—and filters are expensive to construct.)

The rest of the apparatus for working with variable increments in frequency has not been diagrammed. It was like the arrangement shown in Fig 2 except

that amplifier (1) was turned off and the rotary switch was removed from behind amplifier (2) and used to insert periodically a variable condenser into the circuit of the oscillator. This condenser was carefully calibrated to produce known changes in frequency. The rotary switch was adjusted to produce every 3 sec. an increment lasting 0.3 sec.

PROCEDURE

We adopted what is perhaps the unorthodox philosophy that the *O* should be given every possible aid and convenience in carrying out his task. He was comfortably seated in a sound-proofed room and asked to press a key with his right hand whenever the steady tone in an earphone changed in loudness or in pitch, as the case might be. The key operated a counter. The apparatus was adjusted to produce a certain increment every 3 sec. and the rotary switch set running. Through a communication system the *O* was then told to begin reporting whenever he was ready. When he was ready—when he was through swallowing, scratching and shifting about—in other words, when by his own criteria he was all set to attend, he rested his left hand on another key which started a recorder to count the number of increments presented. After 25 presentations, lasting 1¼ min., he was told to rest and a new value of the increment was set up on the apparatus. Ordinarily about 200 judgments were obtained at each of several experimental sittings.

Some *O*s found it easier to keep oriented to the increments when a small light was flashed midway between successive increments. Others considered the light somewhat distracting, and for them it was turned off. Thus each *O* was given his choice as to light or no light. Actually, half of them used it and half did not, apparently without producing any systematic difference in the results.

As already stated, each value of the increment was presented 25 times in succession. The first increment presented in any experimental session was one the *O* could report 100% of the time. Thereafter other values of the increment were presented in 'random' order. What we have described is the procedure as it finally evolved—after much testing and exploring. It departs from the more conventional methods in several particulars. *O* reports only when he hears an increment, and he is not required to report both *greater* and *less*. The judgments are made at the rate of about 500 per hr., including rest periods. Furthermore, it was found to be quite unnecessary to vary the size of successive increments in irregular or random order—as many as 25 increments of constant size are presented in succession. The possible *a priori* objections to this seem not to be of much consequence in practice.

THE QUANTUM OF INTENSITY DISCRIMINATION

Our data for intensity discrimination are less extensive than those obtained with frequency as a variable. We wanted principally to test a method and to verify, if possible, the results reported by Békésy.⁷ We worked, therefore, with one ear (left) of a single well-practiced *O* (*J.V.*) and obtained the results shown in Fig. 4. The solid lines in this figure are drawn

⁷ Von Békésy, *op. cit.*

in such a way as to satisfy equation [4] above, provided we interpret Q as the value on the abscissa where the functions first depart from 0%. This value measures, in terms of the stimulus, the size of the differential quantum for intensity discrimination.

As equation [4] predicts, the functions between 0 and 100% are very nearly rectilinear. They obviously do not conform well to the phi-function

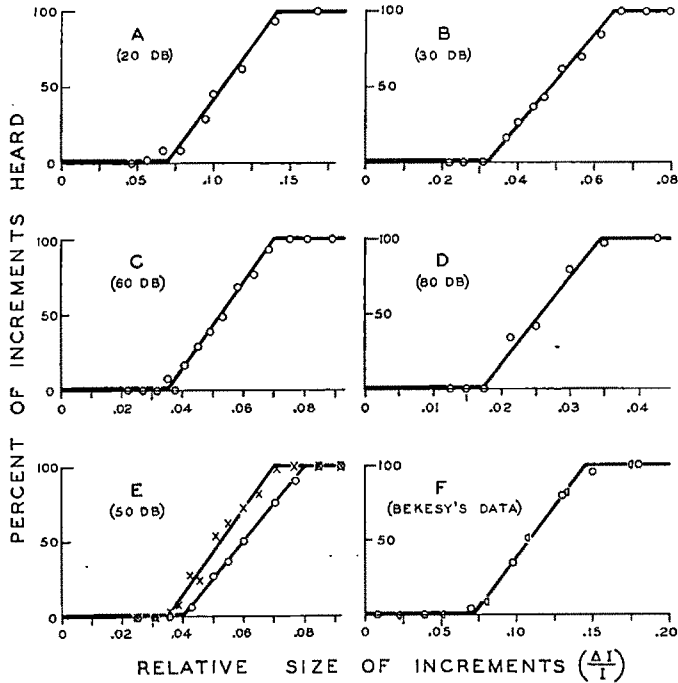


FIG. 4. FUNCTIONS MEASURING THE QUANTUM OF INTENSITY DISCRIMINATION. The solid lines are the theoretical functions and the points show the obtained frequencies with which various increments in the intensity (sound pressure) of a 1000- \sim tone are heard as increases in loudness. The size of the quantum, measured in terms of the stimulus-ratio, $\Delta I/I$, is given by the point on the abscissa where the function first departs from zero. The initial intensity, I , of the 1000- \sim tone (in db. above O 's threshold) is indicated on each plot. Each point is the average of from 50 to 100 observations ($J.V.$). Békésy obtained the circles in plot F for an increase and the half circles for a decrease in intensity (ΔI positive and negative).

of gamma. Furthermore, the points on the abscissa where the functions depart from 0% are exactly one half of the value at which they reach 100%.

It is of interest to compare the size of the quantum, as measured by

the data of plots A, C, D and E of Fig. 4, with the ratio $\Delta E/E$ as measured by the method of beats (Riesz).⁸ Since we have measured intensity in terms of acoustic pressure, our data must first be converted into energy ratios to make them comparable with those of Riesz. When this is done we find that the quantum is roughly half as large as the average differential sensitivity of Riesz's 12 Os. Whether this discrepancy is due to method or to individual differences in sensitivity can not now be stated. Riesz's method did not allow him to obtain psychometric functions.

Our data do, however, confirm Riesz's finding that relative sensitivity improves when the intensity of the stimulus is increased. This fact is evident from inspection of Fig. 4.

It should be pointed out that, strictly speaking, data yielding rectilinear psychometric functions when plotted against sound pressures do not show absolute rectilinearity when expressed in terms of sound energy, but calculation shows that the departure from rectilinearity is negligible. The reason is simply that we are dealing with small differences. Even in the case where the differences are largest (Fig. 4, plot A), transformation of the function into units of sound energy does not alter the shape of the graph by more than the width of the line itself—a thoroughly negligible amount.

Plot B of Fig. 4 represents an artificially small quantum. This is due to the fact that these data were obtained with the apparatus adjusted to produce a more nearly instantaneous transition from the steady tone to the augmented tone. The energy, thereby scattered into other frequency regions, produced a faint click which presumably offered additional cues for discrimination. At higher intensities than 30 db. above threshold, this click was very noticeable and rectilinear psychometric functions could not be obtained. It would seem, therefore, that the effect of transition time on the measured size of the quantum varies with intensity. The precise nature of this effect remains to be determined.

Plot E of Fig. 4 shows that the size of the quantum as measured by the stimulus does not remain invariant under all conditions. The same is also true of the quantum of frequency discrimination. What factors influence the size of the quantum we do not know, but the fact that it can change should caution us against trying to test the quantal theory by averaging data taken at different sessions. If the quantum changes during the collecting of the data, the psychometric function tends to assume a sigmoid form, as is apparent if we average the data of the two functions in plot E.

⁸ For a table of Riesz's results see Stevens and Davis, *op. cit.*, 140.

(Actually, in our later work on frequency discrimination we became impressed more by the stability of the quantum than by its ability to change. Most of our measurements showed high repeat reliability.)

THE QUANTUM OF FREQUENCY DISCRIMINATION

The results for frequency discrimination show what to us is remarkable similarity to those obtained for intensity. Psychometric functions for six

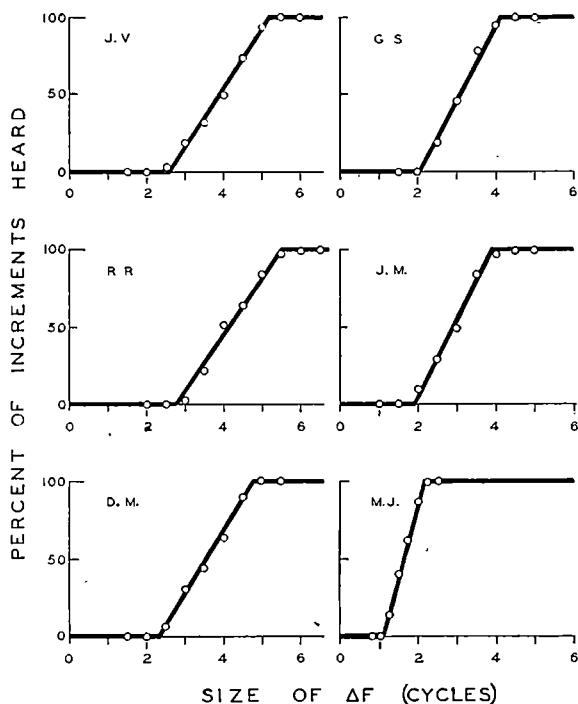


FIG. 5. FUNCTIONS MEASURING THE QUANTUM OF FREQUENCY DISCRIMINATION FOR SIX DIFFERENT *O*s

In each case the stimulus was a 1000- \sim tone 54 db. above threshold. The solid lines were drawn so as to fit the points and at the same time satisfy equation [4] of the text. Each point is the average of 100 judgments.

different *O*s are shown in Fig. 5. Each *O* had made approximately 800 judgments before these data were recorded. The functions themselves are based upon 800 to 1000 judgments each. (Work with two other *O*s was discontinued after 2 hr. practice because they failed to settle down to the point of giving reasonably consistent results.)

The data of Fig. 5 were obtained for a 1000- \sim tone at 54 db. above threshold. Here, as in Fig. 4, the two criteria of a 'good' psychometric quantal function are fulfilled: (a) there is rectilinearity to a high degree; and (b) a two-to-one ratio obtains between the value at which the function reaches 100% and the value at which it first departs from 0%. These

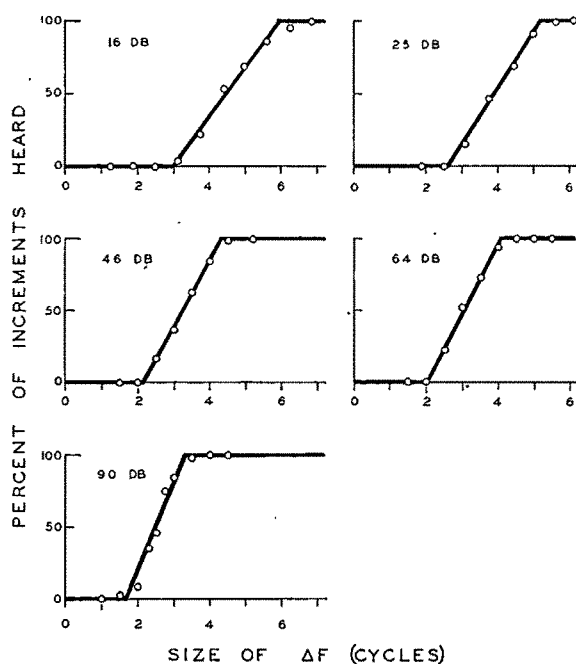


FIG. 6. FUNCTIONS FOR A SINGLE *O* (S.S.S.) MEASURED AT FIVE DIFFERENT SENSATION-LEVELS

Each point is the average of 100 judgments.

two criteria are met despite wide variation in the sensitivity of different *O*s. Among these *O*s we find quanta ranging in size from 1.1 to 2.8 \sim .

Not only is there variability among individuals but for a given *O* the measured size of the quantum varies with the intensity of the 1000- \sim tone. Psychometric functions obtained at different sensation-levels (db. above threshold) are shown for one *O* (S.S.S.) in Fig. 6 and for another *O* (C.T.M.) in Fig. 7. In each case the quantum is larger at low intensities and smaller at high. This dependence of the size of the quantum upon intensity can be seen in Fig. 8. There we see that the sensitivity of each *O* is related in a similar fashion to the sensation-level of the

stimulus. Furthermore, the difference in sensitivity between these *O*s remains rather constant at all intensities.

In Fig. 8 are also plotted the data of Shower and Biddulph (triangles) on differential sensitivity to frequency. These investigators exposed 10 ears to a tone whose frequency was modulated twice per sec. and determined the least perceptible amplitude of modulation by what appears to be the method of limits. The average values for 1,000~ at various sensation levels are seen, in Fig. 8, to be definitely larger than the respective quanta

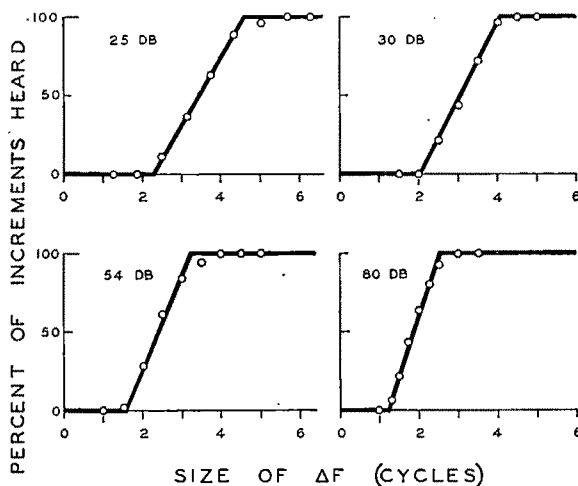


FIG. 7. FUNCTIONS FOR A SINGLE *O* (*C.T.M.*) MEASURED AT FOUR DIFFERENT SENSATION-LEVELS

Each point is the average of 100 judgments.

of the two *O*s, (*S.S.S.* and *C.T.M.*). This difference is most likely due in part to method and in part to the fact that the size of the quantum, as we have measured it, is systematically smaller than the conventional measure of the difference limen—the 50-% point on the psychometric function. As demonstrated in Fig. 1, the quantum is exactly two-thirds the size of the difference limen measured by this 50-% point. When we take this fact into account, we find reasonable agreement between our data and those of Shower and Biddulph. Better still is the agreement between our data and the few data reported by these authors from an experiment in which they employed an abrupt transition between the standard and the comparison tones.

An interesting phenomenon appeared when we tried to experiment

with very faint tones of the order of 10 db. above threshold. At these intensities the *O*s found that the fluctuations in the loudness of the steady tone were sometimes large enough to leave them in doubt as to whether the tone was still present. Under these conditions rectilinear quantal functions were not obtained.

STATISTICAL TESTS

The 15 rectilinear psychometric functions in Figs. 5, 6, and 7 were fitted by inspection in such a way as to represent the data and at the same time

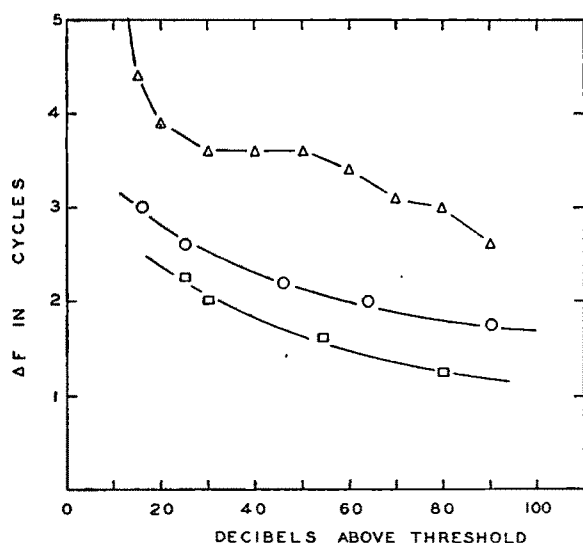


FIG. 8. DIFFERENTIAL SENSITIVITY TO FREQUENCY AS A FUNCTION OF INTENSITY. The rectangles (C.T.M.) and circles (S.S.S.) show the size of the quantum of frequency discrimination at various sensation-levels. The triangles represent the *DL*s for frequency as measured by Shower and Biddulph at 1000 \sim .

to satisfy equation [4]. These functions meet the two criteria derived from the quantal theory: rectilinearity and a two-to-one ratio between the values of the functions at the 100-% and 0-% points. Now the question arises as to how well, by mathematical test, the data fit equation [4]. We shall attempt an answer by showing first that rectilinear functions fit the data better than do phi-functions of gamma, and second that these rectilinear functions tend to exhibit the required slopes.

By the method of least squares rectilinear functions were fitted to the

data of Figs. 5, 6 and 7, and by the method proposed by Boring,⁹ which utilizes Urban's weightings, the ϕ -function of gamma was fitted to these same 15 sets of data. In each case ΔF was taken as the independent variable. In keeping with conventional procedure, extreme values of the dependent variable (percentage of increments heard) were not used. Points falling

TABLE I

GOODNESS OF FIT OF THE PHI-GAMMA HYPOTHESIS AND OF THE QUANTAL HYPOTHESIS STATED IN TERMS OF P-VALUES AND OF $(\Delta F)_{100}/(\Delta F)_0$

(The composite P-values of the right-hand column were obtained by adding the chi-squares, together with the number of degrees of freedom, of the P-values from the row in which the composite P-values appear.)

VARIOUS OS AT A SENSATION-LEVEL OF 54 DB.

	Observer						Composite values
	J.V.	G.S.	R.R.	J.M.	D.M.	M.J.	
P for $\phi(\gamma)$ function	.19	.26	.03	.31	.16	.38	.03
P for rectilinear f.	.56	.66	.48	.56	.83	.94	.92
$(\Delta F)_{100}/(\Delta F)_0$	2.09	1.91	1.97	2.18	2.07	1.89	2.02

DIFFERENT SENSATION-LEVELS FOR S.S.S.

	Sensation-level (db.)					Composite values
	16	25	46	64	90	
P for $\phi(\gamma)$ function	.36	.44	.12	.44	.07	.08
P for rectilinear f.	.29	.80	.50	.57	.13	.37
$(\Delta F)_{100}/(\Delta F)_0$	2.12	1.93	1.89	2.13	1.96	2.01

DIFFERENT SENSATION-LEVELS FOR C.T.M.

	Sensation-level (db.)				Composite values
	25	30	54	80	
P for $\phi(\gamma)$ function	.53	.34	.06	.48	.19
P for rectilinear f.	.72	.95	.63	.88	.95
$(\Delta F)_{100}/(\Delta F)_0$	2.34	1.95	2.20	2.19	2.17

below 3% and above 97% were neglected for the purpose of curve-fitting because of the undue influence they would have on the constants of the fitted functions. After fits had been obtained, the chi-square test of goodness of fit was applied and the P-values determined.¹⁰ In fitting both types of functions we took as the number of degrees of freedom two less than the number of points to be fitted.

⁹ E. G. Boring, Urban's tables and method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

¹⁰ For the method used, see, Guilford, *op. cit.*, 1936, 176 ff.

Table I presents these P-values, together with the measures of slope, $(\Delta F)_{100}/(\Delta F)_0$, of the fitted rectilinear functions. In 14 of the 15 sets of data the P-values for the rectilinear functions are higher than those for the phi-functions of gamma. In general, the P-values for the function predicted by the quantal theory are above 0.5, whereas those for the phi-functions of gamma are less than 0.5.

A more decisive difference between the goodness of the fits can be demonstrated if we take a composite of the individual P-values. We add the chi-squares from which each P was derived, count up the total degrees of freedom, and enter the chi-square tables in the usual manner. The results of these operations are recorded in Table I. We note that the composite P-values for all 15 sets of data taken together are 0.931 when the rectilinear functions are fitted, and only 0.008 when the phi-functions of gamma are tested. This comparison not only favors the function derived from the quantal theory, but, by conventional standards, it makes the classical hypothesis quite unacceptable as a description of the data.

This clear difference between the rectilinear function and the phi-function of gamma emerges in the face of a fact well illustrated in Fig. 1. There we see that when the phi-function of gamma is fitted directly to a rectilinear function by the method of least squares, the absolute differences between the two functions are nowhere very great. The two functions cross at three points, and at these points it is impossible experimentally to differentiate between the two theories. Even where the functions are maximally separated the discrepancies are not large. We should therefore expect random deviations in the observed proportions often to affect the fit of one function as much as the fit of the other, and in general we find that low P-values for the rectilinear functions tend to be associated with low values for the phi-function of gamma. The rank-difference correlation is 0.52. Fluctuations in the observed proportions have therefore *tended* to affect the fits of the two theoretical functions in the same way. This fact makes it easy to understand why, as far as the individual P-values are concerned, there is only a general tendency for the quantal hypothesis to be superior to the phi-gamma hypothesis, and why it is necessary to combine a large number of observations in order to obtain so clear a distinction between the applicability of the hypotheses that the phi-function of gamma can be rejected.

The second criterion derived from the quantal theory is that the ratio between the value of ΔF , where the rectilinear function reaches 100%, and the value where it departs from 0% should be equal to 2. We have computed these ratios, $(\Delta F)_{100}/(\Delta F)_0$, from the rectilinear functions

fitted by the method of least squares, and they are recorded in Table I.

In all 15 sets of data for frequency discrimination, these ratios do not exceed 2.34, nor are they ever smaller than 1.89. The average ratio for the 6 *O*s tested at a sensation level of 54 db. was 2.02; for one *O* (*S.S.S.*) tested at different sensation levels, it was 2.01; and the average ratio of the other *O* (*C.T.M.*) for comparable tests was 2.17.

It appears reasonable, therefore, to conclude that our experimental findings have fulfilled the predictions of the quantal theory not only in respect of goodness of fit, but also with respect to what we believe is the more crucial criterion of slope.

THE QUANTUM AS A MEASURE OF DIFFERENTIAL SENSITIVITY

Altogether we have recorded more than 30 rectilinear psychometric functions showing good agreement with equation [4]. That it is possible to design experimental conditions capable of testing the quantal theory appears, therefore, to be reasonably assured. Since we have not explored by systematic variation all the parameters of our experimental conditions, we can not say with finality, however, just what factors are important and which are irrelevant from the point of view of method. Nevertheless our experience suggests that some of the conditions necessary in order to obtain unobscured rectilinear quantal functions are these.

(1) The experiment must be 'easy' from the point of view of the *O*. He needs optimal conditions for stabilizing his attention and his criteria of judgment. He usually needs a period of practice.

(2) Data need to be taken with sufficient rapidity to obviate the necessity of averaging results from widely separated experimental sessions. Our rate of 20 judgments per min. may not be optimal, but it is apparently an adequate rate.

(3) For some *O*s it seems advantageous to supply a 'warning' signal, such as a light, to indicate when they should attend. This enables them to adjust their breathing to the rhythm of the stimuli and relieves them of the necessity of sustained attention.

(4) It is of crucial importance that the interval of time between the presentation of the standard stimulus and the augments stimulus be negligible. Data satisfying equation [4] are not obtained when the over-all sensitivity of the organism is allowed to change during the interval between the two stimuli. On the other hand, it appears that the quantal functions are obscured or distorted when the transition between stimuli are so abrupt that unwanted transients are introduced.

Given an experimental method adequate to reveal the size of the individual quantum of sensory discrimination, we have open to us a new approach to the problems of differential sensitivity. The quantum is plainly simpler, more elemental, more basic than the classical limen which is a

statistical average of a set of indeterminately varying values. Perhaps, then, we ought to measure the difference limen not by the size of the increment heard as greater half of the time, but in terms of the quantum itself. The quantal theory proposes that certain aspects of the rectilinear psychometric function reflect the magnitude of a 'unit process' somewhere in the nervous system. It seems reasonable to suppose that the psychophysicologist will find a measure of this unit process to be a more significant datum than the somewhat arbitrary measure of sensitivity conventionally employed.

Applied to the data of the present experiments, the conventional measure of sensitivity—in terms of the 50-% point—gives values 1.5 times as large as the respective quanta. How general is this relation? In those psychophysical researches where experimental method precludes rectilinear functions is it possible to measure the quantum of discrimination merely by taking two-thirds of that increment which yields as many positive as negative reports? That this simple operation should be always valid seems hardly likely, but that it sometimes works we have been able to demonstrate in preliminary experimentation with 3 *Os*.

Instead of no interval of time between the standard and the augmented stimulus, a separation of 0.5 sec. was introduced. In one series of observations the standard stimulus was presented first, and *O* pressed a key when the second tone appeared higher in pitch than the standard; in another series the standard was presented second and *O* pressed only when he heard an apparent reduction in pitch. When the results from both series were averaged, non-rectilinear psychometric functions were obtained. These functions were typical of those usually obtained under such conditions, and at the 50-% point these functions coincided with the rectilinear functions obtained from the respective *Os*.

The effect of a time-interval between the standard and comparison stimuli reveals itself as a decrease in the slope of the psychometric function and a departure from rectilinearity. As a matter of fact, the precise form of the function for separated tones can be predicted from the assumptions of the quantal theory. Although the general equation for this function (involving the sum of three integrals) has been derived, an adequate experimental test of its predictive power has not been completed, and here we state merely that the equation predicts non-rectilinear psychometric functions for the condition of stimuli separated in time. For separated tones the predicted functions are not normal probability integrals, although their departure from probability integrals is never more than about 10% at any value.

THE NATURE AND LOCUS OF THE QUANTUM

What one cares to surmise about the mechanism responsible for what we have called 'quantal functions' must be mainly speculative. Our own opinion is that the 'neural unit' is functional rather than anatomical. The organism behaves as though a definite increment of excitation, or potential, or chemical concentration is needed at some central locus in order to enlist a 'final common path' and thereby produce a 'key-pressing.' Conceivably the organism behaves in this respect much like a relaxation oscillator¹¹—a potential builds up to a critical value and the system discharges. The critical value is analogous to the quantal increment.

Three reasons prompt us to reject Békésy's suggestion that the neural unit is a nerve fiber and to propose the notion that the quantum is a central rather than a peripheral phenomenon.

(1) The critical increment in 'excitation' (the quantum) is of no fixed value. Although throughout many experimental sessions the quantum remains sufficiently constant to produce rectilinear psychometric functions, changes in its magnitude are occasionally observed.

(2) There are more nerve fibers in the auditory nerve than there are quanta in a given sensory attribute. Complete data for determining the precise number of quanta of pitch and of loudness remain to be obtained, but the size of the quanta thus far measured would indicate that several fibers might conceivably be involved in the production of a quantal discrimination.

(3) For binaural listening the size of the quantum is generally smaller than for measurements made on a single ear. Fig. 9 shows typical results for frequency discrimination. When the stimulus and its increment enter both ears, the quantum is reduced to about two-thirds of its monaural value. The results in Fig. 9 are typical of 3 Os, but with one O (S.S.S.) no reliable difference between binaural and monaural listening was detectable in two different experiments.

If the quantum were dependent upon the addition of a single afferent nerve fiber (in the case of loudness) or upon the substitution of one fiber for another (in the case of pitch), we should expect that binaural listening would alter the psychometric function, but not in the manner illustrated in Fig. 9. Listening with two ears would presumably increase the probability of activating another nerve fiber. Or, considering inverse probabilities, we can say that a stimulus having 0.5 probability of failing to activate an additional fiber in one ear would have only 0.25 probability of failing in two ears. Extending this reasoning to other values we find that the peripheral theory of the quantum leads us to expect, for binaural listening, a curvilinear psychometric function coinciding with the rectilinear monaural

¹¹ For a discussion of this analogy see Balh. van der Pol, *Beyond radio*, *Proc. World Radio Convention*, Institution of Radio Engineers, Australia, 1938.

function at 0 and 100%. Instead, the typical finding is a rectilinear function satisfying the two criteria of a quantal function. Apparently, then, the two ears coöperate in some *O*s to produce 'excitation' at the functional center where the quantum is located.

One additional fact ought to be mentioned. For a given increment the *O* either hears a change or he does not. In this respect, the discrimination is an all-or-none phenomenon. Of those increments he hears, all are not, however, of the same subjective magnitude. That is to say, most *O*s report that some of the increments they perceive are larger and plainer than

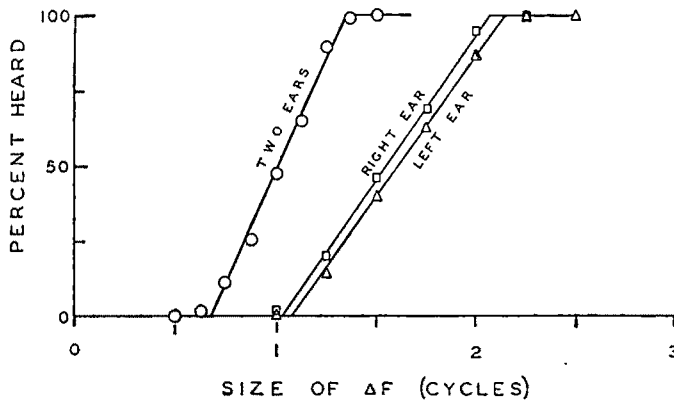


FIG. 9. FUNCTIONS OBTAINED FROM THE SAME *O* (M.J.) FOR MONAURAL AND BINAURAL LISTENING

others. Increments heard 80% of the time tend to be subjectively larger than increments heard only 20% of the time. It would appear, therefore, that the judgment of magnitude is to some degree independent of the all-or-none judgment of presence or absence. Of course, if the increment is not perceived as present, its subjective magnitude cannot be reported, but it seems that different perceptions of magnitude can follow the excitation of a single discriminatory quantum. Conceivably, the perception of magnitude could in turn be a quantal phenomenon involving quanta of smaller size than those mediating the discrimination of presence or absence; but on these problems only speculation is at present possible.

SUMMARY

We have presented experimental evidence that the neural processes determining differential sensory sensitivity are better understood in terms of a concept of neural quanta than in terms of a random variation of sensitivity as subsumed under the phi-gamma hypothesis.

Because the assumptions underlying the quantal and the classical theories lead to different mathematical predictions regarding the form of psychometric functions, it is possible to devise experimental conditions to be employed as a crucial test of the two theories. The principal condition required is a nearly instantaneous transition from the standard to the comparison stimulus. Under these circumstances, the classical theory predicts that the relation between the proportion of increments heard and the size of the increment will be described by the phi-function of gamma, but the quantal theory shows that this relation will be rectilinear and, moreover, when two additional quanta are required for discrimination, that the smallest increment which is always heard will be two times the largest increment, which is never heard.

Data obtained when the intensity of the 1000- \sim tone is given brief periodic increases are adequately described by rectilinear functions fulfilling the two-to-one relation as predicted by the quantal theory. More extensive experimentation with judgments of differences in frequency by 8 *O*s yielded similar results. Moreover it was shown that naïve *O*s give results in accordance with the quantal theory just as consistently as do trained *O*s, and that satisfactory quantal functions can be obtained with tones of widely different intensities.

These conclusions are supported by a statistical analysis in which the phi-function of gamma and the rectilinear function were fitted by the method of least squares to 15 sets of psychometric data. That the *P*-values derived from the chi-square test of goodness of fit favor the quantal hypothesis is evidenced by a composite coefficient of 0.008 for the phi-function of gamma and 0.931 for the rectilinear function. In addition, the slopes of the best-fitting rectilinear functions are uniformly those predicted by the quantal theory.

Not only do our results support the quantal theory and lead us to reject the phi-function of gamma, but they supply us with a precise measure of the size of the differential quantum. We have suggested therefore that differential sensitivity be expressed in terms of the quantum. In certain instances the *DL*, as classically defined, may be converted into a quantal measure by multiplying it by the factor 2/3.

The size of the quantum is determined by several parameters. It is quite different, of course, for different *O*s tested under similar conditions. The size of the quantum also varies with tonal intensity and we have presented curves of this relationship in the present paper. For 3 *O*s, the quantum for frequency discrimination under binaural stimulation is about two-thirds the size of the monaural quantum; but with one *O* we found no difference between monaural and binaural listening.

The fact that the two predicted consequences of the quantal theory are met as well in binaural as in monaural stimulation, even though the size of the quantum may vary, leads us to believe that the quantum is centrally, not peripherally, located. That it is a structural unit such as the neuron seems unlikely, but somewhat more reasonable is the view that it is some *functional* unit of the nervous system involving a number of fibers.

THE DEPENDENCY OF VISUAL ACUITY ON ILLUMINATION AND ITS RELATION TO THE SIZE AND FUNCTION OF THE RETINAL UNITS

By CURT BERGER, Cornell University

One of the best known functions of the human eye is the improvement of visual acuity with increasing degree of brightness. Since Donders¹ and Snellen² created their test-objects, consisting of special letters of different sizes, this function has been investigated by a large number of experimenters.³

König⁴ was one of the first who found by very careful experiments that visual acuity increases with increase of illumination, slowly at first, then very rapidly

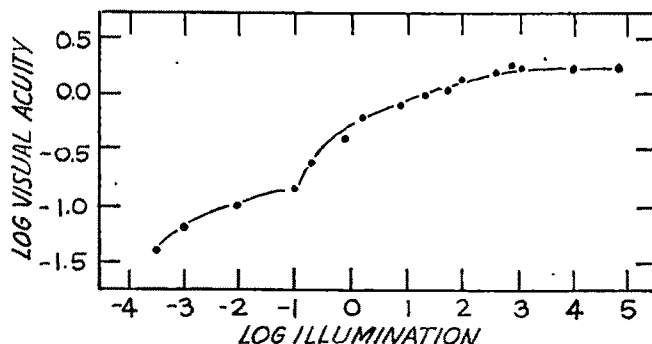


FIG. 1. DEPENDENCY OF VISUAL ACUITY ON ILLUMINATION: KÖNIG, 1897

until it reaches a constant level, at which a further increase of brightness has no more influence. This classical curve of König has been criticized with respect to its form and the kind of test-object used, but even the most recent investigators

* Accepted for publication October 13, 1940.

¹ F. C. Donders, *Die Anomalieen der Refraction und der Accommodation des Auges*, 1866.

² H. Snellen and E. Landolt, *Die Funktionsprüfungen des Auges*, in Graefes-Saemisch's *Handb. d. ges. Augenheilkd.*, 3, 1879.

³ Cf. F. H. Adler, Problems in physiology of visual acuity, *Arch. Ophthalm.*, 11, 1934, 6-19; H. Guillery, Sehschärfe in Bethe's *Handb. norm. u. pathol. Physiol.*, 12, 1931, 745-808; J. P. C. Southall, *Introduction to Physiological Optics*, 1937, 1-404; L. T. Troland, An analysis of the literature concerning the dependency of visual functions upon illumination intensity, *Transact. of the Illuminat. Engineering Soc.*, 26, 1931, 2-107; W. W. Wilcox, The basis of the dependence of visual acuity on illumination, *Proc. Nat. Acad. Sci. Wash.*, 18, 1932, 47-56.

⁴ A. König, Die Abhängigkeit der Sehschärfe von der Beleuchtungsintensität, *Sitz. Ber. d. Akad. d. Wiss., Berlin*, 1897, 559-575.

agree that visual acuity improves with increasing illumination.⁵ The curve obtained by Shlaer⁶ coincides surprisingly with König's classical experiments.

In order to understand the problems involved in these experiments we have to examine (1) the actual conditions used for measurements of visual acuity, and (2) the theoretical explanation for the physiological mechanism, the limit of which is supposed to be measured by visual-acuity tests. Both points have in common the concept of 'minimum visual angle' upon which they are based.

The concept of minimum visual angle was originally derived as a theoretical consequence of physical optics. Every optical system has a limit in its ability

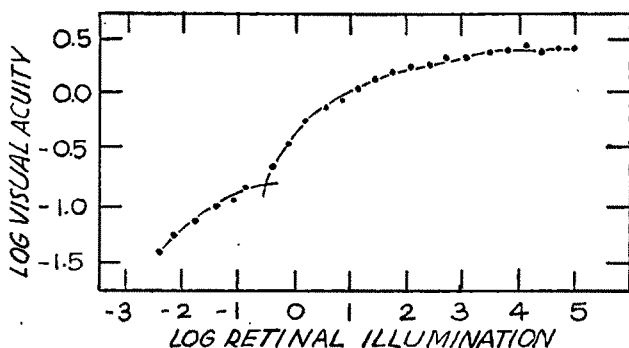


FIG. 2. DEPENDENCY OF VISUAL ACUITY ON ILLUMINATION: SHLAER, 1936

to reproduce the image of an object and this limit is called 'resolving power.' It is measured by the minimum distance between two points which the optical system is still able to reproduce as two separate images. As the points are brought still closer together, their images will fuse and become one point. The optical system can no longer resolve the images. If the retina were an ideal screen and no other factors influenced the visual power of the human eye, the observed minimal distance between two points would be a direct measure of the resolving power of the dioptrical system of the eye. It is a well known fact, however, that the capacity of the retina to resolve two points is much less than that of the dioptrical system alone. It is impossible, therefore, to measure the resolving power of the dioptrical system of the eye by any sensory reaction. Physiologists have, nevertheless, based their terminology upon that used in physics; and they define the

⁵ R. J. Lythgoe, Medical Research Council, Reports of the Committee upon the Physiology of Vision: X. The measurements of visual acuity, *Spec. Rep. Ser.*, 173, 1932, 1-72; Selig Hecht and E. U. Mintz, The visibility of single lines at various illuminations and the retinal basis of visual resolution, *J. Gen. Physiol.*, 22, 1939, 593-612.

⁶ S. Shlaer, The relation between visual acuity and illumination, *J. Gen. Physiol.*, 22, 1937, 165.

'resolving power of the human eye,' as the minimum visible distance between two luminous points.⁷ Physiologically regarded, however, luminous points are the only stimuli of the retina; black spots are the consequence of the absence of light and cannot stimulate the retinal units.

The minimum visible distance between two points is not, however, altogether satisfactory for the definition of the resolving power of the human eye, because the eye accommodates to different distances and so the minimum visible distance between two points will change with the distance from the eye. The angle formed by the two points and the middle-point of the eye, remains constant, however, if the minimum visible distance between two points and the distance from the eye are proportional. This supposed constancy is the reason for using the 'minimum visual angle' as a measure for an optical function and it was the basis for the construction of Snellen's and Landolt's test-objects. The importance of this concept of a 'minimum visual angle' was increased by its normal value. Many observations, especially one with two stars (luminous points) cited by the astronomer Hook,⁸ seemed to prove that the normal minimum visual angle was 60". In terms of retinal distance this corresponds to the diameter of about one single cone.

In accordance with these assumptions, the fundamental physiological basis for understanding visual acuity has always been the histological structure of the retina, *i.e.* the fact that it consists of millions of rods and cones, which were supposed to have isolated nerve-connections with the central parts of the nervous system. The general physiological supposition was that two points (or details) can be recognized as separated only if there is one non-stimulated cone the diameter of which corresponds to the normal minimum visual angle of 60" lying between two stimulated cones. If this is true, why should this function improve when illumination improves? Since the histological structure of the retina remains unchanged, the minimum visual angle should also remain constant under different degrees of brightness and visual acuity should not be affected by changes of illumination. The contradiction between facts and theory is evident. Although physiologists as well as psychologists have attempted to resolve the problem by new theories, a satisfactory and detailed explanation of the relation between visual acuity and illumination had not been established.⁹

It was, therefore, of great interest when Hecht proposed a new interpretation of König's classical curve.¹⁰ Hecht states that the retinal elements (cones) have different thresholds for brightness so that with low degrees of illumination only a small number of cones is in function. When brightness increases, the number of functioning cones also increases. As a result, it becomes possible for more and more details to be recognized as brightness, and consequently, the number of functioning elements per unit increases. In a recent paper,¹¹ he proposes further-

⁷ H. von Helmholtz, *Physiologische Optik*, 1896; F. B. Hofmann, *Physiologische Optik*, 1920.

⁸ R. Hook, *Posthumous Works*, 1705, 12.

⁹ Cf. J. von Kries, *Allgemeine Sinnesphysiologie*, 1923, 1-299; W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920, 1-263.

¹⁰ Selig Hecht, The relation between visual acuity and illumination, *J. Gen. Psychol.*, 11, 1928, 255-282.

¹¹ Hecht and Mintz, *op. cit.*, 593-612.

more, that visual acuity is not as much a matter of the diameter of the individual cones as it is the brightness-differences between the images on different rows of cones.¹²

From the concept of the 'minimum visual angle' mentioned above the actual conditions under which measurements of visual acuity are made, are as follows.

(a) *Form and proportion.* Visual acuity is defined as the ratio between the distance in which a special test-object is just recognized by an observer and the distance of 6 m., which has been chosen as standard distance. In consequence of the above mentioned assumptions Snellen's letters are constructed in such a way that their details which have to be recognized by a normal eye, appear under a visual angle of 60"; Landolt's broken circle has a gap which a normal eye is supposed to just recognize under an angle of 60". The proportions of the details of the letters (or the gap in the circle) to the total height of the letters (or the diameter of the circle) has been chosen as 1 to 5. No reason for this proportion has ever been given, but it is obvious that capital letters as used by Snellen cannot be constructed unless they have five divisions in their vertical cross-cut. If the details of the letters are chosen as 60", the height of the letters has consequently to be 5'. Snellen constructed his letters first, Landolt adapted the dimensions of his ring to the proportions of 1:5—the same as Snellen's letters.

(b) *Size and distance.* Both Snellen and Landolt, in accordance with the laws of physical optics, considered a change in the distance between the test-object and the eye as equal to a change of the size of the test-object in a constant distance from the eye. To facilitate measurements of visual acuity (especially for the practice of ophthalmology), they fixed the distance between eye and test-object to 6 m. Adapting the size of one special test-object to be recognized normally (the details appearing under a visual angle of 1') at that distance, they constructed test-objects of different sizes on the same table for measurements of deviations from the normal visual acuity.

(c) *Type of illumination and retinal adaptation.* Originally no special conditions of illumination had been prescribed for measurements of visual acuity in the practice of ophthalmology, because they were supposed to be made under normal daylight. Thus the eye was kept light-adapted and the illumination fairly standardized. For controlled experiments artificial light, which is reflected light, is generally used for the illumination of the test-objects. Consequently, some light will also be reflected from the dark areas of the test-object and the corresponding parts of the retina will also be stimulated. When brightness increases, the stimulus-intensity of those dark regions on the retina will also increase, but less than the intensity of the white regions. With increase of brightness the absolute difference of the stimulus-brightness of these two regions on the retina will increase.

It is important that the state of adaptation be constant. If, before measuring visual acuity, the eye is dark- or light-adapted only for a short time, measurements may be influenced incalculably, because adaptation and brightness discrimination,

¹² Cf. also H. Hartridge, Visual acuity and the resolving power of the eye, *J. Physiol.* 57, 1922, 52; S. W. Kravkov, in Gräfe's *Arch. Ophthalm.*, 13, 1933, 452.

which are involved in measurements under these conditions, may be different at different degrees of brightness.

In order to discover the importance of these conditions, the experiments reported here were conducted.

APPARATUS AND PROCEDURE

(1) *Variation of distance.* Measurements of the minimum visual angle were made by continuously changing the distance between two small squares until they were just recognized as separate or appeared to fuse into one single object again. Those double-measurements were repeated four times in one day under the same conditions. On a later day the same procedure was followed. Thus each point in the curves represented in this paper is the average value of 16 single measurements.

It should be supposed in analogy to physical measurements that the minimum visual angle was the same whether obtained by widening the distance between the squares or starting from a great distance between the squares by bringing them closer and closer together. That is not true, however. Two different values are obtained and the amount of this difference depends upon various factors. One factor is the speed of changing the distance, while measurements are made; another is the degree of brightness. The higher the brightness, the greater the difference. Although it is difficult to give a satisfactory explanation of this fact, the results may at the time being be simplified by considering only the average value of all measurements under equal conditions. Therefore, in the curves of this paper, only those average values are represented.

(2) *Illumination.* The light-sources were located in a wooden box. The test-objects at one side of this box were lighted from behind so that no light was reflected from the dark parts of the objects.

The intensity which was changed by the use of a variable grating in front of the light-source, was first directly measured for each step with a photo-sensitive cell. Then the retinal illumination (I) was calculated by the following formula: $I = a^2 L (\pi/4) p^2 / x^2 \sigma^2$, where a is the distance between the photosensitive cell and the luminous area, L the amount of measured lux, p the diameter of the pupil, x the distance between test-object and subject and σ the size of the retinal image corresponding to the luminous area.

(3) *Distance.* With the exception of the first experiments, the distance between test-object and O 's eyes was kept constant at 10 m.

(4) *Adaptation.* For the measurements of the minimum visual angle, only one eye was used. O 's head was surrounded by a light-frame which contained about 32 light sources of 20 w. each. The light from these sources was kept inside the frame, from which it could only be reflected and reach the eyes indirectly. Furthermore three bulbs of 100 w. each were fixed at the sides and over O 's head so that enough reflected light fell into his eyes to keep them both light-adapted throughout the experiments. No light fell on the test-object from outside the box.

(5) *Fixation of head.* O 's head was held firmly in position by means of a hard

rubber biting board, made especially for him. No head-movements occurred during the measurements.

RESULTS AND DISCUSSION

(1) *Reliability of measurements.* It is a striking fact that no one has earlier employed luminous points to measure the dependency of the minimum visual distance upon illumination, although very many kinds of other test-objects have been used. The reason may have been, in addition to technical difficulties, that these measurements were not believed to be exact. When letters, or even the broken circle, are used, there is a secondary control. If the observer indicates a false letter or a false position of the broken circle, it is a proof that he really did not recognize the test-object and his observations can be rejected. Such a secondary control is almost impossible by using two points as test-objects. The reliability of such measurements, however, can be controlled by repeating the series a great many times under equivalent conditions; the average error and its constancy is then a measure of the exactness of the method. By a great number of repeated series of measurements with two luminous points, it has been proved, that the average value of the minimum distance on different days is surprisingly constant. Furthermore it is possible to detect in this way very slight changes in the eye's sensibility which cannot be detected by the usual method of measuring visual acuity.¹³ It can, therefore, be concluded that measurements of the minimum visual angle with two luminous points are even more exact than with most other test-objects.

(2) *Effect of distance: (a) Reflected light.* What is the experimental evidence for the supposed constancy of the minimum visual angle at different distances from the eye? There is none; indeed, to the contrary, a large number of measurements have shown that the minimum visual angle *increases* with an increasing distance of the test-object from the eye, if the latter consists of black or white spots illuminated from the side of the observer.¹⁴ In those cases then the 'minimum visual angle' cannot be used as a measure for the resolving power of the eye.

(b) *Transmitted light.* If two small luminous squares or spots of threshold brightness on an almost absolute black background are used, the

¹³ Curt Berger, Weitere Untersuchungen über die Unterschiedsempfindlichkeit (Auflösungsvermögen) des emmetropen Auges, *Skand. Arch. f. Physiol.*, 74, 1936, 27-62.

¹⁴ E. Freeman, Accommodation, pupillary width and stimulus distance, *Opt. Soc. Amer.*, 22, 1932, 402-407, 729-734; Berger, Untersuchungen zur Methodik von Bestimmungen der Unterschiedsempfindlichkeit des emmetropen Auges, *Skand. Arch. f. Physiol.*, 71, 1935, 173; F. I. Musylev, Die Abhängigkeit der Sehschärfe von der Entfernung des Objektes, *Acta Ophthal Copenhagen*, 15, 1937, 216.

minimum visual angle is independent of the distance from the eye (see Fig. 3).¹⁵ Consequently, only these conditions can be considered appropriate for measurements of the 'resolving power of the eye.' They should be distinguished carefully from all other methods, especially from measurements of 'visual acuity' with Snellen's charts, hooks, or the broken circle, which are black on a white background and illuminated with reflected light.

(3) *Effect of illumination on luminous points.* Now a further question arises: How does the 'minimum visual angle' in the case of luminous points depend on brightness variations? Or in terms of the definition: How does the resolving power of the eye depend on brightness variations?

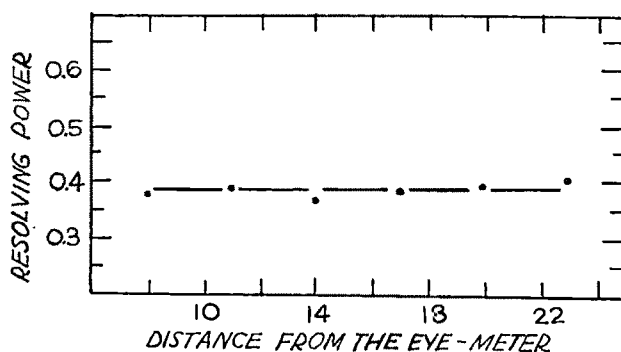


FIG. 3. RELATION BETWEEN RESOLVING POWER OF THE EYE AND DISTANCE: BERGER, 1936

The answer has been obtained from experiments in which luminous squares of different brightness were used. Furthermore, squares of different sizes have been used in different series of experiments. The brightness variations had a range from 2×10^{-2} to 5×10 lux units on the retina.¹⁶ The results are as unequivocal as surprising: The resolving power of the human eye decreases continuously as brightness increases (see Fig. 4). Using small squares, the decrease is about 100%; using large squares, 40%. In both cases it is always continuous. In other words, the relation between the resolving power of the eye and illumination is exactly opposite to the relation between 'visual acuity' and illumination.

How can this result be explained? In the case of luminous squares the original physiological explanation was that the 'minimum visual angle' is determined by

¹⁵ Berger, *op. cit.*, *Skand. Arch. f. Physiol.*, 74, 1936, 27-62; C. Berger and F. Buchthal, Der Einfluss von Beleuchtung und Ausdehnung des gereizten Netzhautareals sowie vom Pupillendurchmesser auf das Auflösungsvermögen des emmetropen Auges, *ibid.*, 78, 1938, 197.

¹⁶ *Idem.*

the existence of an unstimulated retinal unit between two stimulated units. If this were true, we should expect no change in the minimum visual angle with increase of illumination. This probably would be the case if the retinal units had an ideal (*i.e.* an unlimited) brightness-threshold, or if the dioptrical system of the eye would deliver an ideal retinal image, but the image of a luminous spot on the retina is by no means an ideal point or spot of equal brightness. Because of the dispersion of light in the dioptrical part of the eye, the image of a luminous spot has a brightness maximum in the center, while brightness intensity is decreasing on all sides. Thus, with a low brightness degree of the test-object, only the center of the bright spot-image will reach the brightness-threshold of the retinal units and stimulate them, while the surrounding parts cannot bring other retinal units

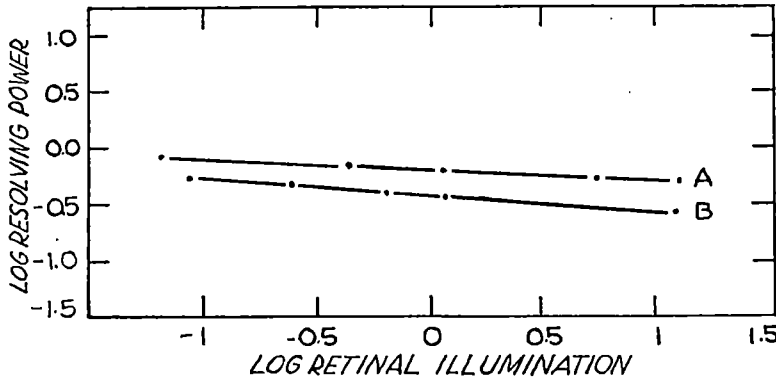


FIG. 4. DEPENDENCY OF THE RESOLVING POWER OF THE EYE ON ILLUMINATION: BERGER AND BUCHTHAL, 1938

(A = retinal image of 1800 sq. μ ; B = retinal image of 18 sq. μ)

into action as their light-intensity is too low. When the brightness of the test-object is increased, not only the center of the image, but more and more of the dispersed light surrounding it will reach brightness threshold and bring other retinal units into action. Consequently, as brightness increases, more and more of the dispersed light will become visible and cover the dark distance between the two point-images. The minimum distance between the points of the test-object must then be increased with increasing illumination so that it can become visible as a dark interval between the spot-images and this increase should be continuous—as it really is. (See Fig. 4.)

If Hecht's supposition were correct (namely, that the cones have different thresholds for brightness), the resolving power of the eye should increase with increasing brightness as well as 'visual acuity.' When the brightness is very low, only a few cones would react and a high number of non-reacting cones would lie between them, so that the minimum visual angle would be relatively large and consequently the resolving power low. With increasing brightness more and more cones would begin to function and fewer and fewer cones would lie between the functioning units, thus increasing the resolving power of the eye.

Especially, if the retinal image of one point is not larger than one functional unit, would such a result be a necessary consequence of Hecht's theory. The experiments show that in this particular case the resolving power of the eye diminishes most rapidly with increasing brightness. The supposition of different units having different brightness-thresholds is very improbable.

Our experiments show, furthermore, that in the case of luminous points on a dark background, the dependence of the minimum visual angle on brightness cannot be a consequence of recognizing brightness differences. The brightness difference between the luminous squares and the space without light between them increases over a very large range, and yet, there is no improvement of the resolving power at all. On the contrary, the greater the brightness-difference between the luminous squares and the background, the larger must be the space between them in order to be recognized. It must, therefore, be concluded that, as the resolving power of the human eye (measured by the minimum visual angle between two luminous squares on a non-illuminated background) diminishes continuously with increase of illumination, it is not probable that the retinal units have different brightness thresholds, or that the recognition of brightness differences can be the general limiting factor for visual acuity or even play a rôle in the dependency of the resolving power of the eye on illumination.¹⁷ The probable consequence of these experiments is that *all functional units of the fovea have the same threshold for brightness and that in the special case of luminous points or small squares the limiting factor for the minimum visual angle is the diameter of the units in the fovea*. The diminution of the resolving power of the eye with increasing illumination can be explained by the diffusion of light in the dioptrical system of the eye. Such diffused light becomes more and more visible as brightness increases, although the retinal area covered by it is, according to the laws of physical optics, independent of brightness.

(4) *Diameter of retinal units.* The conclusions above give a special importance to the absolute value of the 'minimum visual angle' of the light-adapted eye for two luminous points at brightness-threshold. It is then a direct expression of the diameter of the retinal units, which in the center of the retina are commonly supposed to be single cones. The 'minimum visual angle' corresponding to one single cone is about 50'' to 60''. In the case of two small luminous points, however, it was found at brightness-threshold to be 180'' to 200''.¹⁸ According to careful and numerous histological measurements, it is very improbable that the diameter

¹⁷ Hecht and Mintz, *op. cit.*, 593.

¹⁸ Berger, *op. cit.*, 27-62; Berger and Buchthal, *op. cit.*, 197.

of the single cones should really be so large. The fact that the minimum visual angle is so large, leads to the supposition that *the single cones may not be the functional units in the retina*, and that the average functional unit is composed of at least three cones—probably of four or five.

(5) *Size of retinal image.* The functional units in the retina are probably not all alike. In a number of experiments it was found that the minimum visual angle is not independent of the size of the luminous squares.¹⁹ To determine the relation between 'minimum visual angle' and 'size of retinal image,' measurements were carried out with luminous squares of different

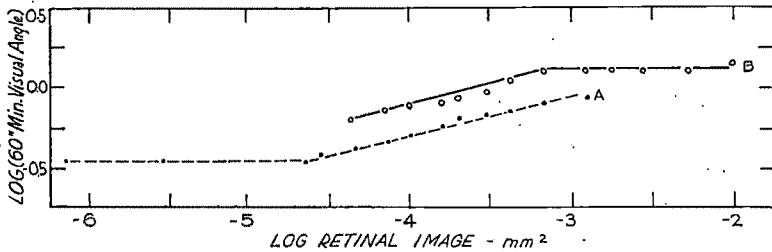


FIG. 5. DEPENDENCY OF THE MINIMUM VISUAL ANGLE ON THE SIZE OF THE RETINAL IMAGE AT BRIGHTNESS-THRESHOLD; BERGER, 1939
(A = luminous squares; B = black squares)

sizes whose retinal images ranged from 0.72 to 1800 sq. μ . Before measuring the minimum visual angle for each size of luminous squares, threshold-brightness was determined in order to avoid as much as possible the visibility of dispersed light and its influence on the measurements. The result was that the minimum visual angle is independent of the size of the retinal image as long as the latter is less than about 18 sq. μ for one square. As soon as the retinal image increases beyond this size, the resolving power of the eye increases continuously until it finally reaches the value of about 50'' to 60'' (see Fig. 5, Curve A). This variation is surprisingly large. Taking the optimum of all values as a basis, the minimum visual angle increases from about 60'' to 180'' with decreasing retinal image, *i.e.*, about 200%.

In order to understand this improvement of the resolving power of the eye, one must first consider that fixation is never absolute because the human eye constantly makes slight movements.²⁰ The measured value of the minimum visual angle is, therefore, an average value for several stimuli on the retina. With this

¹⁹ Berger, *opp. cit.*

²⁰ F. H. Adler and M. Fliegelmann, Influence of fixation on visual acuity, *Arch. Ophthalmol.*, 12, 1934, 475.

consideration in mind, the dependency of the minimum visual angle upon the size of the retinal image can be explained if we suppose that *the fovea is composed of functional units of different sizes*. The larger the retinal images of the two luminous squares, the greater is the number of smaller retinal units which lie between their borders. For this reason the larger squares can be resolved better. The highest value of the minimum visual angle (about 180" to 200") is then an average value of all sizes of the functional retinal units. The lowest one is about 50" to 60" and corresponds to one single cone. In other words, the fovea is probably composed of functional units of different sizes. The lowest one corresponds to one single cone which functions as a separate unit, the largest ones

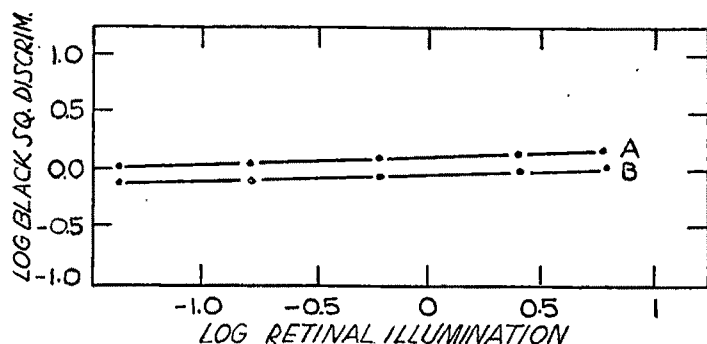


FIG. 6. DEPENDENCY OF THE MINIMUM VISUAL ANGLE ON ILLUMINATION WHEN BLACK SQUARES ARE USED: BERGER, 1939
(A = retinal image of 1800 sq. μ ; B = retinal image of 18 sq. μ)

consisting of 6-8 cones which work together.²¹ The value of 180" to 200" represents an average size of all different units because of the eye-movements.

(6) *Effect of illumination on black squares.* If the minimum visual angle *increases* with increase of retinal brightness when luminous points on a black background are used, and if this dependency is explained by the dispersion of light in the dioptrical media of the eye, then the following question arises: How does the minimum visible distance depend on brightness when it is measured with two black squares on a luminous background?²² Experiments conducted to answer this question show that the

²¹R. Granit, *Die Elektrophysiologie der Netzhaut und des Sehnerven*, Copenhagen, 1936.

²² Although all experiments described here are represented in curves, showing values of 'minimum visual angle,' this term is correct only for small luminous squares. The 'minimum visual angle' is used here to enable the reader to compare all experiments directly. For that reason, the term 'minimum visual angle' is also used in connection with measurements of the minimum visible distance between black squares or luminous squares, exceeding the size of a single cone.

minimum visual angle *decreases with increase of retinal brightness*.²³ The increase is continuous without any sign of two different parts in the curve as Hecht describes it in the case of thin wires (see Fig. 6). This result further supports the theory that the dispersed light in the dioptrical media, and not retinal or central processes, is the cause for the changes of the minimum visual angle with variations of the illumination. In the case of the black squares, the light disperses on the inner borders of the squares. The squares can, therefore, be better resolved when light-intensity increases because a less and less luminous space between them will be a sufficient stimulus for their resolution.²⁴

Although the experiments described above (luminous squares on a black background, and black squares on a luminous background) consistently yield opposite results regarding the dependency of the minimum visual angle on brightness, the curves showing the relationship are not simply reversed. The increase of the minimum visual angle in the case of luminous squares is greater than the decrease with black squares. The decrease in the latter case is, moreover, always the same, regardless of the size of squares used. The curves for squares of all sizes are parallel. In the case of luminous squares, measurements with the smallest retinal images show the greatest increase of the minimum visual angle with brightness-increase; but when larger squares are used the increase is much less (see Figs. 4 and 6). The curves are not parallel.

The differences in the slope of the curves may be a consequence of the total area of illuminated retinal image. The larger the total illuminated area on the retina, the more dispersed light will fall upon the whole retina, and the less will be the stimulus-effect of that kind of dispersed light which is a consequence of the defects of the optical media of the eye. In the case of luminous squares, the increase of retinal image corresponds to an increase in the total amount of light falling into the eyes. The black squares were used on a luminous background of about 17.5 x 2.5 cm. The variation in size of the black squares (4 x 4 mm. to 20 x 20 mm.) had such little influence on the total amount of light falling into the eyes that it can be considered practically constant for different sizes of squares. This would explain the differences in the slope of all the curves where brightness increases. The smallest luminous squares must show the highest slope, because the total amount of lighted area was very small. They must differ from the greater luminous squares since the area of the latter had increased considerably. The black squares must show a slope similar to that of the large luminous squares

²³ Berger, Der kleinste Sehwinkel bei Verwendung lichtloser Quadrate auf selbstleuchtendem Grund und seine Beziehung zur Grösse und Funktion der retinalen Einheiten, bzw. dem "Auflösungsvermögen" des emmetropen Auges, *Skand. Arch. f. Physiol.*, 83, 1939, 39-57.

²⁴ W. W. Wilcox and L. Purdy, Visual acuity and its physiological basis, *Brit. J. Psychol.*, 23, 1933, 233-261.

(in the opposite direction), since the area of the luminous background was very large. There cannot be a difference in the steepness of these curves, when other sizes are used, because the large luminous background remained practically constant.

(7) *Black squares of different sizes.* When the size of the retinal image is increased, the minimum visual angle decreases in the case of black squares as well as with luminous squares. The curves are very similar in shape (see Fig. 5B). The first part of the curve with black squares is absent, because black squares are invisible as long as their retinal size is less than about 72 sq. μ under conditions mentioned above.²⁵

Both facts support the conclusions drawn in the first part of the experiments described. If the dependence of the minimum visual angle on the sizes of retinal image (always using threshold brightness) is a consequence of the functional structure of the retina, there should be no difference in the shape of the curves whether black or luminous squares are used. The experiments show that this is the case. On the other hand, if the size of the average retinal unit is also the basis for the 'minimum visible,' in the case of black squares, then one black square cannot become visible until its retinal image covers just a little more than an average size of the functional unit which is about 3-5 cones. The retinal image corresponding to one single cone is about 18 sq. μ . The size of the retinal image, when one black square just becomes visible, is about 72 sq. μ . That result proves again that not the single cone but an average unit of 3-5 cones is the functional unit in the fovea. The explanation must be, that if the retinal image of a black square is less than such an average functional unit, the whole unit still will be in function, because it is still stimulated by light falling on the border and, therefore, the recognition of one black square will be impossible. In the case of luminous points, any size of retinal image can become visible if its brightness is strong enough to stimulate retinal units. If one part of a retinal unit is stimulated strongly enough the whole unit will go into function and the point will become visible.

(8) *Size of functional units vs. perceived size.* The facts mentioned above are interesting in another respect, in the matter of perceived size. As long as the image of one luminous point does not exceed one functional unit on the retina, it will not be possible to recognize any difference of size until a new unit comes into function. When this occurs an entire second new unit will be joined to the first one and the perceived size will be augmented considerably. In the case of black squares or points, it will be impossible to recognize anything at all until the retinal image reaches the

²⁵ Berger, *op. cit.*, 39-57.

value of about one average retinal unit (72 sq. μ). The black square appears to be astonishingly small, even if it is made larger and larger. It will still be perceived as the same size until a new functional unit is completely free from light. That may be one reason why a luminous point or square always appears larger than a black one of the same size. As long as the retinal image covers less than an average functional unit, the luminous point will appear as large as the functional unit, while the black one will be invisible. If the retinal image is larger than the average retinal unit, the luminous point appears as large as two retinal units, the black one only as large as one functional unit.

(9) *Size of functional units vs. perceived form.* The size of the functional retinal unit is also important moreover in regard to the visibility of forms. Psychologists have pointed out²⁶ that the visibility of forms is a typical example of those processes which cannot be explained by retinal histological facts, but which must be understood by means of processes in the center of the nervous system which do not have a point-to-point relation with the foveal structure. Yet it is easy to understand that no distinction of form can be possible as long as the retinal image is less than one retinal unit. It is a well known fact that the form of very distant objects cannot be distinguished. If the retinal structure is the basis for the visibility of forms, then it will be necessary that more units function at one time in order to recognize more complicated forms. This being so, a line will only be seen as a line, when the retinal image touches at least two different units; a triangle should touch at least three units and a square four units in order that their forms be recognized. This supposition has been the basis for some experiments on the visibility of forms, which, in connection with the above calculated size of the average retinal unit, lead to some new conclusions about the retinal function.

The visibility of a line (a rectangular figure of narrow width), a triangle, a square, a pentagon, and a hexagon have been measured by determining minimal and maximal distances from the eye at which the forms became visible or disappeared again.²⁷ All these geometrical figures had the same area (100 sq. mm. in some experiments and 150 sq. mm. in others). With these measurements, black forms on a luminous background as well as luminous forms on a black background were used. The results reveal: (1) that a relationship exists between retinal structure and the visibility of forms—the more complicated a form the more retinal units must be

²⁶ Cf. K. Koffka, *Psychologie der optischen Wahrnehmung*, in Bethe's *Handb. f. norm. und patbol. Physiol.*, 12, 1931, 1215-1271.

²⁷ Berger and Buchthal, *Formwahrnehmung und Funktion der Fovea*, *Skand. Arch. f. Physiol.*, 79, 1938, 15.

in function at the same time in order to recognize the particular form; (2) that the area on the retina covered by the retinal image of those figures had to be much larger than was supposed—even if the average retinal unit was considered to consist of three cones, from four to six times as many functional units of such size had to be covered by the retinal image for the different forms to be clearly recognized; and (3) that the visibility of forms improves when brightness increases. In the case of luminous geometrical figures, the latter phenomenon could have been a consequence of the increased visibility of dispersed light which was stimulating the immediate surroundings of the luminous figures. It was, therefore, especially interesting to find that the visibility of forms also improves considerably with brightness increase when black figures on a luminous background were investigated. If, in the case of luminous figures the increased visibility of the dispersed light was the reason for the better visibility of the forms, then the opposite result should be expected in the case of black figures. Nevertheless, in using black figures, all results closely resembled those found with luminous figures. The more complicated a figure is, the larger must be the retinal image in order to be recognized as a special form. Also with black figures the retinal image must be considerably larger than supposed and their visibility is better, when brightness is higher. Even with very high brightness, it still was necessary that from two to three times as many retinal units of average size be covered by the image in order to recognize the different forms.

These facts suggest first that the functional retinal unit is the basis for the visibility of forms, and secondly that not all functional units are operative at the same time. Only one third to one half of all functional units may generally be ready to function, the others may be in a recovering state of non-function. This would be in agreement with other experiences in biology. The improvement of the visibility of forms with increase in brightness can then be explained by an abbreviation of the relative refractory-phase of the units so that more units come into action at the same time, when the strength of the stimulus increases. The conclusion that more units come into action at the same time with higher illumination is in agreement with Hecht's supposition; but its cause is probably not a difference in the brightness-threshold of different units, as Hecht states, but rather the abbreviation of the relative refractory-phase of the single units as the light-stimulus increases.

SUMMARY AND CONCLUSIONS

(1) The original idea of 'minimum visual angle,' which is supposed to be the basis for visual-acuity tests, has been examined. Contrary to basic concepts, it has been found, that the minimum visual distance between two points as test-objects is independent of the distance between the eye and

the test-object only if small luminous points or squares at brightness-threshold on a black background are used. Only measurements under those conditions can, therefore, be considered as tests of the 'resolving power of the human eye' and must be distinguished sharply from visual acuity tests with Snellen's or Landolt's test-objects.

(2) In contrast to the increase of 'visual acuity' with illumination, the 'resolving power' of the human eye decreases continuously with increase of illumination. This is explained by the increasing visibility of the light-spots, which are formed on the retina around the luminous point-images as a consequence of light-dispersion in the dioptrical media of the eye and is contrary to Hecht's theory of differences in brightness-threshold of different cones.

(3) The absolute value of 'minimum visual angle' with luminous points at brightness-threshold is not 60", but averages between 180" and 200". This is contrary to Hook's famous observation on stars and also contrary to the physiological concept, that two points can be 'resolved,' if one non-stimulated cone is lying between two stimulated cones. The latter average corresponds to at least three non-stimulated cones.

(4) Visual acuity, as measured by Landolt's broken circle or Snellen's letters and reflected light, depends on increase of illumination for the following reasons:

(a) The light on the retina which falls in the white space between the arms of the circle or the black details of the letter-images is dispersed, covers the black borders, and becomes more and more visible as brightness increases. It will then be possible to recognize a smaller white space or opening on a broken circle or between the black details of a letter, when higher intensities are used.

(b) In consequence of the results of the experiments in which different geometrical figures were used, we must conclude that the visibility of the form of the broken circle or the letters, which improves with increase of brightness, is involved in measurements of visual acuity. The improvement of 'visual acuity' which occurs with increasing illumination, is interfered with by (c).

(c) When a greater brightness is used, generally a smaller test-object is employed. This change in size decreases the visibility of the white space between the arms of the broken circle or between the details of Snellen's letters, because their resolution depends upon the length of the borders, between which the white space is lying. This narrowing of the borders surrounding the white space between the arms of the broken circle or the details of the letters interferes with the improvement of visual acuity, when higher illumination and consequently smaller sizes are used. The improvement would be greater, if the size of the test-object were not changed.

(d) Generally, the broken circle or letters are illuminated from the side of the observer and consequently the dark areas reflect light also. Under these conditions brightness-discrimination may be involved.

(e) In most experiments, brightness-adaptation is not kept constant. Often the eye, before measurements are made, is dark-adapted. Since light-adaptation takes place very rapidly, the adaptation process interferes with and may influence the values finally obtained.

The general improvement of visual acuity with increase of brightness (as measured by Snellen's letters or Landolt's broken circle, black on white background with reflected light) is the result of two factors: namely, the visibility of the dispersed light, and the visibility of form, both of which increase with increase of brightness. The special shape of the curves, showing this improvement, may be determined by the interfering effects of the change in the size of the test-object, of the light reflected from the dark areas, and of an incomplete light-adaptation.

(5) The absolute value of tests with Snellen's or Landolt's objects is greatly influenced by the proportion 1:5, which is a mere consequence of designing capital letters.

(6) It is probable, that the general retinal unit, as far as function is concerned, is not the single cone, but that from two to eight cones work together. Only a few single cones work separately. These retinal units of different sizes have probably all the same threshold for brightness, but probably not all of them are working at the same time. Only one third to one half of all units are constantly ready to function. The rest are in a state of recovery. When brightness increases, more functional units come into action at the same time, because their relative refractory phase is shortened.

CONDITIONING AND RETENTION UNDER HYPNOTIC DOSES OF NEMBUTAL

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The use of depressing drugs in the study of the processes of conditioning (or learning) and retention has potentialities similar to those afforded by the method of extirpation. It is possible with drugs to eliminate certain kinds of behavior from the repertoire of the subject, and in some cases to defunctionalize specific portions of the central nervous system. Perhaps the greatest advantage of the method is that the effects produced are temporary, rather than permanent and irreversible.

One of the possibilities permitted by the use of drugs is the direct application of local depressing agents to the surface of the cerebral cortex.¹ The conditioned behavior of monkeys has been examined under such circumstances by Harlow and Settlage.² These experimenters used both Nupercaine and Novocaine in addition to freezing a portion of the cortex with ethyl chloride. The first two substances failed to alter the previously learned reactions of the Ss but ethyl chloride temporarily destroyed a visual discrimination habit. Hence data on the 'locus' of the habit within the reacting organism were brought to light.

Subcutaneous or intravenous injection of the drug curare as a means of eliminating certain kinds of activity during conditioning has also led to promising results. Curare has been employed as a depressing agent by Harlow and Stagner who used both cats and dogs as Ss,³ and by Girden and Culler with dogs.⁴ Its special effects upon the higher and lower centers of the central nervous system have been observed by Culler and his associates.⁵ This drug has the property of eliminating conditioned responses which are built up in the normal or undrugged state. New conditioned responses (CRs) can nevertheless be formed under curare. They disappear when the effects of the drug have worn off but can be called out again upon a second application of the drug. Girden and Culler interpret these findings

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¹ Cf. J. G. Dusser de Barenne, Some aspects of the problem of "corticalization" of function and of functional localization in the cerebral cortex, *Ann. Res. Nerv. Ment. Dis.*, 13, 1934, 85-106.

² H. F. Harlow and P. Settlage, The effect of the application of anesthetic agents on circumscribed motor and sensory areas of the cortex, *J. Psychol.*, 2, 1936, 193-200.

³ H. F. Harlow and F. Stagner, Effects of complete striate muscle paralysis upon the learning process, *J. Exper. Psychol.*, 16, 1933, 283-294.

⁴ E. Girden and E. Culler, Conditioned responses in curarized striate muscles in dogs, *J. Comp. Psychol.*, 23, 1937, 261-274.

⁵ E. Culler, J. D. Coakley, P. S. Shurrager, and H. W. Ades, Differential effects of curare upon the higher and lower centers of the central nervous system, this JOURNAL, 52, 1939, 266-273.

to mean that conditioning without curare involves the cortical centers, while conditioning under the drug is sub-cortical in nature.⁶

The Russian investigator Petrova has recently made⁷ extensive studies of the bromides as they affect the conditioning and extinction processes. In general it has been found that the bromides retard the rate of conditioning and accelerate the rate of extinction.⁷ Experimental neurosis can sometimes be 'cured' by the administration of bromides alone.⁸ In other cases combined doses of bromides and caffeine are necessary.⁹ These results are especially significant in the light of the now common psychiatric practice of using depressing drugs in the treatment of epilepsy, schizophrenia, and other psychopathological states.

Further probing into the basic nature of conditioning and retention is made possible by the use of the barbiturates. With doses of the barbiturates sufficiently large to produce complete surgical anesthesia in animals, the excitability of the cerebral cortex to electrical stimulation is much less than under ether anesthesia.¹⁰ The spinal reflexes are not so strongly affected. It seems likely, therefore, that the focus of the depressing effect upon the central nervous system is in the cortical and sub-cortical ganglia.¹¹

Wolff and Gantt have studied the effects of sodium isoamylethyl barbiturate (Sodium Amytal), together with the effects of three other drugs, upon the conditioned salivary response in dogs.¹² Their analysis of the changes produced by large doses of Sodium Amytal upon the conditioning process is divided into four steps: (1) they first noted a brief facilitating stage before the drug had begun to exert its full effect; (2) this was followed by the long phase of depression or narcosis; (3) next was the recovery period; and finally (4) a postnarcotic phase in which the CR was in some cases found to be greater than average.

⁶ Girden and Culler, *op. cit.*, *J. Comp. Psychol.*, 23, 1937, 261-274.

⁷ Cf. e.g. M. K. Petrova, [The combined action of bromides and caffeine in a case of a chronic ultraparadoxical phase and a general explosiveness of the excitatory process in a castrated dog of a "medium" type of nervous system—2 cases of abortive catatonias] *Trud. fiziol. Lab. Pavlov.*, 7, 1937, 591-647 (*Psychol. Abstr.*, 12, 1938, No. 3571); also E. R. Hilgard and D. G. Marquis, *Conditioning and Learning*, 1940, 119.

⁸ Petrova, [Experimental neurosis cured by bromide] *Arkh. biol. Nauk.*, 34, 1934, 15-39; [Bromides and their effect upon castrated dogs—further materials on the study of the mechanism of the action of bromides] *Trud. fiziol. Lab. Pavlov.*, 7, 1937, 5-105 (*Psychol. Abstr.*, 12, 1938, No. 3567); [The formation of conditioned neural connections in a castrated puppy] *Trud. fiziol. Lab. Pavlov.*, 7, 1937, 231-257 (*Psychol. Abstr.*, 12, 1938, No. 3431).

⁹ Petrova, [Curing a castrated dog of the strong well-equilibrated type of a prolonged (18 months) neurosis by means of caffeine and bromide] *Trud. fiziol. Lab. Pavlov.*, 7, 1937, 105-131 (*Psychol. Abstr.*, 12, 1938, No. 3568); [The combined action of bromides and caffeine in curing pathological states of weak inhibitable dogs, which were produced by attempts to transform negative conditioned stimuli into positive and positive into negative] *Trud. fiziol. Lab. Pavlov.*, 7, 1937, 649-728 (*Psychol. Abstr.*, 12, 1938, No. 3572).

¹⁰ J. F. Fulton and A. D. Keller, *The Sign of Babinski: A Study of the Evolution of Cortical Dominance in Primates*, 1932, 143-144.

¹¹ T. Sollman, *A Manual of Pharmacology and Its Applications to Therapeutics and Toxicology*, 5th ed., 1940, 735.

¹² H. G. Wolff and W. H. Gantt, Caffeine sodiobenzoate, sodium iso-amylethyl barbiturate, sodium bromide and chloral hydrate: Effect on the highest integrative functions, *Arch. Neurol. Psychiat.*, 33, 1935, 1030-1057.

Barbiturates have also been used by Settlage in the attempt to answer the question whether conditioning in cats is 'intra-neural' or demands overt movement of the effectors.¹³ He gave his Ss Sodium Amytal, alcohol, and Nembutal in quantities just sufficient to prevent observable conditioned responses, but not strong enough to inhibit the unconditioned reflex to the original stimulus (faradization). After the effects of the different drugs had worn off, his animals showed that they had been conditioned even though no CRs had been seen to occur during the training. "Thus the effect of the drug," he writes, "was to inhibit the processes underlying the elicitation of the conditioned response without preventing the formation of new stimulus-response connections."¹⁴

PROBLEM

The present experiment was designed to study further the processes of conditioning and retention under the influence of a strong depressing drug. The barbiturate Nembutal (Pentobarbital Sodium, Abbott) was selected as the hypnotic agent and dogs were used as Ss. With a refined application of the flexion conditioning technique, the investigators hoped to obtain a more exact analysis of the effects of such a depressing drug than has heretofore been available. Graphic recording of the responses of all four feet together with the respiration should permit a detailed study of the behavior in terms of the latency, duration, and amplitude of the individual movements made. In this way, the precise nature of the effects of the drug upon the building up of the CR and on other relevant behavior, should be capable of definite quantification. The data should also have some bearing on the problem of the neural correlates of learning and retention.

PROCEDURE

The experimental situation. The buzz-shock conditioning method was used throughout the experiment and six unselected mongrel dogs—one female and five males—served as the Ss. They were young but mature animals weighing between 30 and 40 lb. The buzz stimulus was a pure tone of 1000 ~ delivered through a loud speaker with an intensity of 50-55 db. above the human threshold at the S's ears. Its duration was 2 sec. The unconditioned stimulus was a make-break D.C. shock, 0.2 sec. in length, which overlapped the buzz for the last 0.2 sec. of its occurrence. The shock was received by the S through stainless steel electrodes securely taped to the anterior and posterior surfaces of the right hind foot. Of particular importance for the results of this experiment is the fact that *the intensity of the unconditioned stimulus was adjusted whenever necessary, so as to maintain an unconditioned flexion response of just 4 in. in extent.* If, on any buzz-shock trial, S lifted his foot to a height of about 2.5 in., the shock was automatically cut out by a mercury switch in the circuit.

¹³ P. Settlage, The effect of Sodium Amytal on the formation and elicitation of conditioned reflexes, *J. Comp. Psychol.*, 22, 1936, 339-343.

¹⁴ *Ibid.*, 341.

During their entire training the animals were isolated in a sound-proofed chamber where they were observed through one-way-vision windows. Graphic records of the movements of all four feet, the respiration, the buzz, shock, and time were obtained in an apparatus room outside the soundproofed room. A more complete description of the technique and training procedure will be found elsewhere.¹⁹

In order to assist the animals in maintaining a standing posture during the drugged state a special supporting harness was employed. It consisted of a canvas hammock which was passed under the chest and abdomen, together with four rubber slings which held each one of the legs at its point of union with the trunk. The neck was supported by a loose but rigid wooden collar. As a means of equalizing the conditions during both the drug and non-drug trials, this harness was used with all Ss during all of their conditioning and retention tests.

Training sequence. After an initial habituating period of 4 days,²⁰ the Ss were given massed training in blocks or groups of 100 trials. Successive trials in

BLOCKS		1	2	3	4
TRIAL NOS.		1-100	101-200	201-300	301-400
GROUP A	F27	CONTROL		CONTROL	
	M40	DRUG		DRUG	
GROUP B	M41	CONTROL		CONTROL	
	M42	DRUG		DRUG	
GROUP B	M43	CONTROL		CONTROL	
	M44	DRUG		DRUG	

FIG. 1. SHOWING PLAN OF ROTATION USED IN THE PRESENT EXPERIMENT

By cross-combining the data as indicated by the diagonal arrows, a series of 200 trials in which the drug was given was obtained which was directly comparable to a second series of 200 trials in which no drug was given. The same Ss furnished the data for both series of trials, and all stages of the learning or training process were represented in both the drug and control results.

each block were spaced from 15 to 120 sec. apart, except that a 5-min. rest period was given after every group of 20 trials. One trial in every 5 was a "buzz-alone" trial in which the unconditioned stimulus was omitted by E. Four different blocks of 100 trials each, or 400 trials per dog, were administered in this manner. Each 100-trial block or unit was separated from the others by an interval of one week. In this way any after-effects of the drug which persisted following a period of training would be completely dissipated before the next training session began. A single laboratory period in which 100 trials were given consumed approximately 2 hr.

¹⁹ W. N. Kellogg, R. C. Davis, and V. B. Scott, Refinements in technique for the conditioning of motor reflexes in dogs, *J. Exper. Psychol.*, 24, 1939, 318-331.

²⁰ *Ibid.*, 328-329.

Since an intensive conditioning experiment is necessarily limited to a small number of Ss, it was impossible in the present instance to divide the animals into two equivalent groups of experimental (drugged) and control (non-drugged) Ss. But by a scheme of rotation illustrated in Fig. 1, each S was made to serve as its own control. Dogs numbered F 27, M 40, and M 41 were drugged during the second and fourth blocks of 100 trials. Dogs numbered M 42, M 43, and M 44 were drugged during the first and third blocks of 100 trials. By cross-combining the data as indicated by the diagonal arrows in Fig. 1, a series of 200 trials in which the drug was given was obtained which was directly comparable to a second series of 200 trials in which no drug was given. The same Ss furnished the data for both groups of trials, and all stages of the learning or training process were represented in both the drug and control results. From an experimental point of view, therefore, the data compared are truly from equivalent groups, even though small numbers of individuals were used.

The dosage of Nembutal which was administered was slightly less than one-half that usually given for complete surgical anesthesia. This quantity, which amounted to 0.08 grain per lb., was considered to be a good 'hypnotic' dose. It was injected intraperitoneally 20 min. before the training of the S was begun.

RESULTS

Qualitative observations. A few excerpts from the laboratory protocols will show the general nature of the behavior of the animals while under the influence of the drug.

F 27: "Soon after the injection, the S became very unsteady in walking—almost incapable of standing up. Showed marked lack of muscular control, especially as it affected balance. Other behavior, such as responses to other dogs and to the experimenters, seemed fairly normal. When taken from the stock after training, dog could stand up to eat but staggered when she walked and fell from her shelf even though she lay prone upon it."

M 40: "Given 2.9 cc. Nembutal solution and placed in stock 20 min. later. Had a spasmodic bark, which lasted for the first 20 trials, after which it disappeared. Dog seemed to go to sleep about 35 min. after the injection of the drug, but woke up shortly afterward and apparently remained in a stuporous condition throughout the rest of the trials. Unable to stand when fed at the end of the experiment. Much lack of coordination and balance. Went to sleep immediately after feeding."

M 44: "Was out nearly 'cold' 22 min. after receiving Nembutal. Snored continuously during first part of training period, but unconditioned reflex was readily elicited. Soon developed a spasmodic jerk, independent of the unconditioned response or the CR in the right-rear (shocked) foot. Later showed some bodily tremor and generalized activity. Mild struggle behavior was manifest toward the last."

Frequency and amplitude of the CR. Conditioned flexion responses were tabulated for each of the four feet separately. A flexion CR upon the graphic records consisted of any measurable deflection in an upward direction from the horizontal *during the 1.8 sec. of the buzz which preceded the shock or unconditioned stimulus.* The relative frequency of CRs was then computed for each foot separately, by taking the percentage of lifts

occurring within each group of 20 trials. Doubtful or uncertain responses were regarded as "not CRs."

Composite frequency graphs of all the *Ss*, for the right-rear (shocked) foot, are given in Fig. 2. These graphs were obtained by cross-combining the data according to the method previously described, and illustrated in Fig. 1. The dotted line shows the learning performance of the six animals under the influence of Nembutal, and the solid line is the corresponding control record of the same *Ss*. The break in the curves between

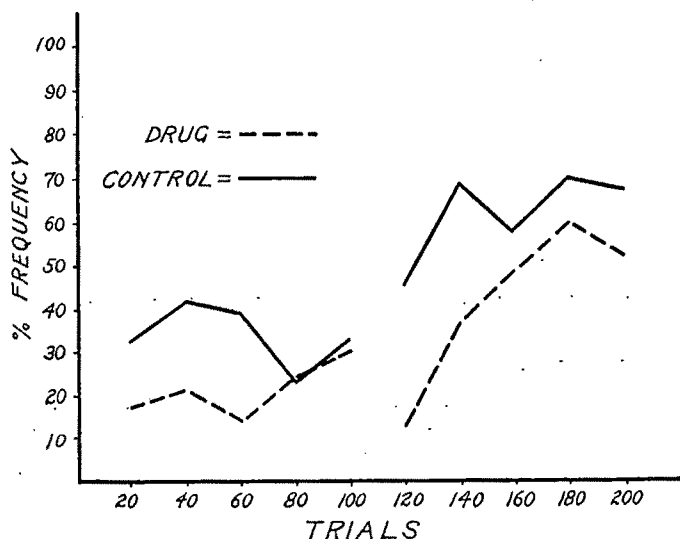


FIG. 2. COMPOSITE FREQUENCY GRAPHS FOR THE RIGHT-REAR (SHOCKED) FOOT ONLY FOR ALL THE *Ss*

These graphs were obtained by cross-combining the data according to the method illustrated in Fig. 1. The dotted line shows the learning performance of the six animals under the influence of Nembutal, and the solid line is the corresponding control record of the same *Ss*. The break in the curves between points 100 and 120 indicates the passage of an interval of 2 wk., together with the interpolation of 100 trials of training which are irrelevant to the curves as they are plotted. In the case of the control curve, the interpolated trials were given under Nembutal.

For the drug curve the interpolated trials were control or non-drug trials.

trials 100 and 120 indicates the passage of an interval of 2 wk. together with the interpolation of 100 trials of training which are irrelevant to the curves as they are plotted. In the case of the control curve, the interpolated trials were given under Nembutal. For the drug curve the interpolated trials were control or non-drug trials.¹⁷

¹⁷ Cf. Fig. 1.

The frequency curves for the three non-shocked feet, which are not shown here, were almost horizontal for the drug sessions with a relative frequency never exceeding 7%. In the control trials, the non-shocked feet started with a relative frequency of slightly more than 20% and gradually decreased in responsiveness almost to zero at the two-hundredth trial.

Both the drug and control graphs of Fig. 2 are typical of the acquisition of the conditioned response. Each has a clearly sigmoid trend despite its irregularities. The generally lower level of the graph for the training under Nembutal is of course due to the depressing effect of the drug upon the learning process.

There is, however, an important difference between the two graphs which should be pointed out, *viz.* the control graph rises between trials 100 and 120, while the graph for the drugged state shows a definite drop. These opposing trends we would explain as follows. (a) The rise in the control curve may be due to the positive or facilitating effect of the interpolated learning under the drug which took place between trials 100 and 120. (b) An alternate possibility is that the rise in the control graph is simply the difference between two adjacent points on the non-drug learning curve and is not necessarily influenced by the interpolated training under Nembutal. Since the subsequent rise in the control graph between the points plotted at trials 120 and 140 is as great or greater than the rise occurring in the interpolated interval, one may suppose that the whole graph represents a continuous process which was not seriously interrupted or disturbed either by the interpolated time interval or by the interpolated training. There is certainly no evidence that the training in the drugged condition had an inhibiting effect on the regular or control training.

The drop during the interpolated interval of the training under Nembutal we would ascribe to the differential effect of the drug at different periods after its administration. The effects of single injections of Nembutal, as of single injections of other depressing drugs, do not remain constant with the passage of time. Complete recovery in dogs takes place with moderate doses in from 6 to 18 hr. Since the data plotted at trial 100 were obtained from 120 to 140 min. after the injection of the drug and the data plotted at trial 120 were obtained from 20 to 40 min. after injection, the effects of the drug must have been different during these two periods. The curve of learning under Nembutal which is shown in Fig. 2 is hence complicated by the gradual wearing off of the drug during each block of 100 trials. Its general contour is actually influenced by two factors: (a) the increase in efficiency of the learning; and (b) the decrease in the

depressing effect of the drug. The drop which occurs between the end of one training session and the beginning of the next we therefore take to be an indirect measure of the change in the depth of the drugged condition.

In the present experiment, as in previously reported studies, the amplitude graphs were similar to those obtained for frequency. The distance which the separate feet were lifted in response to the buzz decreased gradually for the shocked member as the training progressed, in a manner which is roughly indicated by the frequency curves of Fig. 2. Similarly the three non-shocked feet were found to decrease in the amplitude of their condi-

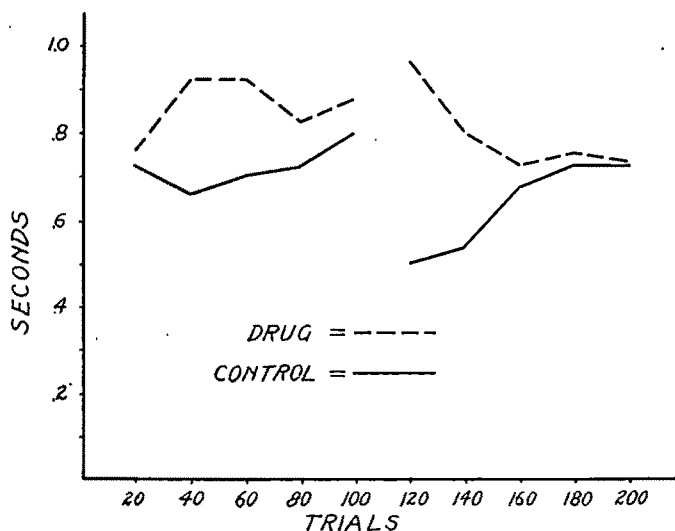


FIG. 3. THE LATENCY OF THE CR UNDER NEMBUTAL AND WITHOUT IT (Composite graphs for 6 Ss.)

tioned movements without the drug, and to remain at so low a level under Nembutal that any appreciable decrease was impossible. Because of the close similarity between the amplitude and frequency findings, we present here no quantitative results of the amplitude measurements.

Latency of the CR. In view of the fact that Nembutal is a motor depressant, it seemed reasonable to suppose that the latency of the CRs occurring under the influence of the drug would be greater than the latency of the CRs made by the same Ss in the non-drugged state. That this is, in fact, the case will be seen by examining the latency graphs in Fig. 3. These composite curves which include the data for the right-rear foot for all animals, were made up in the same manner as the graphs in Fig. 2. The

latency itself was measured in units of 0.1 sec. from the onset of the buzz to the beginning of the CR.

The lack of a definite upward or downward trend throughout the curves as a whole agrees substantially with earlier findings reported from this laboratory to the effect that the latency of the CR is the least predictable of the available measures of the conditioned movement.¹⁸ The rise in both sections of the control or normal graph (an increase in latency) may indicate the gradual onset of fatigue during the 2-hr. laboratory session.

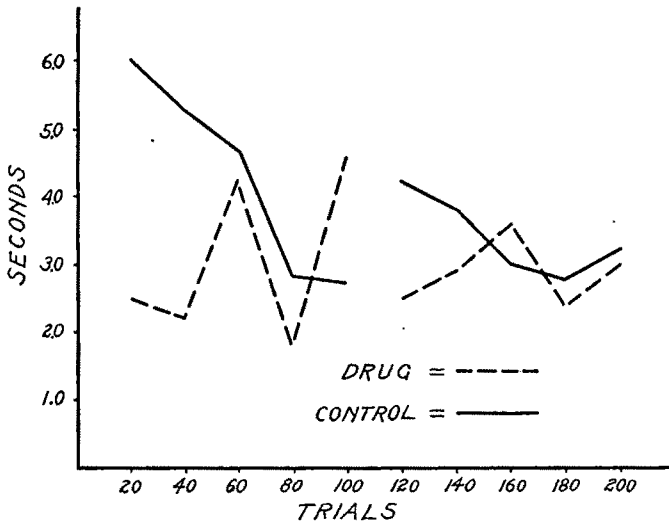


FIG. 4. THE DURATION OF THE CR UNDER NEMBUTAL AND WITHOUT IT
The length of the response tends to shorten with normal practice, because of the fact that *S* is relatively slow at first in returning his foot to the floor. Under Nembutal no such trend takes place. *S* is less alert and apparently less able to maintain a long response during his early training.

Similarly, the drop in the second half of the curve of latencies under Nembutal may be taken as a measure of the wearing off of the drug. Yet the trend of the first half of the drug curve does not support such an interpretation. In view of the generally constant level maintained by the latency of the CR over long periods of 'normal' training,¹⁹ we are inclined to be hesitant about making definite commitments concerning the possibilities suggested by the present graphs.

¹⁸ I. S. Wolf and W. N. Kellogg, Changes in general behavior during flexion conditioning and their importance for the learning process, this JOURNAL, 53, 1940, 384-396.

¹⁹ *Ibid.*, 394.

Duration of the CR. The length of the conditioned response was measured to the nearest 0.1 sec. for each of the buzz-alone trials in which a clear-cut CR occurred. Composite graphs for the right-rear foot, covering all of the training sessions, made up in the same manner as the graphs in Figs. 2 and 3, are given in Fig. 4. It will be noted that there is a distinct difference between the nature of the behavior under the drug and that which took place in the normal state. Without Nembutal the animals' early CRs were as long as 6 sec. in duration. As training progressed, however, the Ss tended to replace the lifted foot much sooner to the floor, so that the duration of the CR was reduced to 3 sec. or less. Under the influence of the drug the conditioned response was shorter throughout and showed no such consistent trend or modification as is demonstrated by the non-drug graph.

Retention of CR. To test the ability to retain previously learned responses under the normal and drugged conditions, 3 Ss—M 42, M 43, and M 44—

TABLE I
RETENTION-DATA FOR THREE DOGS

		Condition during retention-tests		
No. training trials before retention-tests	Control trials	normal	drugged	normal
	Drug trials	0	300	300
	Total	300	300	600
		300	600	900
No. buzz-alone stimuli given during retention-tests		60	60	60
CRs elicited	Total no.	2	0	20
	% Frequency	3.3	0.0	33.3
	Total amplitude (in.)	1.0	0.0	22.8

were given 20-trial retention-tests after each of the first three blocks of 100 training trials which they received. These Ss, it will be remembered were drugged during the first and third groups of 100 trials and were normally conditioned during the second and fourth groups of 100 trials. The tests were conducted in the following manner. Before the beginning of trials 101-200, the Ss were presented with 20 'buzz-alone stimuli.' The CRs that they made to these stimuli served as a measure of the retention of the training in trials 1-100 which they had received one week previously. Each of the three animals were similarly tested with 20 retention (buzz-alone) stimuli before trials 201-300 of his regular training and again before trials 301-400. The results are summarized in Table I.

It will be seen from this table that the Ss responded with a total of 2 CRs

after having received 300 conditioning trials under Nembutal (100 trials per dog). They were in an undrugged state at the time of the test. Here then is additional though meager proof that bona fide conditioning did take place under hypnotic doses of the drug. None of the 3 Ss had at this time received any previous training except that given with Nembutal (*cf.* column 1, Table I).

When the same Ss, however, were given 'buzz-alone stimuli' while in the drugged state (column 2) they responded not at all, even though they had previously received a total of 600 training trials (200 per dog) half of which had been given in the non-drugged state. It is to be remarked, however, that the effect of the dose was especially potent at the time of the tests, since the injection had been made only 20-40 min. previously. The failure to obtain any CRs in this case must be interpreted to mean that buzz-stimuli without reinforcement are not sufficient to elicit previously well-established CRs when the organism is in a heavily drugged condition.

The third test (column 3) given after 300 trials of conditioning per dog, shows a strong retention effect. The difference between the score of 20 CRs obtained on this test and the score of 2 CRs obtained on the first test, is a measure of the effectiveness of the intervening 200 trials of training per dog. Just how much of the increase is due to the 100 trials of training under Nembutal and how much is due to the 100 trials in the normal condition, cannot of course be determined from these figures.

NEUROPHYSIOLOGICAL IMPLICATIONS

From the behavioral data which have been presented, it is possible to draw certain inferences regarding the probable physiological and neurological causes of the lowered level of learning and retaining ability produced by the drug. These inferences are, of course, speculative; yet they should be worth examining as an explanation of the experimental findings.

Effectors. Consider behavior for a moment in terms of the S—O—R formula; or consider a single act or movement on the part of the organism as a receptor-transmission-responding sequence. One may then ask, whether the lowered efficiency in the learning and retaining ability of the Ss under Nembutal is due primarily to a change in the effector system, a change in the receptors, or to changes in the transmitting or nervous system. With reference first to the effectors, the question becomes: Was the decrease in learning efficiency due to a lowered ability of the leg muscles to contract? Did the drug in some way affect the striped musculature so as to curtail its contractile power?

A fact which bears directly upon the answer to this question is to be

found in the procedure of the present experiment. It will be recalled that the extent of the unconditioned flexion response was held constant both for the training under Nembutal and for the non-drug trials. The unconditioned stimulus was adjusted, in other words, so as to produce a flexion reflex of 4 in. no matter what the state of the S. To accomplish this result, it was obviously necessary to increase the intensity of the unconditioned stimulus when the animals were drugged. Under these circumstances, the muscular activity itself (in terms of the size and vigor of the unconditioned response) seemed to be quite unaffected. It does not appear, therefore, that the loss of efficiency in the conditioned behavior which developed with Nembutal should be ascribed to a reduction in the contractile ability of the muscles. Our observations would indicate that contractile ability was unimpaired providing sufficiently strong stimuli were employed.

Receptors. There can be no doubt, on the other hand, that a marked change in sensitivity occurs after Nembutal has been given. The RL can easily enough be demonstrated to be raised by a depressing drug of this nature. In the present experiment, the very necessity of increasing the strength of the unconditioned stimulus in order to keep the unconditioned response constant, was evidence of the lowered sensitivity produced by the drug.

A careful differentiation should be made at this point between "stimulus-intensity" and "intensity of stimulation." If the "stimulus" is thought of as the energy impressed upon the receptor, then the "stimulation" may be considered as the effect produced in the receptor mechanism of the organism by the stimulus. It was necessary to increase the stimulus-intensity in this experiment; but there are good grounds for saying, with regard to the unconditioned electric shock, that the intensity of stimulation was constant. The "intensity of the stimulus" was in this instance readable from a voltmeter. The "intensity of stimulation" could be inferred from the magnitude of the response produced by the stimulus. Since the unconditioned response was held constant by increasing the stimulus under Nembutal, the stimulation producing the response may be thought of as constant throughout the entire experiment.

If one wishes to accept this line of reasoning for the unconditioned stimulation, it appears at once that it cannot be applied in the same way to the *conditioned* stimulation. The conditioned buzz-stimulus was fixed at 50-55 db. above the human threshold. Nothing was done at all about the intensity of the conditioned stimulation since no technique (such as introspection) was available for measuring conditioned stimulation. Up to this point in the present analysis it would appear then that one reason for the

lowered efficiency of learning under the drug may be that the Ss did not hear the conditioned stimulus as well in the drugged state. It is even conceivable that in the deepest part of the narcosis, they did not hear it at all! Quite possibly there were fundamental differences, therefore, in the intensity of the stimulation received by the Ss from the conditioned and unconditioned stimuli presented. The criteria of "fixed intensity" which were employed were different in the two instances. In one case (unconditioned stimulus) the intensity of the stimulation was held constant; in the other case (conditioned stimulus) it was the intensity of the stimulus which was held constant.²⁰

Nervous system. The nervous system remains to be considered as a causal factor in the decrease in learning efficiency. There would seem to be a strong probability that the main depressing effect was neural in origin. The great mass of evidence from extirpation experiments all points to a reduction in learning efficiency as a result of neural dysfunction. That the barbiturates, of which Nembutal is a typical representative, affect especially the higher centers of the nervous system, has already been pointed out.²¹ The pharmacologists assert that the focus of the depressing effect produced by the barbiturates upon the central nervous system is in the cortical and hypothalamic regions.²² Direct electrical stimulation of the motor cortex under barbital anesthesia is much less effective than under ether and other non-barbiturates. Yet the spinal reflexes are not so much affected by the barbiturates as they are by other depressing drugs.²³

In the light of such evidence, the writers can find small reason to question the probability that the present experiment demonstrates the effects of a strong cortical and sub-cortical depressant upon the ability to learn and to reproduce learned material. The principal loophole in such an inference is the uncontrolled nature of the conditioned stimulation in this study, a matter upon which further data will shortly be forthcoming.

SUMMARY AND CONCLUSIONS

Six dogs were given a total of 2400 conditioning trials by the buzz-shock method. Half of these trials were presented while the animals were under the influence of hypnotic doses of Nembutal and half were given in the normal state. By rotating the order of different groups of trials each dog's performance under the drug was

²⁰ Experiments are now in progress in the Indiana Conditioning Laboratory which keep at a constant level the intensity of the conditioned stimulation. The writers hope soon to have data on this possibility as a source of the lowered efficiency of conditioning produced by a depressing drug.

²¹ Cf. footnotes 11 and 12.

²² Cf. Sollman, *op. cit.*, 735.

²³ Fulton and Keller, *op. cit.*, 143-144.

made to serve as a control for his performance without it. Following are some of the principal findings.

(1) General behavior in the drugged state was characterized by muscular unsteadiness, staggering, and an apparently fluctuating contact with the environment.

(2) Conditioning was nevertheless possible under hypnotic doses of Nembutal, but it was less efficient and proceeded at a lower or more depressed level than it did in the normal state.

(3) The learning curve obtained under these circumstances, computed both from the frequency and the amplitude of the CRs, was sigmoid in character like the usual conditioning curve.

(4) Its form was influenced not only (a) by the learning process, but also (b) by the wearing off of the drug, the effect of which did not remain constant throughout a long training period.

(5) By adjusting the intensity of the unconditioned stimulus, the unconditioned response was maintained constant in extent and regular in occurrence, even though the amplitude and frequency of the conditioned response varied considerably.

(6) The latency of the CR was greater under Nembutal than without it; but measurements of latency throughout the learning period showed no evidence of the learning process either under Nembutal or in the normal state.

(7) The duration or length of the conditioned response was on the whole shorter in the drugged state.

(8) In the undrugged state, long CRs, which were made at the start of training, tended to become shorter as training continued. In conditioning under Nembutal, the CR remained more nearly of the same length, which was not far from the shorter duration to which the normal CR approached.

(9) The retention of conditioned responses which were built up under Nembutal could be demonstrated at a later time when the Ss were not under the influence of the drug.

(10) The reverse phenomenon was not obtained, however, in this experiment. The depressing effect of the drug masked any evidence of the retention of previously learned CRs, when buzz-alone stimuli were used to test for the previous learning.

(11) The contractile power of the muscles was apparently not impaired by hypnotic doses of the drug, providing stimuli of sufficient intensity to produce the necessary responses were employed. It seems likely, therefore, that the loss in efficiency of learning was not localized in the muscle tissues.

(12) It is possible that this loss is in part traceable to the receptors whose sensitivity was markedly reduced by the action of Nembutal.

(13) Since the barbiturates are known to exert a depressing effect upon the cortical and sub-cortical ganglia, the most plausible inference, in the opinion of the writers, is that the decrease in learning ability was of neural origin. The present experiment may, therefore, be said to demonstrate the effect of a powerful neural depressant upon the ability to learn and to reproduce learned material.

AN EXPERIMENTAL STUDY OF THE RELATIONSHIP BETWEEN ATTENSITY AND INTENSITY

By WELLINGTON A. THALMAN, Southern Illinois State Normal University
and KARL M. DALLENBACH, Cornell University

In this experiment we sought an answer to two questions that concern the dimensional status of attensity. (1) Is attensity an independently variable dimension of experience? (2) As a quantitative dimension, does attensity—in the same manner as intensity, duration, and extent—come under Weber's law?

Independent variability is one of the tests of an experiential dimension. Yet in this respect the status of attensity is not clear, at least so far as its relations with intensity are concerned. The difficulty is that both dimensions vary with the physical intensity of stimulus. Stimulus-intensity is, on the one hand, the prime condition of dimensional intensity and, on the other hand, it is also a determiner of the degree of attensity. The case is analogous to that of tonal pitch and volume, both of which vary with the frequency of the tonal stimulus. In their case, independent variability was nevertheless demonstrated by the experimental showing that their differential limens are unlike.¹ While both tonal attributes vary with stimulus-frequency, yet pitch-discrimination and discrimination of volume do not require the same stimulus-difference. Carrying out the analogy, we put the question of the independent variability of dimensional attensity and intensity to the same experimental test. We determined limens for attensity and intensity under identical conditions of intensive stimulus-variation. In doing so we also obtained an answer to the question whether Weber's law holds for attensity as it does for the other quantitative dimensions of experience.

APPARATUS AND PROCEDURE

Apparatus. The liminal determinations were made by means of the attensimeter devised by Dallenbach.² This exposure apparatus provided two circular spots of light, 2.5 cm. in diameter, one on either side of a point of fixation. The distance of each spot from the fixation-point was 13.77 cm., representing a visual angle of 7°50' at the observation-distance of one meter.

The source of light was a 500-w. lamp. The exposure-time, 106 ± 1.86 ms.,

* Accepted for publication June 15, 1930. The publication of this study has been long delayed, awaiting the completion and publication of J. W. Macmillan's study, which follows, *infra*, 374-384.

¹G. J. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 121-164.

²K. M. Dallenbach, An apparatus for the study of the conditions of clearness, *ibid.*, 34, 1923, 93-95.

was controlled by a rotating disk with one sector removed as in the Whipple tachistoscope.

In using the method of constant stimulus-differences, the intensity of the stimuli was controlled by means of slides with a varying number of perforations, placed between the light-source and the stimulus-field; every perforation had a diameter of $3/16$ in. This arrangement allows us to express the physical intensity of the stimuli in terms of the number of perforations in the slides. Thus the two standard stimuli were 10 and 30; standard 10 calibrated at 130 foot-candles.

Procedure. The experimental hour always began with 10 min. of dark adaptation. Then came a warming-up series, followed by about 20 experimental series. Several rest-periods were provided, and *O* was asked to report whenever his eyes became fatigued.

With two standard stimuli, two space orders, two instructions, and 3 *O*s, the number of presentations totaled 1200—50 for each experimental variation. When judgment was 'doubtful,' the presentation was repeated immediately and without comment.

Observers. The *O*s were Mr. O. D. Anderson (*An*), Mr. W. T. James (*Ja*), and Miss E. R. Moul (*Mo*), all graduate students with practice in observation and report.

Instructions. The experiment was repeated twice, first with instructions calling for reports of attentivity, then for reports of intensity. The instructions for attentivity, given at the beginning of every experimental hour, were as follows:

"At the signal 'Ready' fixate the spot of light before you. At 'Now' two areas of light will be shown. Report which, if either, is the more attentive, clear, or vivid, *i.e.* which of them catches your attention the more. Give the position of the more attentive as 'right' or 'left,' whichever the case may be. If they are equally attentive, say 'same.' If you are uncertain, report 'doubtful.'

For intensity, the differential part of the instructions read:

"Report which, if either, is the more intensive. Give the position of the more intense. . . . If they are equally *intense* . . ."

Preliminary experiments. Before entering upon the experiment the *O*s were given special training in the discrimination of degrees of attentivity by methods devised by Geissler³ and extended by Dallenbach.⁴ In the first method, *O* listened to two metronomes, beating at the rates of 100 and 120 strokes per min., directly back of a screen 1 m. from *O*. He was instructed as follows:

"When the signal is given, direct your attention to the sounds and count the number between coincident strokes. After $1\frac{1}{2}$ min. you will be asked to report. Describe the pattern of your experience during the period of observation."

The same stimuli together with various tasks were employed in the second method with the following directions:

"When the signal is given, direct your attention away from the sounds and

³ L. R. Geissler, The measurement of attention, *ibid.*, 20, 1909, 510 f.

⁴ Dallenbach, The measurement of attention, *ibid.*, 24, 1913, 467-468; The measurement of attention in the field of cutaneous sensation, *ibid.*, 26, 1915, 444-446.

(1) repeat to yourself the letters of the alphabet backward; or (2) keep adding 7 continuously; or (3) keep multiplying by 2 continuously; or (4) starting with 400, keep subtracting by 2 continuously; or (5) count backwards starting with 1000. Describe the pattern of your experience during the period of observation."

The following typical reports will indicate the character and value of the training:

An. "At the very first, the experience has the character of scatteredness, a 'helter-skelter' sort of thing. Then suddenly one rate stood out."

"When one rate is clear, the other rate is in the background. The only time that both rates are in the focus at the same degree of clearness is when I let myself go and let the sounds come in as they will."

Ja. "By going back to my task, the sound became less clear. I don't think the task was interesting enough though to keep the sounds out altogether. The experience seemed to be a shift between two levels. There were times when the lower level was more clear than at other times."

"In getting back to the task of counting the letters of the alphabet backwards, the sounds assumed a place in the secondary level. In the secondary level at times I was even conscious of the difference in beat."

"While performing the task it seemed as if the sounds were driven out to the edge of consciousness automatically."

Mo. "After the signal I multiplied quite readily and continuously up to 512 and the strokes were very vague. I might have been able to say there was some noise somewhere, but it wasn't more specific than that. Then there is a complete shift. Speed and accuracy of the performance break down and the strokes become focal."

In addition to the preliminary practice in attensity, *O* was asked at the end of a series of 25 presentations in the main experiments for a description of his experience in making the judgments. These descriptions report the *Os'* criteria of attensity and intensity.

RESULTS

The lower and upper limens for attensity and intensity with the degree of precision of each together with the points of subjective equality, presented separately for every *O* and each of the two standards and space-orders, appear in Table I.

The results show the points of subjective equality of two of the *Os*, *An* and *Ja*, to be consistently closer to the standard for intensity than they are for attensity. This significant difference is clearly exhibited in Table II. For example, with standard 10 and the variable on the left, *An's* points of subjective equality are 7.4 for attensity and 9.7 for intensity. All the other values show the same relation. This clear-cut difference, however, is lacking in the results of the third *O*, *Mo*. In her case, the points of subjective equality are approximately the same for attensity and intensity, although the attensity-values, taken by themselves, are of the same order as those of the other *Os*. *Mo's* divergent results in the intensity-series, which came later in the experiment, are clearly traceable to excessive eye-strain. In fact, owing to this condition, her judgments were so irregular that the up-

per limen for intensity with standard 10 to the left could not be computed.

Weber's law and attensity. The determination of relative *DLs* is complicated by the large space-error and also by the fact that, to spare our *O*s, we used only two standards. We may, however, treat the results as if they were obtained by the method of equivalents, and compare the

TABLE I
LOWER AND UPPER LIMENS FOR ATTENSITY AND INTENSITY WITH THE DEGREE OF PRECISION AND POINTS OF SUBJECTIVE EQUALITY, STANDARDS 10 AND 30, BOTH SPACE-ORDERS, ALL *O*s
Standard

O		10 right		10 left		30 right		30 left	
		Att.	Int.	Att.	Int.	Att.	Int.	Att.	Int.
An	L ₁	6.79	8.72	12.86	10.40	22.33	25.71	49.43	40.06
	h	0.21	0.13	0.07	0.78	0.06	0.03	0.02	0.04
	L _u	7.99	10.67	12.46	8.27	25.03	27.32	38.63	39.46
	h	0.19	0.13	0.08	0.07	0.05	0.04	0.03	0.04
	PSE	7.36	9.66	12.65	10.20	23.58	26.60	43.50	39.75
Ja	L ₁	5.16	7.94	17.47	12.90	15.41	19.19	60.43	30.75
	h	0.28	0.15	0.02	0.09	0.10	0.07	0.02	0.04
	L _u	6.84	10.08	5.55	10.24	21.07	22.12	21.51	23.47
	h	0.24	0.16	0.03	0.10	0.08	0.08	0.01	0.04
	PSE	5.95	9.03	12.77	11.54	17.88	20.72	44.89	27.10
Mo	L ₁	3.42	3.18	24.83	32.06	11.41	10.24	84.79	80.98
	h	0.43	0.40	0.04	0.06	0.13	0.11	0.02	0.02
	L _u	5.02	5.98	16.05	—*	16.33	18.38	59.86	8.70
	h	0.34	0.25	0.04	—*	0.10	0.09	0.02	0.01
	PSE	4.13	4.26	20.94	—*	13.52	13.17	71.06	64.97

* Due to eye trouble during this series, Mo's judgments were too irregular to compute these values.

ratios of the lower and upper limens to the two standards *when the variables are on the same side*. Such a comparison is shown in Table III.

The 'Diff.' columns in Table III show that the difference between the ratio of the limen to one standard and the ratio to the other standard is less for attensity than for intensity in six cases and is greater in only two cases. Since the ratios for attensity are thus on the average better than those for intensity, which is known to follow Weber's law, attensity must follow the same law.

Space-error. Other experiments have shown that stimulus-position is a condition of attensity,⁵ and that the left is favored when *O* is right-handed

⁵ Dallenbach, Position vs. intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-286.

and the right is favored when *O* is left-handed.⁶ Both findings are confirmed in our own experiment. All of our *O*s, when tested with the Downey

TABLE II
POINTS OF SUBJECTIVE EQUALITY FOR ATTENSITY AND INTENSITY, STANDARDS 10 AND 20,
BOTH SPACE-ORDERS

O	Dimension	Variable left	Standard	Variable right
<i>An</i>	Att.	7.4	10	12.7
	Int.	9.7	10	10.2
	Att.	23.6	30	43.5
	Int.	26.6	30	39.8
<i>Ja</i>	Att.	6.0	10	12.8
	Int.	9.0	10	11.5
	Att.	17.9	30	44.9
	Int.	20.7	30	27.1
<i>Mo</i>	Att.	4.1	10	20.9
	Int.	4.3	10	—
	Att.	13.5	30	71.1
	Int.	13.2	30	65.0

questionary,⁷ were found to be right-handed, *Ja* being of the RRR type and the other two (*An* and *Mo*) of the RRL type. In agreement with this con-

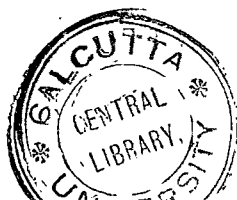
TABLE III
RATIOS FOR ATTENSITY AND INTENSITY OF UPPER AND LOWER LIMENS TO STANDARDS
10 AND 30, BOTH SPACE-ORDERS

O	Limen	Standard right			Standard left			
		10	30	Diff.	10	30	Diff.	
An	lower	Att.	67.9	74.4	+ 6.5	77.8	60.7	-17.1
		Int.	87.2	85.7	- 1.5	96.2	74.9	-21.3
	upper	Att.	79.9	83.4	+ 3.5	80.3	77.7	- 2.6
		Int.	106.7	91.1	-15.6	120.9	76.0	-44.9
Ja	lower	Att.	51.6	51.4	- 0.2	57.2	49.6	- 7.6
		Int.	79.4	63.9	+ 5.5	77.5	97.6	+20.1
	upper	Att.	68.4	70.2	+ 1.8	180.1	139.9	-40.2
		Int.	100.8	73.7	-27.1	97.6	127.8	+30.2

dition, Tables I and II show consistently lower values for the point of subjective equality when the variable is on the left.

⁶ R. S. Burke and K. M. Dallenbach, Position vs. intensity as a determinant of attention of left-handed observers, *ibid.*, 35, 1924, 267-279.

⁷ J. E. Downey, Types of dextrality and their implications, *ibid.*, 38, 1927, 321-322.



The magnitude of the space-error, computed as half the difference between the points of subjective equality on the right and the left, is indicated in Table IV. This Table shows the error to be consistently greater for attentivity than intensity, a result to be expected if stimulus-position favors attentivity.

Attensity and movement. To check on the Os' reports they were asked, at the end of a series of 25 presentations, for a description of their attentive and intensive judgments. A striking feature of these descriptions is the

TABLE IV
SPACE-ERROR FOR EVERY O AND FOR BOTH ATTENSITY AND INTENSITY WITH
STANDARDS 10 AND 30

O	Standard			
	10		30	
	Att.	Int.	Att.	Int.
An	2.65	0.27	9.96	6.58
Ja	3.41	1.25	13.51	3.19
Mo	8.41	—	28.77	25.60

number of motor references in the attentive judgments, as shown in the following excerpts.

An. "Eye movement in a rightward direction followed immediately by a left eye-movement."

"It makes me look at it."

"An incipient movement leftward."

"An incipient turning of the eye toward the left."

Ja. "At times one pulls me toward the right or toward the left."

"Eye moves to the right."

Mo. "My attention shifted to left."

"There was something about the right one that drew it to me. In the next instant the left light stood out more clearly."

"The right light drew me toward it."

"At the first instant the experience did not consist in anything more than a looking to the right."

SUMMARY AND CONCLUSIONS

After training in the discrimination of degrees of clearness or attentivity, differential limens were obtained for attentivity under conditions of visual stimulation. Then intensive limens were obtained under the same conditions. In connection with the space-error, the Os were tested for handedness.

The experimental results may be summarized as follows:

(1) The point of subjective equality is nearer the standard in comparisons of intensity than it is in comparisons of attentivity. The attentive limen

is larger than the intensive limen. Attensity is thus shown to be an independently variable sensory dimension of experience.

(2) Attensity, like the other quantitative sensory attributes, is governed by Weber's law.

(3) The space-error is greater in magnitude for attensity than for intensity, and its direction agrees with the *O*'s handedness. That stimulus-position is a condition of attensity is thus again confirmed.

(4) The occurrence of motor references in reports of attensity suggests the need of special investigation.

EYE-MOVEMENTS AND ATTENTION

By JOHN WALKER MACMILLAN, University of Maryland

The problem of this study is to determine the relation between visual attention and eye-movement. Its solution is of practical as well as of theoretical significance. If attention and eye-fixation vary coördinately, then the motor theory of attention finds support and an objective and practical method of measuring visual attention is at hand. Without troubling to examine the underlying assumptions, many investigators, particularly in the field of advertising, have attempted to gauge the attentional value of their displays by means of the eye-fixations or eye-movements of the subjects regarding them.

Thus Scott, in 1902, undertook to determine the habits of magazine readers in a public library by noting whether, at the moment of observation, they were looking at advertisements or at stories.¹ In 1924, Nixon investigated various conditions of effective advertising by presenting to his Ss pairs of advertisements differing in such factors as color, content, size, type, and layout, and noting which member of a pair "gets the most visual fixation."² These and all similar studies make the assumption that fixation and attention are one, that at what a person is looking is to what he is attending.

Guilford, in 1936, was the first to test that assumption experimentally.³ His problem was to discover "whether eye-movements could be used at all as indicators of attention values of stimuli."⁴ Concerned with the objective measurement of attention, he investigated the possibility of "using the direction of the first significant eye-movement after the exposure as the objective indicator of the more attention-compelling side of the field."⁵ He resorted to photography in place of the direct observation of eye-movement. Presenting paired sets of letters and geometric forms, he sought to control the direction of attention by varying such factors as position, isolation, size, and novelty. His Os were given, in alternation, two observational tasks. They were "to report which part of the field is most prominent or most compelling, most intriguing, or the part you feel inclined to look at,"⁶ a task interpreted as involving 'attributive clearness.' Alternatively, they were to "report what you have seen. Name or describe as many objects as you can observe."⁷ This second task was taken to involve 'cognitive clearness.'

Guilford's photographic records revealed "a surprisingly large number of eye-

* Accepted for publication August 11, 1940. This study was made in the Psychological Laboratory of Cornell University under the direction of Professor Karl M. Dallenbach.

¹ W. D. Scott, *The Psychology of Advertising*, 1902, 222.

² H. K. Nixon, Attention and interest in advertising, *Arch. Psychol.*, 12, 1924, (no. 72), 1-67.

³ J. P. Guilford, Varieties and levels of clearness correlated with eye-movements, this JOURNAL, 48, 1936, 371-388.

⁴ *Ibid.*, 383.

⁵ *Ibid.*, 382.

⁶ *Ibid.*, 375.

⁷ *Ibid.*, 375.

movements . . . even more movements in the 3 sec. just before the exposure than in the 3 sec. just after the exposure."⁸ The cognitive instructions yielded more movements than the attributive instructions. Moreover the correlation between eye movement and clearness turned out to be low. Between the direction of the first significant movement after exposure and the side of the field reported as attributively clearer the correlation was $+0.215$, and a little higher in the case of cognitive clearness. Thus Guilford's hopes of discovering an objective measure of attention were blasted.

While we find no reason for doubting the low correlations Guilford established, it nevertheless seemed to us worthwhile to reinvestigate the problem with certain modifications in the experimental conditions.

In the first place, the conditions of stimulation in Guilford's experiment, involving sets of letters and forms, were fairly complex, and as Guilford himself intimates, this complexity encouraged eye-movements. We adopted, therefore, the method of inducing attentional differences that has been developed by Dallenbach⁹ and recently used with improvements in apparatus by Thalman and Dallenbach.¹⁰ This method reduces stimulation to the simplest possible form: to two small areas of light. Attention is controlled—not by several factors, each complex in itself, such as position, isolation, size, and novelty—but by a simple variation in the intensity of light areas. This method is also simpler, as well as surer, on the side of observational instruction. It calls only for reports on attentivity (*i.e.* vividness or clearness),¹¹ a type of report consonant with the simplicity of presentation. More complex presentations, such as Guilford's, certainly render attributive reporting more difficult.

Secondly, we determined upon a condition of complete dark adaptation. The frequency of eye-movements in Guilford's experiment may have been influenced by the condition of relative light adaptation of his Os. . .

Thirdly, the photographing of eye-movements introduces the necessary complication of reflecting light from the cornea. Guilford, as well as other investigators, have used a powerful beam of strongly visible light. By using infra-red light, we were enabled to keep dark adaptation unimpaired and to reduce the influence of this accessory factor. While, as we shall see, the presence of the light betrayed itself in the experimental results and so cannot be left out of consideration altogether, it remained generally unnoticed during the period of observation.

⁸ *Ibid.*, 387.

⁹ K. M. Dallenbach, Apparatus for the study of the conditions of clearness, this JOURNAL, 34, 1923, 94 f.

¹⁰ W. A. Thalman and K. M. Dallenbach, An experimental study of the relationship between attentivity and intensity, *supra*, 367-373.

¹¹ E. B. Titchener, The term 'attentivity,' this JOURNAL, 35, 1924, 156.

APPARATUS AND PROCEDURE

Our technical problem was to present to *O* two stimulus-objects in such a way that one was more likely to catch attention than the other, *i.e.* be

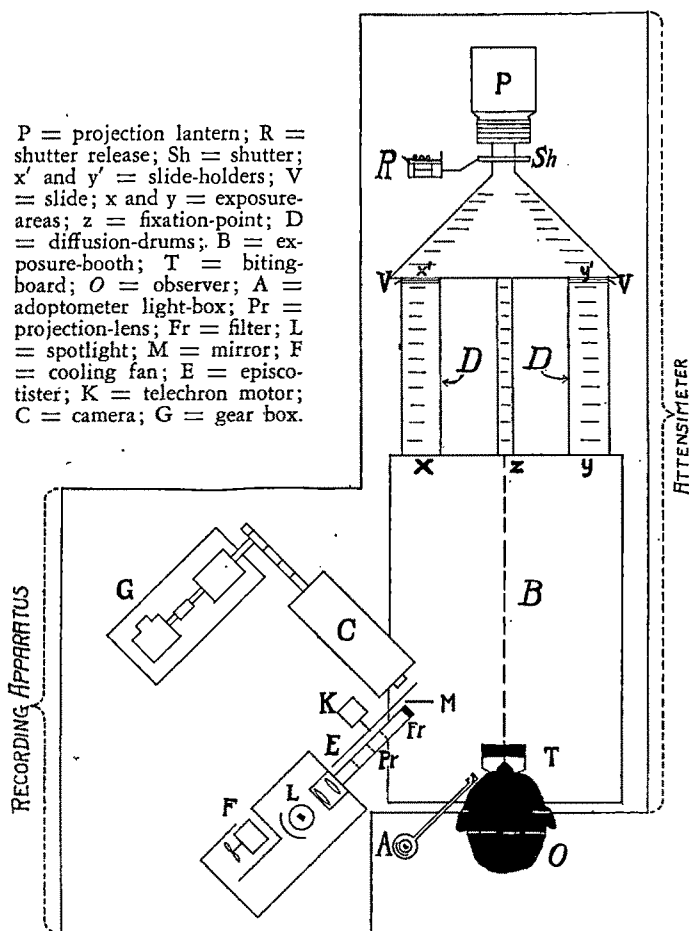


FIG. 1. EXPOSURE AND RECORDING APPARATUS
(Top view)

more vivid or clearer than the other, and to record any eye-movements occurring during the period of observation.

Exposure-apparatus. The stimulus-objects were presented by the attensimeter devised by Dallenbach and modified by Thalman and Dallenbach¹² (Fig. 1). During

¹² *Op. cit.*

the observation two circular areas of light (x and y) 8 cm. in diam. appeared at a distance of 1 m. from O's eyes. Between the centers of the two light areas the distance was 27 cm. The exposure-time was controlled by a Luc 6 plate camera-shutter (Sh) which was actuated by the release of a spring catch (R). The exposure-times were thus kept constant at approximately 60 ms.

Fixation. O's fixation was controlled by the fixation-point (z), centered between the two stimulus-areas, and by a biting-board (T).

Recording apparatus. In addition to the attensimeter an apparatus for recording eye movements was employed (Fig. 1). The camera (C), made from a Kodak Model C 16-mm. moving picture projector, was placed with the lens 8 in. from O's left eye and 35° to the left of his line of regard. The projector was stripped, leaving only the film-drive sprocket-wheel, spool-holders, and exposure-gate on the chassis. An f:3.5 lens from a Bell and Howell Filmo 16-mm. movie camera was mounted in place of the projection lens and calibrated for its position. It was used at its widest aperture throughout the experiment. The camera was enclosed in a light-tight box with the lens projecting from one end. One side of the box was hinged to facilitate the placing and removal of film.

The camera was driven by a chain from a gear-box (G) consisting of a sewing machine motor and a 900:1 reduction gear mounted on felt to reduce noise.

The film used in the camera was 16-mm. infra-red sensitive film made by the Eastman Kodak Co. The camera, although not a constant speed drive, pulled the film past the lens at approximately $\frac{1}{2}$ frame per sec.

To make it possible to photograph eye movements a spot of light was reflected from the cornea of the eye to the camera. This was thrown by a spotlight (L) made from the lamp socket, reflecting mirror, condensing and projection lenses which had been removed from the projector. The lamp bulb had a 500-w. concentrated filament. This was cooled by a fan (F) made from the motor taken from the projector. The hum of this fan proved advantageous during the experiment as it served to mask the noise made by the operation of the camera, thus enabling E to run the camera without O's knowledge. The projection lens (F), having too wide an angle for our purpose, was elongated, and disks with small holes were placed at three places inside the barrel of the lens in order to reduce the size of the beam of light and to parallelize the rays.

The spotlight (L) was placed at right angles to the camera box (C) in such a way that the light was reflected from a small adjustable mirror (M), directly beneath the camera lens, to the cornea of O's left eye. During the adjustment of the apparatus the beam of light was focused on O's cornea at reduced intensity, but after the filter was put in place the light was increased to its maximum intensity. The filter (Wratten No. 87 extra dark infra-red), when placed over the end of the projection lens, absorbed most of the visible rays.¹³ Those remaining were invisible except when fixated, and then they were seen only as a faint red glow.

In order to obtain a time-line on the film a telechron motor (K) with a constant speed of 60 r.p.m. was fitted with an episotister (E), which shuttered and exposed the lens of the camera alternately every $\frac{1}{8}$ sec. An adaptometer light-box (A), a light-tight chamber with a hollow rod projecting from one side through which light

¹³ This filter absorbs 100% of light at 740 mμ and 90% at 760 mμ and transmits nearly 100% at 800 mμ.

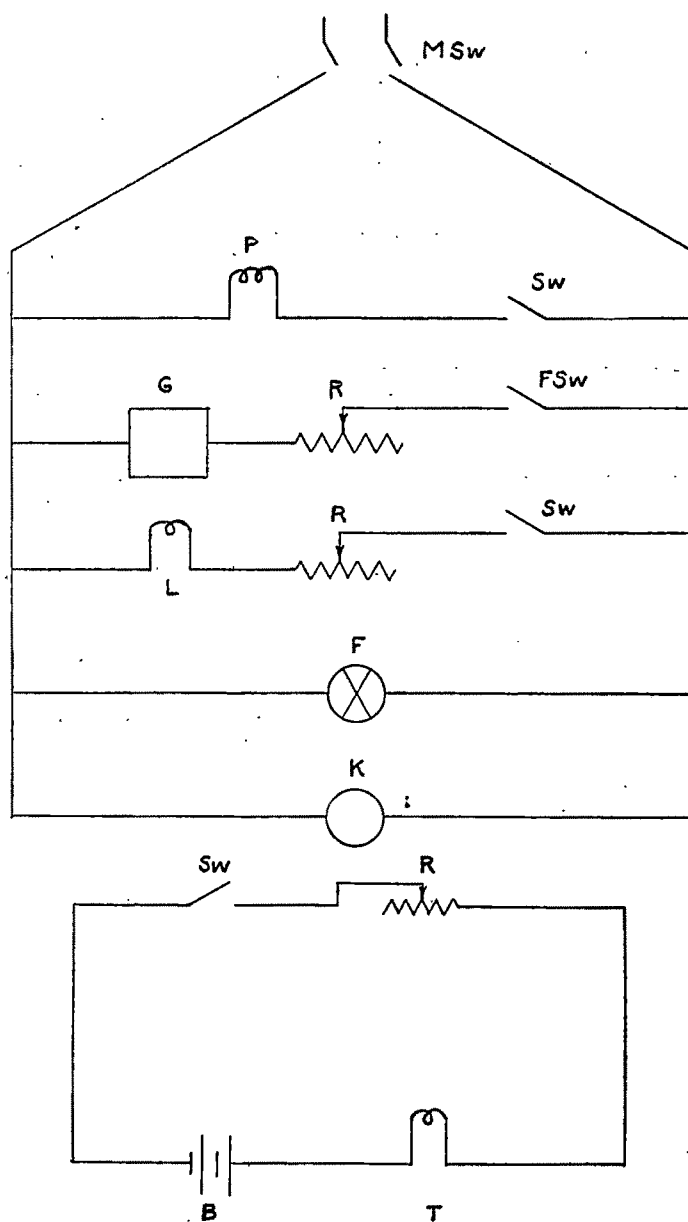


FIG. 2. SCHEMATIC WIRING DIAGRAM

MSw = main switch; FSw = foot-switch; Sw = toggle switches; P = projection lamp; G = gear box; R = rheostat; L = spotlight; F = cooling fan; K = telechron motor; B = dry-cells; T = lamp of the fixation-point.

travels and is reflected by a right-angled mirror through a small hole near the end, was adjusted and wired so that when the exposure-areas (x and y) were shown a spot of light was thrown on the film just beside O 's eye-record. Thus a record of the exact time of the exposure was obtained.

Calibration of the apparatus. In a correlation of eye-movement and attention it is important to determine how the magnitude of the movements actually executed compares with the extent to which the eyes would have to travel in order to fixate one of the stimulus-areas. At the conclusion of the experiment, therefore, the apparatus was calibrated for every O by having him sit, as in an observation-period, with the inside of the booth lighted.¹⁴ The exposure-field was marked off on each side of the fixation-point by white chalk marks at distances of 3, 6 and 9 cm. The center of each exposure-window, $13\frac{1}{2}$ cm. from the fixation-point, was also marked with chalk. These distances correspond to approximately 2° , 4° , 6° , and 9° of arc from the fixation-point. O was instructed to look at each of these points in turn and then back to the fixation-point, and these eye-movements were recorded on the film. With these calibrated records the experimental records were compared under low power magnification.

Wiring of the apparatus. A schematic diagram of the wiring is shown in Fig. 2. The foot switch (FSw) was located conveniently near E 's foot. The cooling fan (F) and episiotister motor (K) were arranged in such a way that it was impossible to leave them off while an experiment was in progress. It was found unnecessary to vary the rheostat (R) which controlled the camera speed and the speed of the cooling fan after they had once been adjusted correctly.

Observers. There were three O s in the experiment: Miss Margaret Hubbard (Hu), Miss Marion Kadel (Ka), and Mr. Oliver Lacy (La), all graduate students in psychology. Hu alone came to the experiment with practice in observation, and all the O s were unpracticed in judging attensity at the beginning of the experiment.

Procedure. The experimental plan is shown in Table I. The stimulus-areas (Fig. 1, x and y) were presented to the O s by the method of limits. Every O observed in 240 series with the standard on the right and 240 series with the standard on the left. A single observational period consisted of 10 series each way. Two periods constituted an experimental unit. The whole experiment thus divides into 12 units, as is indicated in the first column of the table.

The intensity of the stimulus-areas was controlled by means of slides (placed at x' and y') with varying amounts of perforation, see Thalman and Dallenbach.¹⁵ The standard light was 16% of the full intensity provided by the apparatus. The variable light was changed by steps of 1% except during the first four practice units, when changes of 4% were made in the descending series. This was true for Hu and La . the third O (Ka) had such a decided left-preference as to require differential treatment. For her the standard had to be set at 7% in the first practice unit, but thereafter it was 16%, the same as for the other O s. The variable steps were 1%

¹⁴ The apparatus was not calibrated for every O previous to the experiment because it was thought best to keep them in ignorance of the appearance of the exposure-apparatus so that visual acquaintance could not influence their observations.

¹⁵ *Op. cit.*, 367.

throughout the experiment when the standard was on the right for this *O*, whereas when the standard was on the left the steps were 4% during the first 4 units, 2% during the next 3 units and 1% thereafter. All these conditions are set forth in Table I.

To accustom *O* to the photographic conditions the spotlight on the cornea was introduced beginning with Unit 6. Photographs were then taken beginning with Unit 7, records being made for 8 series of observations in each of the next five units. The series to be photographed were selected at random, except that the first and last two series in an observation period were avoided. In Unit 11, *O* was instructed to keep his teeth in the biting-board for several seconds after making his

TABLE I
THE INTENSITY OF THE STANDARD AND VARIABLE STIMULUS-AREAS, THE EXPERIMENTAL CONDITIONS, AND THE JUDGMENTS OF THE *O*s IN THE DIFFERENT EXPERIMENTAL UNITS

Experimental unit	Hu and La			Ka			Conditions	Judgment
	Stand.	Var. steps		Stand.	Var. steps			
		Asc.	Des.		Left	Right		
1	16%	1%	4%	7%	1%	4%	cooling fan	attensity
2-4	16%	1%	4%	16%	1%	4%	cooling fan	attensity
5	16%	1%	1%	16%	1%	2%	cooling fan	attensity
6	16%	1%	1%	16%	1%	2%	light on cornea	attensity
7	16%	1%	1%	16%	1%	2%	photographed	attensity
8-11	16%	1%	1%	16%	1%	1%	photographed	attensity
12	16%	1%	1%	16%	1%	1%	photographed	intensity

observation. This was done in order to get records of any eye-movements that might be made after the exposure.

At the beginning of an experimental period *O* was seated comfortably, introduced his head into the exposure-booth (B) and became dark-adapted for about 10 min., when the instructions were read. Following the instructions *O* placed his teeth in the biting-board and the experimental series were begun.

Instructions. The instructions were: "At the signal 'ready' fixate the spot of light before you; at 'now' two areas of light will be shown; report which, if either, is the more clear, that is, the more vivid. Give the position of the clearer as 'right' or 'left' whichever the case may be; if they are equally clear say 'same'; if you are uncertain report 'doubtful'." These instructions were repeated until the conclusion of Unit 11. In Unit 12 the word 'intense' was substituted for 'clear' and the word 'intensity' for 'clearness.' This change in observational task was made, following the example of Thalman and Dallenbach,¹⁰ to check our *O*s' accuracy in reporting clearness, and particularly to ensure that they were not confusing it with intensity.

RESULTS

Reliability of the judgments of clearness. Before turning to the records of eye-movement, we shall consider the judgments of attensity. Our *O*s' reliability can be checked in two ways. The first check may be obtained

¹⁰ *Op. cit.*

through the *Os*' points of subjective equality. These are listed in Table II, separately for every experimental unit, for each *O* and for the two space orders. The regularity of the results of *Hu* and *La* is brought out very prominently when they are compared with those of *Ka*, our *O* with a decided left-preference. Her results evidence a progressive decrease in left preference, with relative stabilization after Unit 7, the beginning of the photographic units of the experiment.

We have a second check in a comparison of the results of Units 12, in which 'intensity' was judged, with those of Unit 11, the last of the 'at-

TABLE II

POINTS OF SUBJECTIVE EQUALITY IN TERMS OF PERCENTAGE OF STIMULUS-INTENSITY, FOR EVERY EXPERIMENTAL UNIT, FOR EACH *O* AND FOR BOTH POSITIONS OF THE STANDARD

(Value of standard = 16%)

Exper. unit	<i>Hu</i>		<i>Ka</i>		<i>La</i>	
	R	L	R	L	R	L
1	15.8±2.30	14.5±2.95	2.0±0.6	56.4±8.98	15.2±7.85	21.6±4.02
2	15.3±2.30	18.7±5.96	6.1±0.93	60.8±8.52	13.5±4.40	18.5±6.90
3	18.1±4.75	17.7±5.04	6.0±0.65	63.4±8.09	15.0±2.75	20.9±5.17
4	19.3±4.79	17.0±4.10	9.2±1.71	41.4±7.30	13.6±2.75	18.4±3.75
5	17.7±1.10	17.8±0.96	7.1±1.19	54.3±5.30	13.2±1.37	22.6±1.86
6	16.4±1.28	19.7±1.06	10.0±1.1	39.0±3.1	12.7±1.26	21.9±3.01
7	19.6±1.46	16.5±2.20	11.1±1.32	33.4±2.06	14.6±1.94	19.0±1.9
8	18.3±2.50	18.3±2.08	14.0±1.6	32.7±3.64	12.8±1.32	20.4±2.20
9	17.5±2.60	19.6±1.16	11.9±1.56	26.6±2.80	18.2±1.80	13.7±1.01
10	17.6±2.79	18.3±1.50	14.4±2.00	19.0±1.30	16.4±1.75	16.2±1.32
11	20.9±2.30	16.5±1.30	16.8±1.72	18.5±1.5	13.7±1.17	18.8±1.22
12	20.1±1.88	14.5±1.5	21.5±1.45	15.0±1.3	19.5±1.95	15.4±1.24

tensity' units. Thalman and Dallenbach found that a differential judgment of attentivity required a larger stimulus-difference than one of intensity,¹⁷ a result that holds also for our *Os* except when the variable is on the left. This discrepancy, however, need not reflect upon Thalman and Dallenbach's experiment nor cast doubt upon our own *Os*' ability to distinguish attentivity and intensity. The difference in the conditions of our experiment and Thalman and Dallenbach's, consisting in the presence of the photographing apparatus at *O*'s left, could easily be responsible for the discrepancy.

Records of the eye-movements. The first striking result obtained from an examination of our photographs is the extreme paucity of eye-movements. Table III presents this point clearly. Of a total of 687 photographic records only 577 were readable.¹⁸ Of these 45 showed significant eye-movements.

¹⁷ *Op. cit.*

¹⁸ Some of the records were discarded because of faulty manipulation of the film, others because of poor visual fixation, head movements, and bad focussing of the light on the cornea.

Although the small number of eye-movements makes any further analysis precarious, we nevertheless present in Table IV the relations obtaining between direction of eye-movement and direction of attention. Of the 45 eye-movements that did occur, 21 were to the right and 24 to the left. Of these, 28 were in the same direction as the judgment of clearer, and 17, the remaining, were in the opposite direction (indicated

TABLE III
THE NUMBER OF JUDGMENTS AND THE NUMBER OF EYE-MOVEMENTS MADE BY ALL
THE Os FOR BOTH POSITIONS OF THE STANDARD

	Hu		Ka		La	
	R	L	R	L	R	L
Total records	82	88	121	120	149	127
Records discarded	15	11	36	34	4	10
Records studied	67	77	85	86	145	117
Movements observed	2	5	9	8	10	11

by asterisks in the table). Thus no trace appears either of a preference in movement-direction or of a correspondence between direction of eye-movement and direction of attention, except for one *O*, *Hu*, who had the smallest number of eye-movements, *i.e.* 7. This *O* alone exhibits lateral preference (2 movements to the right and 5 to the left), and

TABLE IV
THE NUMBER, DIRECTION, AND EXTENT OF THE EYE-MOVEMENTS FOR ALL THE Os
AND FOR BOTH POSITIONS OF THE STANDARD
(Figures in parentheses show the extent of the movements in degrees.)

Movements	Hu		Ka		La	
	R	L	R	L	R	L
R	1(6)	1(2)	*3(2) 1(2)	2(2) *1(2) 1(4)	*4(2) 3(2)	4(2) *3(2)
L	1(2)	2(2) 2(4)	4(2) *1(2)	2(2) *2(2)	2(2) *1(2)	2(2) *2(2)

* Indicates that the movement was in the opposite direction to that of the judgment.

a perfect correlation between direction of movement and direction of attention.

The magnitudes of the eye-movements are indicated in Table IV by numbers in brackets. Of the 45 records, 41 showed an excursion between 2° and 4°, 3 between 4° and 6°, and 1 more than 6°. Only one of the movements, therefore, was of sufficient magnitude to indicate a shift of the eyes from the fixation-point to one of the stimulus-areas.

The foregoing results are illustrated by the enlarged reproductions of eye-movements shown in Fig. 3 which are magnified about 15 diameters. Samples from the records of each *O* and of various degrees of movement are given. The eye-records (A) have arrows showing the place and direction of movement. The times of exposure of the stimulus-areas (B) are shown just to the right of the eye-records. The exposure took place at

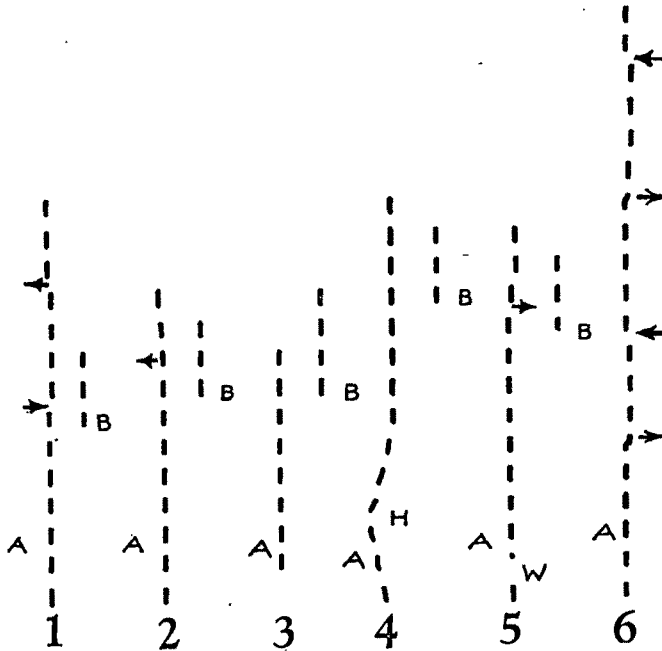


FIG. 3. SAMPLE REPRODUCTIONS OF THE RECORDS

A = eye-records; B = exposure; H = head-movement; W = wink. The arrows indicate the place and the direction of the eye-movements.

the beginning of the first dash, the recording light remaining on until the exposure-shutter was reset. Record 1, made by *Hu* in Unit 11, shows movement 2° to the left, the judgment at the time of the record was 'left.' Record 2, made by *La* in Unit 8, shows movement 2° to the right, the judgment being 'right.' Record 3, made by *La* in Unit 10, shows no movement, the judgment being 'right.' Record 4, made by *Ka* in Unit 6, shows a head movement (H) previous to the exposure and no movement at the time of the exposure. Record 5, made by *Ka* in Unit 10, shows movement of 4° to the left with judgment 'left.' Record 6 is a

calibration record made by *Ka*. It shows movement from the fixation-point to the position 2° left, back to the fixation-point, then 4° left and back to the fixation-point.

SUMMARY AND CONCLUSIONS

(1) In this experiment three *Os* reported which of two small light areas of variable intensity, exposed for about 60 ms. under conditions of full dark adaptation, was of greater attensity (dimensionally clearer or more vivid). During a selected number of observations, photographic records were taken of eye-movement by reflecting a small beam of infra-red light from the cornea. This beam was invisible during the observations.

(2) Our results corroborate Thalman and Dallenbach's in yielding differential stimulus-values for attensity and intensity. Exceptions are accounted for by the introduction of the eye-movement recording apparatus at *O*'s left.

(3) Under our conditions, which aimed at reducing the conditions of attention to the simplest terms possible, correlation between eye-movement and attention reduces to practically zero: (a) very few eye-movements take place (45 out of 687 records); (b) the eye-movements that do occur show no tendency to follow the direction of attention; and (c) the magnitude of the movements is less than is required to fixate one of the stimulus-areas.

(4) Under our conditions no lateral preference appears in the movements.

(5) A decided left preference in the case of one *O*, evidenced in the stimulus-values, proved itself amenable to practice, showing progressive decrease and final disappearance.

(6) From the point of view of technique we conclude that, while better photographic conditions than our own might succeed in bringing to light incipient movements of small magnitude, the next methodical step is to obtain potential records of the eye-muscles. In this connection it is of interest that in Thalman and Dallenbach's experiment, in which judgments of attensity were obtained under the same conditions as our own but without photographic recording of eye-movement, the *Os* reported eye-movement.¹⁹ Such reports, while not necessarily indicative of objective movement, nevertheless may have some basis in muscular action.

¹⁹ *Op. cit.*

A CRITICAL AND EXPERIMENTAL STUDY OF COLOUR PREFERENCES

By H. J. EYSENCK, University College, London, England

Experimentation in the field of colour preferences began comparatively early; Cohn's article in 1894 is generally regarded as the first definitely empirical approach to the various problems commonly subsumed under this heading.¹ Over 50 researches have been reported since that time, but little agreement has been reached even on the most fundamental points; namely, (1) the existence of a general order of preference for colours, (2) the relative popularity of saturated and unsaturated colours, and (3) differences in preferences for colours between the sexes.

(1) *General preference.* Cohn denied the existence of any general order of preference for colours; he found that among equally saturated colours preference depends exclusively upon individual taste.² Dorcus agrees with this statement to the extent of saying that in view of his results "we must be rather skeptical as to whether there is such a thing as colour preference."³ More recently Von Allesch, laying great stress on the chaotic diversity of preferences among colours, maintained that results such as his were too variable to justify us in saying that any colours were in general pleasing or displeasing.⁴ On the other hand, Walton, Guilford, and Guilford found "a common basis of feeling for different colours" among their observers, and they maintain that "there . . . remains sufficient agreement upon colour preferences to indicate a basic, biological cause of likes and dislikes for colours."⁵ St. George,⁶ and Garth⁷ also argue in favour of native preference tendencies. Murray and Spencer noted that preferences remained constant even when backgrounds of varying colour were used.⁸

(2) *Saturation-factor.* As regards the relative popularity of saturated and unsaturated colours, Cohn found that the more saturated colours were generally preferred.⁹ Walton and Morrison also found that "the saturated single colours were

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¹ J. Cohn, Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten und ihre Combinationen, *Philos. Stud.*, 10, 1894, 562-603.

² *Ibid.*, 599.

³ R. M. Dorcus, Color preference and color association, *Ped. Sem.*, 33, 1926, 432.

⁴ G. J. von Allesch, Die ästhetische Erscheinungsweise der Farben, *Psychol. Forsch.*, 6, 1924, 1-91.

⁵ W. E. Walton, R. B. Guilford, and J. P. Guilford, Color preferences of 1279 university students, this JOURNAL, 45, 1933, 322-328.

⁶ M. V. St. George, Color preferences of college students with reference to chromatic pull, learning, and association, *ibid.*, 51, 1938, 716.

⁷ T. R. Garth, Color preferences of 559 full-blooded Indians, *J. Exper. Psychol.*, 5, 1922, 417.

⁸ H. D. Murray and D. A. Spencer, *Colour in Theory and Practice*, 1939, 134.

⁹ Cohn, *op. cit.*, 599.

preferred."¹⁰ Similar results were obtained by Jastrow,¹¹ Luckiesch,¹² Bradford,¹³ and Minor.¹⁴ Washburn, on the other hand, found that the affective value of tints was highest, shades next, and saturated colours lowest.¹⁵ Major's results also did not confirm those reported by Cohn.¹⁰ Titchener attempted to reconcile the opposing points of view by suggesting that there were two different types of observer, one type preferring the saturated colours, the other the unsaturated colours.¹⁷ That such a type factor could only be of secondary importance is suggested by results obtained by Guilford, who found that of hue, tint, and chroma, "hue was the most important factor."¹⁸

(3) *Sex difference.* Many investigators have reported differences between the sexes in preferences for colours. Thus Dorcus found that "yellow has a lower affective value for the females than with the males;"¹⁹ St. George maintains that blue for men stands out far more than for women;²⁰ and Jastrow found that women preferred red to blue, men blue to red.²¹ Von Allesch, on the other hand, did not observe any such differences,²² and Garth, after examining several thousand cases, came to the conclusion that "the color sequences between the two sexes are about the same."²³

Opinions are sharply divided, then, on these three points. An attempt is made in this study to resolve these differences; both by means of an experimental investigation, and by means of a critical review of some of the more important researches. In the last part, the two sets of results are compared, and certain conclusions indicated.

EXPERIMENTS

Stimulus-colours. Ten Ostwald coloured papers were used, pasted onto cardboard without leaving a margin. The size of the papers was $5\frac{1}{2}$ by $3\frac{1}{2}$ in., and the colours were blue, red, green, violet, orange, yellow, all fully saturated; green, red, and orange tints; and a yellow shade.

Subjects. The Ss were mostly university students, men and women in equal numbers. A few professional men and women were also included, and one or two artists.

¹⁰ W. E. Walton and B. M. Morrison, A preliminary study of the affective value of colored lights, *J. Appl. Psychol.*, 15, 1931, 297.

¹¹ Joseph Jastrow, Popular esthetics of color, *Pop. Sci. Mo.*, 50, 1897, 361-368.

¹² M. Luckiesch, Note on color preference, this JOURNAL, 27, 1916, 251-256.

¹³ E. J. G. Bradford, A note on the relation and aesthetic value of perceptive types in color appreciation, *ibid.*, 24, 1913, 545-554.

¹⁴ A. Minor, Über die Gefälligkeit der Sättigungsstufen der Farben, *Zsch. f. Psychol.*, 50, 1909, 433-444.

¹⁵ M. F. Washburn, Note on the affective value of colors, this JOURNAL, 22, 1911, 114-115.

¹⁶ D. R. Major, On affective tone of single sense impressions, *ibid.*, 7, 1895, 57-77.

¹⁷ E. B. Titchener, *Experimental Psychology*, 1, part II, 1901, 152.

¹⁸ J. P. Guilford, Affective value of color as function of hue, tint, and chroma, *Psychol. Bull.*, 30, 1933, 679.

¹⁹ Dorcus, *op. cit.*, 416.

²⁰ St. George, *op. cit.*, 716.

²¹ Jastrow, *op. cit.*, 9.

²² Von Allesch, *op. cit.*, 91.

²³ T. R. Garth, *Race Psychology*, 1931, 128.

Procedure. Two separate experiments were carried out. In the first, 12 Ss were asked to rank the colours in order of preference. These rankings were then correlated, and the resulting table of correlations factor-analyzed. In the second experiment, 30 Ss were asked to rank the colours in order of preference; the average correlation between the rankings was found, and the average orders of the men and women were calculated separately.

RESULTS

In the first experiment, it was found that out of 66 correlations, only one was negative (-0.13 ± 0.33 , or less than one-half its standard error).²⁴ All the other correlations were positive, nine being larger than twice their standard errors. The average of all the correlations in the table was 0.28.

We can calculate from this coefficient the probable correlation of the average order of preference of our 12 Ss with the 'True Order,' *i.e.* the order of the whole population of which our Ss form only a sample, by means of a formula which I have shown elsewhere to be applicable to this kind of data.²⁵ This correlation has the value of 0.91. By reference to the table given in that article,²⁶ it can be seen that it would need 200 un-weighted rankings to obtain an average that would correlate with the 'True Order' to the extent of 0.99.

Two factors were extracted from the table of correlations. The first factor, which had positive saturations throughout, accounted for 34% of the variance; the second bipolar factor accounted for 4%. The residuals on which this second factor was based were not statistically significant when tested by Fisher's test of the difference between the theoretical and actual correlations, expressed in terms of their inverse hyperbolic tangents ($z = \tanh^{-1}r$).

The amount of variance contributed by the first general factor in this test is larger than the percentage contributed by a general factor of intelligence in an analysis of the intercorrelations of 56 intelligence tests, carried out by the present writer.²⁷ Hence we may conclude that there is more agreement between the orders of preference for colours given by our Ss, than there is between the results of tests of intelligence of the kind used in that investigation.

The nature of this general factor of colour-appreciation can be eluci-

²⁴ Following the advice given by R. A. Fisher (*Statistical Methods for Research Workers*, 1932, 46), the standard error of each correlation is given, rather than the probable error.

²⁵ H. J. Eysenck, The validity of judgments as a function of the number of judges, *J. Exper. Psychol.*, 25, 1939, 650-654.

²⁶ *Op. cit.*, 653.

²⁷ Eysenck, Primary mental abilities: A critical review, *Brit. J. Educ. Psychol.*, 9, 1939, 273.

dated by relating it to a general factor found in the analysis of 18 tests of aesthetic appreciation.²⁸ This factor, which was called 'T,' was shown to run through all the tests examined, and to correlate with general factors extracted from an analysis of the rankings of 64 polygons and 31 odours. On correlating the saturations of the 12 Ss taking part in the present experiment with their scores in a test of the 'T'-factor, a correlation of 0.53 was found, with a standard error of 0.22. It would appear, then, that the general factor of colour appreciation is saturated to the extent of 0.53 with the 'T'-factor, and hence the tentative explanation of the 'T'-factor given in my article may be suggested to apply to preferences for colours also.

The bipolar factor which appeared after the general factor had been eliminated was not significant, as mentioned above; but when the colours preferred by the Ss with the highest positive and negative saturations respectively were examined, it was found that this factor divided those who preferred pure, saturated colours from those who preferred unsaturated colours, *i.e.* tints and shades.

This finding is confirmed by the results of a similar experiment carried out by Mr. J. B. Parry, unpublished as yet. He had 15 Ss rank 24 Ostwald colours, 8 pure, 8 tints, and 8 shades. The average intercorrelation between the rankings was 0.21, and after the elimination of a general first factor which accounted for 26% of the variance, a bipolar factor was extracted, accounting for 13% of the variance and dividing those Ss who preferred pure colours from those who preferred the tints and shades. Both these and my own results are in entire agreement with the opinion expressed by Titchener²⁹ and with Cohn's later work.³⁰ Further confirmation will be found in the analysis of Von Allesch's data below.

In my second experiment, the average correlation between the rankings of the 30 Ss taking part was again 0.28; that is to say, the correlation between their average order and the 'true order' would be 0.96. The average rankings of the 15 men and the 15 women are given in Table I; in that table are also given the average rankings of the 12 Ss in the first experiment. Only the six pure colours are included in this table, for reasons which will later become obvious.

The rankings of the 15 men and the 15 women agree in placing blue, red, green, and violet above the two other colours; but they reverse

²⁸ Eysenck, The general factor in aesthetic judgments, *Brit. J. Psychol.*, 31, 1940, 94-102.

²⁹ Titchener, *op. cit.*, 151.

³⁰ J. Cohn, Gefühlston und Sättigung der Farben, *Philos. Stud.*, 15, 1900, 279-286.

the position of yellow, which is preferred by the women, and orange, which is preferred by the men.

We may now summarize our findings so far under three headings: (1) On the average, agreement between rankings of colours is as high as agreement between tests of intelligence. (2) Two types are found in the population, one preferring saturated, the other unsaturated colours. (3) There are no sex-differences, apart from a slight preference of women for yellow over orange.

DISCUSSION

(1) *General preference.* It might be said that the above conclusions are not based on large enough numbers of Ss to be very reliable. Such criti-

TABLE I
AVERAGE RANKINGS OF COLOUR PREFERENCES

Colour	Exper. 1	Exper. 2	
	12 Ss	15 men	15 women
blue	1.18	1.31	1.09
red	1.41	1.48	1.43
green	3.81	4.01	3.72
violet	4.71	4.37	4.56
orange	4.53	4.62	5.22
yellow	5.36	5.21	4.98

cism would leave out of account the very high correlations between the average orders and the 'true orders,' as calculated by means of the formula given in my article referred to above.³¹ In view of the fact, however, that at least the first of our conclusions is in contradiction to the opinion of many experts, some doubt might still be felt with regard to the adequacy of the research. Accordingly, a review was made of previous investigations, in order to discover how far the above conclusions agree with the results of other investigators.

Recent opinion seems rather to have moved away from the view that there is an 'objective' order of colour preferences for human beings. Both Chandler³² and Woodworth³³ cite with approval the work of Von Allessch, which according to Chandler "seems to render obsolete nearly all other work on the aesthetics of colour."³⁴ Von Allessch, as mentioned before, summed up his work by saying that the results were too variable to justify us

³¹ Cf. footnote 25.

³² A. R. Chandler, Recent experiments on visual aesthetics, *Psychol. Bull.*, 25, 1928, 720-732; *Beauty and Human Nature*, 1935.

³³ R. S. Woodworth, *Experimental Psychology*, 1938.

³⁴ Chandler, *op. cit.*, *Psychol. Bull.*, 25, 1928, 720-732.

in saying that any colours were in general pleasing or displeasing, and this opinion has since been endorsed by many others.

Chandler also attempted to show in another way the complete chaos, produced by the lack of agreement between different Ss' preferences. He assembled in the form of a synoptic table the results of a number of investigations, and pointing to the differences in the results argued that no objective order could be derived from them. Woodworth commented that the results in this table "leave a rather confused impression."³⁵

Our task, then, will be twofold. First, we must examine the results reported by Von Allesch, in order to see whether it is impossible to reconcile them with our own; and secondly, we must bring together the results of the various investigations that have been carried out so far, and determine the exact amount of agreement between them.

Von Allesch bases his conclusions largely on a table giving the preference judgments of 10 Ss.³⁶ He does not report any statistical treatment of his data, but seems to have relied on casual inspection entirely. Fortunately his results are given in full enough detail to make a statistical analysis possible.

I have correlated the rankings of the 10 Ss, and factor-analysed the resulting table of correlations. The majority of the correlations in this table are positive, only one in seven being negative. None of the negative correlations are statistically significant. The average of all the correlations in the table is 0.26, *i.e.* practically the same as that found in my own research. A general factor is the first to be extracted from this table; it contributes 30% to the variance. A second factor, accounting for 12% of the variance, divides the Ss roughly into those who prefer pure colours, and those who prefer tints and shades.

Thus it appears that Von Allesch's results are essentially identical with my own, both as regards amount of agreement between the Ss, and as regards the nature of the factors determining the judgment of the Ss. In view of the high praise that Von Allesch's work has generally received, support from this quarter is most gratifying.

As regards the alleged lack of agreement in the results of different investigators, we must first consider the fact that they were not working with the same materials. Some used coloured papers of one kind or another, others used dyed wools or velvets, coloured lights, coloured crayons, or simply colour names in their experiments. As Exner has shown, how-

³⁵ Woodworth, *op. cit.*, 382.

³⁶ Van Allesch, *op. cit.*, 13.

ever, one blue is not necessarily equivalent in pleasantness to another blue, or one red to another red;³⁷ hence one should not expect complete agreement between the various investigators. If there was no agreement at all, of course, our case would be seriously weakened, if not wholly destroyed.

There are great differences in the numbers of colours used, and in the choice of those included. Nearly all investigators, however, have included highly saturated blue, red, green, violet, orange, and yellow among their colours. Hence an average ranking was calculated for these six colours from the results reported by Katz and Breed,³⁸ Walton and Morrison,³⁹ Jastrow,⁴⁰ Cattell and Farrand,⁴¹ Walton, Guilford and Guilford,⁴² Luc-

TABLE II
AVERAGE RANKINGS OF COLOUR PREFERENCES OBTAINED IN THE VARIOUS EXPERIMENTS

Colour	White Ss (12,175)	Coloured Ss (8885)	Weighted total (21,060)
blue	1.12	1.83	1.42
red	2.32	2.03	2.20
green	3.32	2.98	3.18
violet	3.66	4.28	3.92
orange	5.30	4.76	5.07
yellow	5.28	5.12	5.21

kiesch,⁴³ Fernberger,⁴⁴ Miles,⁴⁵ St. George,⁴⁶ Washburn,⁴⁷ Farnsworth and Chichizola,⁴⁸ Dorcus,⁴⁹ Garth,⁵⁰ Hunlock,⁵¹ Parry (unpublished results) and by myself in the previous section. These investigations only deal with white Ss; a separate ranking was calculated for coloured Ss from

³⁷ F. Exner, Zur Charakteristik der schönen und hässlichen Farben, *Kön. Akad. d. Wissensch., Wien; Sitzbericht d. Mathem. Naturwiss. Klasse*, 3 Abt., 1902, 901-922.

³⁸ S. E. Katz and F. S. Breed, Color preferences of children, *J. Appl. Psychol.*, 6, 1922, 255-266.

³⁹ *Op. cit.*

⁴⁰ *Op. cit.*

⁴¹ J. McKeen Cattell and Livingston Farrand, Physical and mental measurements of Columbia University students, *Psychol. Rev.*, 3, 1896, 618-648.

⁴² *Op. cit.*

⁴³ *Op. cit.*

⁴⁴ S. W. Fernberger, Note on affective value of colors, this JOURNAL, 25, 1914, 448-449.

⁴⁵ C. Miles, Individual psychology, *ibid.*, 6, 1895, 534-558.

⁴⁶ *Op. cit.*

⁴⁷ *Op. cit.*

⁴⁸ P. R. Farnsworth and Y. L. Chichizola, Color preferences in terms of sigma units, *ibid.*, 43, 1931, 631.

⁴⁹ *Op. cit.*

⁵⁰ T. R. Garth, A color preference scale for one thousand white children, *J. Exper. Psychol.*, 7, 1924, 233-241.

⁵¹ E. B. Hunlock, Color preferences of white and negro children, *J. Comp. Psychol.*, 7, 1927, 389-404.

the results reported by Mercer,⁵² Garth,⁵³ Garth, Moses, and Anthony,⁵⁴ Garth, Iheda, and Langdon,⁵⁵ Chou and Chen,⁵⁶ Gesche,⁵⁷ Shen,⁵⁸ Imada,⁵⁹ Mizuguchi and Arki,⁶⁰ and Hunlock.⁶¹ The rankings given in each

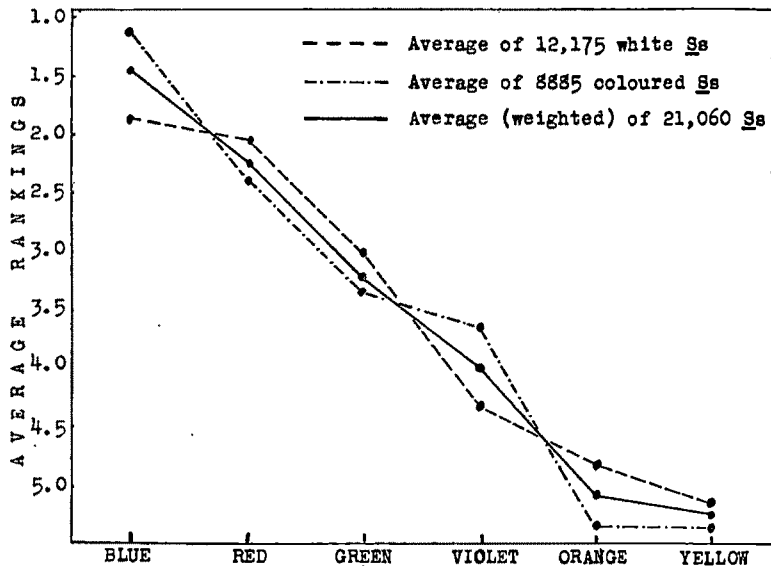


FIG. 1. AVERAGE RANKINGS OF COLOUR PREFERENCES

of these investigations were weighted by the number of Ss. The results are given in Table II.

The correlation between the average marks given to the various colours by the white and coloured Ss respectively is 0.96; hence we concluded with

⁵² F. M. Mercer, Color preferences of 1006 Negroes, *ibid.*, 5, 1925, 109-146.

⁵³ *Op. cit.*, footnote 7.

⁵⁴ T. R. Garth, H. R. Moses, and C. N. Anthony, The color preferences of East Indians, this JOURNAL, 51, 1938, 709-713.

⁵⁵ T. R. Garth, K. Iheda, and R. M. Langdon, Color preferences of Japanese children, *J. Soc. Psychol.*, 2, 1931, 397-402.

⁵⁶ S. K. Chou and H. P. Chen, General versus specific preferences of Chinese students, *ibid.*, 6, 1935, 290-314.

⁵⁷ I. Gesche, Color preferences of 1152 Mexican children, *J. Comp. Psychol.*, 7, 1927, 297-311.

⁵⁸ N. C. Shen, A note on the color preferences of Chinese students, *J. Soc. Psychol.*, 7, 1936, 68-81; The color preference of 1368 Chinese students, with special reference to the most preferred color, *ibid.*, 8, 1937, 185-204.

⁵⁹ M. Imada, Color preferences of school children, *Jap. J. Psychol.*, 1, 1926, 1-21.

⁶⁰ F. Mizuguchi and S. Arki, Color preference of adults, *ibid.*, 22-37.

⁶¹ *Op. cit.*

Garth that "there are no racial differences in colour preferences."⁶² As there is no reason to keep the two rankings apart, a weighted total ranking is calculated which is also given in Table II. Fig. 1 represents diagrammatically these three rankings; it will be noted that the line representing the ranking of the total groups is almost straight till it breaks at the orange.

(2) *Agreement among investigators.* To determine how high the agreement was between the various investigators, the rankings given by them were correlated with the total average order. These correlations give us unweighted saturations, which are identical with weighted saturations if no more than one general factor is present. As we have already seen that among saturated colours there is no secondary factor, this method gives

TABLE III
AVERAGE RANKINGS OF COLOUR PREFERENCES OF MEN AND WOMEN

Colour	7,378 men	6,247 women
blue	1.45	1.68
red	2.47	2.50
green	2.53	2.52
violet	4.36	4.14
orange	4.94	5.13
yellow	5.5	5.03

us the saturations of the various rankings in a far less laborious way than by means of a formal factor-analysis.

The average saturation of the rankings produced by the white Ss is 0.82; the average saturations of the rankings produced by the coloured Ss is 0.72. The agreement between the various investigators, then, is very high indeed, and we cannot accept Chandler's dictum that there is a complete lack of agreement.

(3) *Sex differences.* We turn next to the question of sex differences. Using a similar method to that reported above, 17 investigations giving average orders for men, and 16 investigations giving average orders for women were examined, and total averages calculated for the orders of the two sexes separately, suitably weighted by the number of Ss. Two reports were included which have not been mentioned yet; namely, those by Geissler⁶³ and Hirohashi.⁶⁴ The results are given above, in Table III.

The correlation between these two orders is 0.95; as in our own investi-

⁶² Garth, *Race Psychology*, 1931, 135.

⁶³ L. R. Geissler, The affective tone of color-combinations, *Studies in Psychology; Titchener Commemorative Volume*, 1917, 150-174.

⁶⁴ B. Hirohashi, Some experiments in beauty of color, *Jap. J. Psychol.*, 1, 1926, 406-432.

gation, the only reversal is in the case of orange and yellow, orange being preferred to yellow by the men, yellow to orange by the women. Otherwise, preferences are very similar between the sexes.

The results reached so far enable us to suggest, at least tentatively, a possible basis for the universal scale of preferences we have encountered. Two such bases, in fact, offer themselves. First, it is found that preference for any colour varies inversely with the luminosity factor of that colour.⁶⁵

Secondly, there is a direct relation between liking for a colour and its 'differentiation' from white, as shown by the minimum amount of spectral colour that must be added to the test field before it is seen to differ from white.⁶⁶ It must be left to further research to show whether this agreement between preferences and the two factors mentioned is really indicative of a causal connection. The agreement seems rather striking for it to be due merely to accident.

SUMMARY AND CONCLUSIONS

Three questions were investigated, all connected with preferences for simple colours. The results of the experiments agreed with a critical re-assessment of the results reported by other investigators. These results were:

(1) There is a certain amount of agreement between the colour preferences of people. This agreement is as high as that between intelligence tests; it is not restricted to Europeans, but also found among coloured races; and it is connected with a general factor of aesthetic appreciation discussed elsewhere.

(2) Subsidiary to this general factor of preference for colours is a bipolar factor, which divides those who prefer saturated colours from those who prefer unsaturated colours, *i.e.* tints and shades.

(3) There is high agreement between the two sexes with regard to their colour preferences. Apart from a slight preference for orange among the men and for yellow among the women, the average orders given by the two sexes are identical, the correlation between them being 0.95.

⁶⁵ The brightness at any wave-length relative to that at 5550 A.U. along the equal energy spectrum is known as the luminosity factor, Cf. Murray and Spencer, *op. cit.*, 89.

⁶⁶ *Ibid.*, 91.

ANCHORING EFFECTS IN JUDGMENT

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As psychophysics developed, there emerged certain phenomena, in connection with the relative and absolute judgments, which indicated that the actual judgment is determined by central, organizational processes as well as peripheral, sensory ones. Psychophysics thus became a technique for studying the 'higher mental processes' as well as sensation.

That there are principles of judgment, which are central in nature and not primarily regulated by sensory process, is demonstrated further by their appearance in other fields than psychophysics. After Volkman had shown the presence of anchoring effects with judgments of the inclination of lines,¹ Hunt and Volkman proved these effects also held for judgments of the pleasantness and unpleasantness of colors.² Similarly, Volkman, Hunt, and McGourty³ have duplicated with lifted weights certain phenomena first observed in affective judgments by Hunt and Flannery.⁴

This development led Hunt and Volkman to speak of "general principles of judgment" and to suggest a "process" of judgment.⁵ There is no attempt here to set up a 'faculty.' The fact is that common principles exist in all fields of judgment. We may thus speak merely of *judgment*, rather than of the psychophysical judgment, the affective judgment, the esthetic judgment, etc. It is to these common principles and their attendant phenomena that the word judgment here refers. This belief in a common, centrally located process is reinforced by such other phenomena as affective equilibrium, the 'round number' tendency, the relation of reaction-time to certainty of judgment, and the 'halo' effect on rating scales.

The purpose of the present investigation was to obtain further proof of the generality of these principles by extending the study of anchoring effects beyond the psychophysical and affective judgments to judgments of esthetic value, of ethical value, and of people's intelligence from their photographs. It also was hoped that more might be learned about the

* Accepted for publication October 15, 1940. This study was facilitated in part by a grant from the American Association for the Advancement of Science.

¹ J. Volkman, The anchoring of absolute scales, *Psychol. Bull.*, 33, 1936, 742.

² W. A. Hunt and J. Volkman, The anchoring of an affective scale, this JOURNAL, 49, 1937, 88-92.

³ Volkman, Hunt, and M. McGourty, Variability of judgment as a function of stimulus-density, this JOURNAL, 53, 1940, 277-284.

⁴ Hunt and J. Flannery, Variability in the affective judgment, this JOURNAL, 51, 1938, 507-513.

⁵ Hunt and Volkman, *op. cit.*, 92.

anchoring effect itself and about certain other phenomena which enter and complicate the picture. The anchoring effect appears when an *O* is presented with a group of stimuli and asked to judge this group in terms of an absolute scale consisting of a definite number of steps. The absolute scale tends to be determined by the stimulus-range of the group, since *O* adjusts his scale to the range of stimuli present. In this sense, the judgments are relative rather than absolute. If, however, we anchor one end of the scale upon a stimulus-value well outside the range of the original group, *O* expands his scale to include this value and his original judgments are displaced toward the end of the scale away from the anchoring value. Thus we may ask *O* to judge the affective value of a moderately toned group of stimuli, using a scale of 11 numbers in which the low numbers represent low affective value and the high numbers high affective value. Subsequently we may anchor step 11 by telling *O* that "eleven means a pleasantness equal to that of the most pleasant color you have ever seen." If he now rejudges the stimuli, his scale will have extended to include this anchoring value and his judgments of the original stimuli will be displaced toward the lower end of the scale, *i.e.* these stimuli will be judged less pleasant if considered with reference to "the most pleasant color you have ever seen."

In the seven experiments to be reported here, the same general technique was followed. *O* was presented with a series of stimuli and asked to rate them in terms of an 11-point scale. The low numbers of the scale always indicated low values of the attribute in question, the high numbers high values. At a later time, one extreme of the scale (either step 1 or step 11) was anchored by instruction to a value presumably outside the range of stimuli by telling *O* to let the number represent the least or greatest amount that he could "think of" for the attribute in question. *O* was then asked to make another series of judgments under these new instructions and the displacements in judgment were noted. The displacement was always figured in terms of the average value of the stimulus-series. At the conclusion of each experiment protocols were gathered, and the *O*s were asked to tell "what went on in your mind, how you made your judgments." The temporal intervals between the unanchored and anchored series varied from 15 min. to 48 hr. This factor was found to be of no importance.

Thirteen *O*s were used in Experiment 1. They were asked to judge the affective value of 12 Milton Bradley colors. Each stimulus-color was presented five times at each of the experimental sessions (before and after anchoring) and the affective value of any color was assumed to be the average of the five judgments obtained for it. Anchoring was at the low extreme of the scale in terms of "the most unpleasant color you can think of."

Experiment 2 used 13 *O*s, each of whom was asked to judge the esthetic value of 12 reproductions of ivory carvings. No definition of *esthetic* was given. Again, five presentations of each stimulus-object were given and the esthetic

value was assumed to be the average of the five judgments rendered. Anchoring was at the low end of the scale as the "least esthetic carving you can think of."

In Experiment 3, 14 *Os* were shown 12 photographs of children and asked to judge how intelligent the children looked. The pictures were presented simultaneously and only one judgment per session was requested. Anchoring was at the low end of the scale with the words, "the least intelligent looking child you have ever seen."

Experiment 4 repeated the technique of Experiment 3 with another group of 10 *Os*.

Experiment 5 dealt with esthetic value again and used eight colored reproductions of modern paintings. There were 10 *Os*. All the stimulus-objects were presented simultaneously and only one rating was taken. Anchoring was at the high end of the scale in terms of "the most esthetic picture you have ever seen."

Eleven *Os* were used in Experiment 6. They were asked to rate a group of eight crimes for the enormity of the breach of ethics involved. The crimes ranged from "putting a slug in a slot machine" to "robbing a filling station." The situations were presented simultaneously and only one judgment was taken. This time (a) anchoring was at the high end of the scale, but in terms of a specific concrete example. Point 11 was said to represent the amount of ethical offense involved in "murdering your own mother, wilfully and without provocation or justification." At a third session (b) the same *Os* were asked to rate these crimes again, this time with anchoring at the low extreme in terms of "cheating while playing solitaire by yourself."

In Experiment 7, 36 *Os* were used. Ethical judgments were taken under the same conditions used in Experiment 6, except for the anchoring values. Anchoring was at the high end of the scale, but at first (a) it was done in general terms, 11 representing "the worst crime you can think of." At a second session (b) another anchoring value was given at the same end of the scale, but this time it was made definite and specific by letting "11" represent "murdering your mother, wilfully and without provocation or justification." This difference enabled us to see whether or not any further displacement would appear when the anchoring value was made concrete and specific.

The results are shown in Table I. This table contains the differences in average judgment after anchoring (since they are all in the direction demanded by the anchoring effect, no attention has been paid to their sign), the probable error of these differences, the differences between the average deviations of the series of judgments before and after anchoring (these are always in the direction of a smaller average deviation), and the probable errors of these latter differences.

In every case the anchoring effect is established. The demonstration of this originally psychophysical phenomenon in affectivity, esthetics, ethics, and the complicated judgments involved in rating a person's intelligence from his photograph shows its universality. Most certainly there are universal principles of judgment operating, no matter what the field may be.

The extension of these findings to the fields of ethics and esthetics thus offers further material for an experimental approach to 'value.' We should mention that, as noted before,⁶ the changes in judgment are largely unconscious, and *O* is frequently surprised to find that such changes have taken place.

In every experiment but one, where there is no change, the average deviation decreases after anchoring. We should expect this change, since anchoring results in an extension of the scale with the stimuli being crowded into a smaller portion of the scale. Thus the displacement in the average judgment also implies a smaller average deviation, as less of the total range of the scale is now being allotted to the stimulus-series.

While all the shifts in judgment in these experiments are in the direction demanded by the anchoring hypothesis, not all of the individual

TABLE I
CHANGES IN AVERAGE JUDGMENT AFTER ANCHORING
(All differences are in the expected direction)

Experiment	Type of judgment	Average judgments		Average deviations	
		Diff.	P.E.	Diff.	P.E.
1	Affective	.50	.15	.30	.09
2	Esthetic (carvings)	.20	.14	.10	.08
3	Intelligence	.40	.10	.30	.08
4	Intelligence	.40	.29	.40	.20
5	Esthetic (pictures)	.60	.57	.0	.18
6a	Ethical	2.80	.29	.60	.14
6b	Ethical	1.50	.30	.10	.13
7a	Ethical	2.50	.23	.20	.10
7b	Ethical	1.50	.28	.40	.10

results are statistically reliable. It is obvious that something besides the anchoring phenomenon is appearing. In general, one might say that the further one moves away from psychophysics with its relatively simple 'sensory' judgments and approaches the complex judgments of value, as in esthetics, the less clear does the anchoring effect become. Fortunately, a comparison of the results with the protocols of individual *O*s shows us why the anchoring effect is less definite, and reveals several subsidiary principles that may operate in the anchoring situation. We are thus enabled to expand our understanding of judgment.

The first point upon which the protocols bear is the necessity of anchoring outside the range of the stimulus-series. Hunt and Volkmann found that, in general, the amount of shift is a function of the stimulus-distance

⁶ Hunt and Volkmann, *op. cit.*, 91.

between the anchoring value and the stimulus-series.⁷ In other words, anchoring acts by extending a previously relative scale, largely determined by the actual range of stimuli present, to approximate an absolute one, determined by all the conceivable potential stimuli. The greater the discrepancy between the 'relative' scale before anchoring and the 'absolute' scale afterward, the greater will be the shifts in judgment.

In using materials of this sort, whose values are determined subjectively, cannot be measured in terms of objective units, and vary from individual to individual, there is extreme difficulty in controlling the stimulus-range. It sometimes happens that the range of stimuli is so wide for *O* that it is impossible to find an anchoring value outside the stimulus-series being used, *i.e.* *O* is unable to think of a color that is more unpleasant than one of those being presented to him. This difficulty was particularly prevalent in the esthetic experiments, and it is noticeable that the differences here, while in the expected direction, are not statistically reliable. The clarity of the results for ethical judgments is due to the comparative social agreement in the field which made it possible to select a limited group of stimuli and an anchoring value well outside the stimulus range.

Surprisingly enough, however, a slight displacement resembling the anchoring effect sometimes appears even when the anchoring value is not outside the range of the stimulus-series. Thus we must amend the suggestion of Hunt and Volkman that failure to anchor outside the stimulus-range destroys the shift in judgment and admit that even under such circumstances some minor shift in values may appear. Since the scale as a whole cannot be shifted, this change could not be the genuine anchoring effect but must be some more specific phenomenon, perhaps attendant upon the further definition (through anchoring upon a definite color) of one extreme of the scale. The shift is not one involving the entire scale, but rather a shift in some of the scale-values when one extreme becomes closely identified with a member of the actual stimulus series.

Another source of confusion also appeared in the judgments of esthetic value and intelligence. It is best illustrated by one of our *O*s who showed large shifts in the wrong direction. She was a relatively untutored person, who found the judgments particularly hard to make. The first series of judgments of intelligence and esthetic value she therefore made blindly and without any feeling of certainty. The addition of anchoring values had the effect of offering her a concrete definition of the aspect to be judged. Es-

⁷ Hunt and Volkman, *op. cit.*, 91.

thetic value meant nothing to her by itself, but by keeping in mind the best picture she had seen she managed to achieve an effective definition of esthetic value by referring constantly to the properties of the picture of which she was thinking. Her first series of judgments were without any real understanding, which came later when the anchoring values were added. There is, therefore, no similarity between judgments in the unanchored and anchored judgments. It is thus possible to point out another phenomenon which may occur in the anchoring situation; if *O* is not clear about the attribute which he is judging, the addition of an anchoring value may serve to define this attribute more clearly and thus change the basis of judgment. When this occurs, the usual anchoring shift will be absent.

Another variation which may occur appears when *O*, after being given an anchoring value at one end of the scale, spontaneously and on his own initiative adds one at the opposite end of the scale. This act results in an extension of the scale at both ends with a resulting compression of the judgments toward the middle of the scale. There is a double anchoring effect. As a result there is little or no shift of the central tendency, but a decrease in the average deviation. While these cases do not show positive results in any method of scoring based on a shift in central tendency, individual inspection of the data shows clearly that they support the general principle of anchoring. Such an occurrence is clearly seen with one *O* who showed with double anchoring no shift in central tendency but a clear drop in the average deviation.

When a double anchoring effect of this sort is produced by the spontaneous addition of a second anchoring value at the unanchored end of the scale, the usual effect is an extension at both ends of the scale *as a whole*, with a compression of the judgments toward the mean. The two anchoring values combine in a unified effect. One *O*, however, seemed to make his judgments in relation to the two anchoring values as independent reference points. The higher judgments were driven down, even below the mean; and the lower judgments were driven up, some of them above the mean. The two ends of the stimulus-series are thus driven past one another in an overlapping fashion. This *O* seemed to be using two scales, one anchored at the low extreme of the scale for those stimuli with low values, and one anchored at the high extreme for those stimuli with high values.

Usually, when the anchoring shift takes place, the stimulus-series moves as a whole. Occasionally, however, individual stimuli seem to be displaced within the stimulus-series, and either to move a relatively greater distance

than the average shift of the whole series or to move in a direction opposite to the direction of the entire group. Examination of the protocols shows that this movement takes place when the individual stimulus either resembles or contrasts with the anchoring value in very marked fashion. This relationship was particularly obvious for the judgments of intelligence. Keeping in mind the least intelligent child he had ever seen would raise *O*'s judgments of most of the pictures, until he suddenly noticed that one of the children in some way resembled the unintelligent child he had in mind. Then this child would be judged as much less intelligent than previously. Or this *O* might notice that one child looked exactly opposite in some respect to the one he had in mind. As a result, the child would be given a relatively greater advantage in intelligence than the other children. We may call these effects *assimilation* and *contrast*. When they occur the stimulus seems to lose its character as one of a series, and be judged individually in direct and immediate relation to the anchoring value. This effect does not happen often, and the series usually moves as a whole with little dislocation of the relative position of each stimulus.

In Experiment 7 the displacement, produced by anchoring with the general instructions to let step 11 represent "the worst crime you can think of," is increased if the anchoring is changed to the concrete example "murdering your mother, wilfully and without provocation or justification." Such a concrete anchoring value gives a 'better' effect. It seems to be due to the fact that it is possible to get closer to the true extreme of experience with a concrete example than it is with such general instructions as "the worst you can think of," "the best you have ever experienced." In terms of relative and absolute scales, it is possible to approach more nearly the true absolute scale by anchoring with a concrete, specific illustration. The phenomenon occurs as though the total range of experience or memory available to *O* tended to shrink and become somewhat less until it is stretched back toward the absolute range by some definite example. Some of the effect may be due to the fact that the clarity of a concrete illustration renders the judgment easier. The experiments yielded preliminary data on judgmental reaction times which indicate that this principle may hold.

That the relative superiority of concrete over general anchoring values is important in many psychological techniques may be demonstrated by reference to a recent experiment in audition by Stevens and Volkman.⁸ They applied the psychophysical technique of fractionation in plotting the pitch-frequency function. Their *O*s were given a tone and told to manipu-

⁸ S. S. Stevens and J. Volkman, The relation of pitch to frequency: a revised scale, this JOURNAL, 53, 1940, 329-353.



late a variable oscillator until they had selected another tone which sounded half as high in pitch as the first. There was some distortion in the results due to the fact that the *Os* had difficulty in conceiving of "zero" pitch as a reference point for the fractionation. As Stevens and Volkman say, "without an objective reference, the *Os* tend to assume a zero pitch that is too high."⁹ This conclusion seems to be directly comparable to the present findings in Experiment 7, and one could have predicted that Stevens and Volkman would have found it necessary to furnish their *Os* with a reference pitch if they were to use a truly absolute scale in making their judgments.

To show the universality of these phenomena of judgment, Stevens and Volkman's situation can be transposed to the field of ethical judgments and the tonal experience duplicated there. Fourteen *Os* were told to consider the crime of murdering one's mother, wilfully and without provocation or justification. They were then told to think of a crime "one-half as bad" and to write it down. Then they were given the same crime of murdering one's mother, but this time a 'zero' reference point was added with "cheating at solitaire while playing by yourself;" and they were asked to think of a crime "half way between" the two. In Stevens and Volkman's terms the crime selected under the first condition should be "higher," in our terms "more severe." Since there is no objective scale for reference, the crimes had to be rated for severity by the *Os*. In 12 cases out of the 14, the first case was the more severe as was predicted.

The importance of these principles of judgment is self-evident. Not only must they be taken account of in predicting every-day human behavior involving acts of judgment, but they must be watched for in such techniques of experiment and testing as proceed with the assignment of values according to some arbitrary scale. One might even suggest that they must be considered in any philosophical treatment of value theory, as they raise interesting questions concerning the locus and absolute nature of value itself.

SUMMARY

If judgments made with an unanchored scale be repeated with the scale anchored by the further definition of one extreme, there is a shift in the average value of the stimulus-judgments, and this shift is in a direction away from the anchoring value. Thus, if a scale is anchored at its low extreme, the judgments tend to rise, and vice versa. The effect seems to be stronger if some concrete illustration is used as an anchoring point.

By demonstrating the presence of this effect in such diverse fields as the effective,

⁹ Stevens and Volkman, *op. cit.*, 340.

esthetic, and ethical judgments as well as in judgments of how intelligent children look, the present investigation offers further proof that there are common principles governing acts of judgment, and that these 'laws' of judgment must be considered in experimental procedures involving the use of scales, as well as in any theoretical consideration of value judgments.

While the displacement mentioned above is the usual result of anchoring one extreme of a scale, the following phenomena have also been observed:

(a) Unless the anchoring value lies beyond the range of stimuli used, there is in general no displacement; but in some cases displacement may appear even though the anchoring value is not outside the range of the stimulus series.

(b) When *O* is unfamiliar with the aspect being judged, the addition of an anchoring value may act to define the aspect in question, and may result in a complete change in the basis of judgment with an attendant change in the actual judgments given.

(c) Occasionally, the addition of an anchoring value at one extreme of the scale results in *O*'s spontaneously adding one of his own at the opposite extreme. Such an addition results in a compression from both ends of the scale, with very little, if any, shift in central tendency.

(d) Assimilation and contrast may enter where specific stimuli are brought into individual meaningful relationship with the anchoring value.

THE SHORT-CIRCUIT PHENOMENON OF PHI-MOVEMENT

By C. O. WEBER, Wells College

An interesting puzzle (the Yogi puzzle), described by K. M. Dallenbach, may be regarded as a variant of phi-movement.¹ In the ordinary form of phi-movement there are two-stimulus-objects separated by an appropriate space interval and appearing in rapid succession. In the case of the Yogi puzzle we have one stimulus-object (the sword) moving over a circular trajectory (see Fig. 1). The sword cannot be seen during movement which is very rapid. The initial and final resting states of the sword serve as the two stimuli which generate phi-movement. When the sword moves in clockwise direction it covers some 340° , thus bringing it within

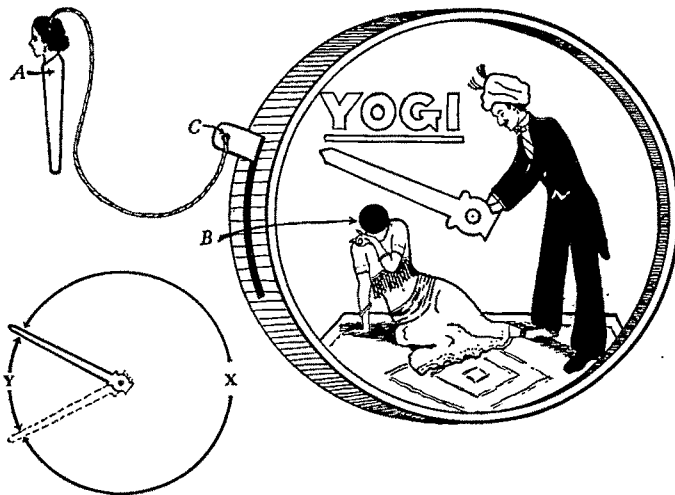


FIG. 1. THE "YOGI PUZZLE"

20° to the left of the starting point. The observer appears to see the sword moving in counter-clockwise direction over the shorter trajectory of 20° . Hence the term "short-circuit phi" seems a satisfactory designation for this form of apparent movement.

The principle that phi-movement tends to occur over the shorter of two paths was demonstrated in the classic experiments of Wertheimer.² If lines ac and ae (Fig. 2, part A) are successively exposed, ac moves toward ae . If the angle between these two lines is gradually increased the same direction of movement con-

* Accepted for publication January 1, 1941.

¹K. M. Dallenbach, The "Yogi puzzle" and the "endless spiral": Demonstrational devices of apparent movement, this JOURNAL, 48, 1936, 509-511.

²M. Wertheimer, Experimentelle Studien Über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61; 1912, 161-265.

tinues until a critical position for ac is reached (Fig. 2, part B) when it moves over the shorter distance to the left.

Phi-movement is frequently identified with stroboscopic movement.³ Analysis will show that stroboscopic movement involves short-circuit phi rather than phi-movement proper.⁴ Assume that the wheel in Fig. 3 is moving in clockwise direction and that it is being photographed by a motion-picture camera. The first exposure of the camera gives the initial positions of lines a and e as respectively vertical and horizontal. Suppose that the speed of the shutter is such that the time-interval before the next exposure allows a to move to position e the wheel appears at rest. If the speed of the shutter is somewhat faster than the rate required for stroboscopic rest, line a is seen (let us say) at a' and e at e' ; and the wheel now appears to move in counter-clockwise direction, as determined by the shortest distance, i.e. e to a' . If

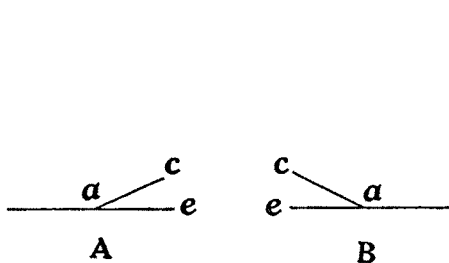


FIG. 2. MOVEMENT OF LINES

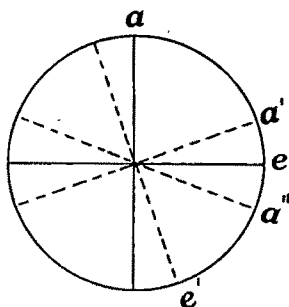


FIG. 3. MOVEMENT OF A WHEEL

the shutter is somewhat slower than is required for stroboscopic rest, line a moves to a'' , and clockwise motion is now set up between e and a'' . In all this, the initial positions of a and e serve as reference points which determine the direction of the resultant apparent movement. If the speed of the shutter is such that the lines a and e (in their next appearance) will bisect the angles between them, stroboscopic rest appears once more, for there is now no occasion for short-circuit phi since the distances back to a or forward to e are equal. Mibai observed all of these essential facts in the study of stroboscopic movement.⁵ This investigator notes that when the spokes of a rotating wheel appear at about the middle of the spaces between neighboring spokes (in successive exposures) the wheel appears to fluctuate in the direction of its motion.

A large model of the Yogi puzzle was constructed in the Wells Laboratory. It consists of a rectangular box showing on one side a steel hand secured at right angles to a shaft which is made to rotate by means of a spring inside the box. The hand always moves in clockwise direction over a circular dial. The angular excursions may be varied from 50° to 330° of arc, and the speed of movement may be

³ Thus, K. Koffka speaks of Wertheimer's studies of stroboscopic movement. *Principles of Gestalt Psychology*, 1935, 281.

⁴ I am indebted to Dr. Martin Scheerer for calling my attention to this identity.

⁵ Sugi Mibai, An experimental study of apparent movement, *Psychol. Monog.*, 42, 1931, (no. 190), 14.

changed by varying the tension of the spring. Hands of varying lengths and widths may also be used. The fields over which the hand moves can be varied by slipping different circular cardboard dials into place. The device is thus suitable for a systematic study of many of the factors which determine this form of apparent movement.

One might suppose that the illusion of the Yogi puzzle has its origin in the meaningful portrait of the Yogi, his sword, and the head of the victim. That this is not the case is evident from the fact that the illusion is very compelling when a plain white field is used with our device. Many *O*s even after an hour of continuous observation are not aware that the counter-clockwise movement of the hand is always illusory.

Random vs. serial order. The extent of the excursion of the hand can be varied by 15 steps. The shortest excursion is 50° and the longest is 330° , the successive differences in extent being 20° . In our first experiment 6 *O*s made six series of judgments, each series consisting of the 15 possible angular extents given in random order. The hand used was 5.5 cm. in length and 2 mm. in diam. The hand was painted black and moved over a dial of white cardboard. The *O*s merely recorded *R* (right) when the hand appeared to move clockwise, and *L* (left) when it ap-

TABLE I
EFFECTS OF PRACTICE ON SHORT-CIRCUIT PHI

Series	Apparent	Real	Series	Apparent	Real
1	130.0°	230.0°	4	115.0°	245.0°
2	120.0°	240.0°	5	106.6°	253.3°
3	116.6°	243.3°	6	101.6°	258.3°

peared to move counter-clockwise, and *D* (doubtful) when they were uncertain regarding the direction of movement. The illusion was not explained to them, but a few were familiar with it. Familiarity, however, did not destroy the illusion. These instructions for the *O*s were given for all of the experiments to be described. The results of the first experiment are given in Table I.

To understand the Table I and other reports to follow, it must be remembered that the hand always moves in clockwise direction. If the excursion is only 50° , all of the *O*s see real movement. If the excursion is 330° , all of the *O*s see short-circuit phi; *i.e.* see the hand moving in counter-clockwise direction. In preparing Table I the mid-point was found for each series of each *O* where real movement changes to apparent movement, a process which often required interpolation. The table therefore shows no area where the *O*s were doubtful, although doubtful judgments were often given. In the Series 1 of Table I, for instance, the 6 *O*s found real movement dominant on the average for an excursion of the hand up to 230° . Short-circuit phi appears on the average when the counter-clockwise distance (between the terminus of the movement to the starting point) is 130° . Table I shows that practice weakens the illusion, since it decreases from 130° in Series 1 to 101.6° in Series 6. In this respect, this illusion resembles familiar illusions of form (Müller-Lyer and others).

On the whole, real movement is perceptually dominant over apparent movement, since the average maximal extents (all series) for which apparent movement is seen is 115° , while real movement is seen to the average maximal extent of 245° .

As the velocity of the hand increases (as will be shown later) apparent movement improves, *i.e.* is seen over a larger trajectory; but real movement still exceeds apparent movement even with the greatest velocity of the hand of which our device is capable. This greater strength of real movement, however, may be more apparent than real. Perhaps the *Os* are mentally set to see clockwise motion: unfortunately our device gives real movement only in clockwise direction. If the speed of the moving hand is too rapid to be seen, then the report of clockwise movement, the direction in which the hand really goes, must also be a report on apparent movement.

When the 15 excursions are given in *serial* order both real and apparent movement are stronger than when the order is *random* as in our first experiment. Nine *Os* were given alternate series, one beginning with real movement dominant (*i.e.* with extents of 50°, 70°, 90°, etc.), the next series with apparent movement dominant (*i.e.* beginning with an extent of 330° and proceeding back to 50°). Under these conditions apparent movement is seen over an average trajectory of 154.5° as compared with only 120.4° for random order. Real movement is seen to the average extent of 255.1° as compared to equivalent random series giving an average of 239.6°. The serial order of presentation strengthens the movements in the way described because it generates the mental set of expectation. The fact of such strengthening, observed by Wertheimer and also by Koffka, is explained by Koffka as due to the influences of the 'traces' left by successive observations.⁶

There are a very few *Os* who never get the illusion and who always or nearly always report clockwise movement. For those who do get the illusion, susceptibility shows striking variations, reminding one of the large individual variations encountered in visual constancy phenomena. In the group of 9 *Os*, one saw apparent movement to the average extent of only 60° while the most susceptible *O* saw apparent movement over a trajectory of 170.0°.

Velocity. Using 5 *Os*, judgments were made in four series of observations for each of three velocities of the hand. There were 15 excursions in each series, all beginning at the terminus 330.0° where apparent movement is dominant, and proceeding serially to the short excursion of 50.0° where all *Os* see real movement. The three velocities of the hand were secured by stretching the powerful spring three extents, namely: 5, 9, and 13 mm. Series for the three velocities were taken in chance order.

Since our study was made in the interests of qualitative analysis, the actual velocities of the hand corresponding to the three spring tensions were not determined. Such measurements have been made by other investigators of phi-movement. Wertheimer had found that phi-movement shows its first beginning when the time-interval between the exposure of two lines is cut down to less than 0.2 sec. The movement becomes quite clear when the interval is 0.06 sec., and vanishes again for intervals of 0.03 sec. or less.⁷ The time-interval required for optimal movement depends of course on various factors, such as the intensities of the stimuli and the space intervals between them, as shown by the well-known investigations and laws of Corté.⁸

⁶ K. Koffka, *op. cit.*, 510 ff.

⁷ M. Wertheimer, *op. cit.*, 161-265.

⁸ Some of the significant values obtained by Corté and others are given by Koffka, *op. cit.*, 288-304.

The results of our studies show that short-circuit movement gets stronger as the velocity of the hand increases. For the slowest movement apparent movement is seen to the average extent of 76.9° , for medium velocity there is an increase to 130.0° , while for the greatest velocity the average extent of apparent movement is 137.0° . This relationship between speed and the strength of apparent movement was established for only 5 Os, but each O's results verified the principle.

This result appears to contradict the law established by Corte that in order to retain optimal movement the time-interval between stimuli must be increased as the spatial separation of the stimuli increases. In our results, on the contrary, we find short-circuit movement covering greater distances as the time-intervals between stimuli become shorter, *i.e.* as the velocity of the movement increases. That there is no real contradiction, however, is shown by two considerations regarding short-circuit movement. Such movement (*a*) becomes more forceful the shorter the gap it has to cross, and (*b*) more forceful to the extent that real movement is blotted out. Naturally, then, as the velocity of the hand increases, the real component of the movement becomes less visible. In other words, short-circuit ϕ becomes stronger (can cover greater distances) as speed of the hand increases because this increasing speed blots out its antagonist, the perception of real movement.

Length of hand. Two lengths of hand were used: 5.5 cm. and 9.5 cm. The width of the hands were 2 mm. in each case. Nine Os completed 8 series of determinations for each length, alternating the series between long and short. Each series started with the excursion where all the Os saw apparent movement, and progressed step-wise to the point where all Os saw real movement. Medium spring tension (9 mm.) was used, and the movement took place over a uniform white field.

The results show that apparent movement is stronger for the shorter hand. This result should be expected in view of the fact that the shorter the hand the less visible its real movements become, thus facilitating the perception of apparent movement. For the shorter hand apparent movement was seen over an average trajectory of 96.2° ; for the longer hand the corresponding average was 83.2° . This result, however, did not hold for 3 Os, two of them giving equal values for the two hands, and one giving stronger illusory effects for the longer hand. In this experiment, as in the first, apparent movement becomes progressively weaker as the series proceed from the first to the eighth.

Arrows in the field. In all experiments previously described the black hand moved over a dial of plain white cardboard. A final experiment was carried out to determine the effects of arrows in the field. Ten Os were used, each completing 16 series of judgment, each series consisting of 15 trials varying serially in angular extents. The 16 series divide into 4 series each for 4 different conditions of observation; namely, (*a*) beginning with apparent movement over a plain field, (*b*) beginning with real movement over a plain field, (*c*) beginning with apparent movement over a field containing arrows pointing in counter-clockwise direction thus reënforcing illusory movement, and (*d*) beginning with real movement over a field containing arrows pointing in clockwise direction and thus reënforcing real movement. Arrow 'inhibition' was not studied, *i.e.* no series were made in which apparent or real movement was against the directions indicated by the arrows. The four types of series just described were given in a prearranged order designed to distribute practice and fatigue effects. The results are given in Table II.

It appears from these results that apparent short-circuit phi is not affected by arrow reënforcement, since the difference in the two fields is negligible. The arrows do, however, strengthen real movement, increasing the average trajectory from 255.1° to 268.4° .

The arrows in all cases were *within* the area traversed by the hand, and consisted of 21 arrows on each dial drawn in three concentric circles with 7 arrows in each circle. The arrows were drawn in heavy lines with black ink. The average failure of the arrows to reënforce apparent movement may be due to the disrupting effect on perception with results when any stimuli appear in a field where apparent movement is seen. The moving hand was also black, and the perception of the real arrows in the field may disrupt cortical processes which would otherwise lead to the perception of apparent movement. To test this matter, observations were

TABLE II
REËNFORCEMENT EFFECTS OF ARROWS

Movement	Plain field	Arrow reënforcement
Apparent	154.5°	154.4°
Real	255.1°	268.4°

made on the classical form of phi-movement, using a modified Dodge tachistoscope. In this apparatus, two small black circles appear alternately in a white field. The circles are separated by 4 cm., and give rise to the clear but illusory perception of one ball moving back and forth in horizontal direction. If other lines or figures are drawn in the field traversed by apparent movement they have decided disrupting effects on apparent movement.

Individual differences were marked in these studies of arrow reënforcement. Three of the 10 Os, in fact, did show stronger apparent movement with arrows in the field, but the results from the remaining 7 nullified these results on the average. Again, 4 of the 10 Os actually gave real movement over a smaller trajectory when arrow reënforcement was used; but the 6 remaining were so susceptible to arrow reënforcement that they account for the difference between the plain field and the field with arrows in the case of real movement.

SOUR THRESHOLDS AS A FUNCTION OF THE pH OF HYDROCHLORIC AND SULPHURIC ACIDS

By R. B. MALMO, Yale University, and M. M. ELLIS, University of Missouri

It has long been known that the external hydrogen ion concentration is not the only significant variable in the discrimination of the taste of sour. In order to determine what these other variables might be, we obtained and compared the psychometric functions of 6 *O*s for the sour taste of two acids—hydrochloric and sulphuric. Six different pH values of each acid were used as stimuli. Any significant differences in the psychometric functions thus obtained must be attributable to variables other than hydrogen ion concentration, since this factor was constant for the two acids. An incidental object of this study was to test further the adequacy of the "method of absolute judgments" in the field of taste. Pfaffman has shown that the absolute method is as adequate as the constant method for determining the sensitivity of subjects to di-sodium hydrogen phosphate.¹ Although we did not compare the two methods directly, the fact that we obtained significant differences between the psychometric functions for the sulphuric and hydrochloric acids with the method of absolute judgments demonstrates the applicability of the method to the study of taste. The greater convenience of the absolute method recommends it.

Procedure. The *O*s were blindfolded and seated three at a table. One *E* at each table presented the solutions to the *O*s in marked 250-c.c. beakers. Every *O*, in accordance with standardized instructions, took enough of the solution to cover fully his tongue, held it in his mouth for 10 sec., expectorated, reported a number-value from 1 to 6 designating the sourness of the solution tasted (1 = lowest intensity of sour; 2 = very mildly sour; 3 = mildly sour; 4 = sour; 5 = very sour; and 6 = most intensely sour), and then rinsed his mouth with distilled water. A second *E* controlled the stimulation-time with a stopwatch.

The interval between stimulations was 50 sec. One judgment of sourness was thus given every minute. At the beginning of every experimental period, a preliminary series was given every *O* to acquaint him with the intensity-range. The *O*s, however, were instructed to begin judging with the application of the first stimulus.

Ten judgments of every stimulus-value were obtained from every *O* on each of 10 days. The first five days were devoted to experiments with sulphuric acid, the last five to experiments with hydrochloric acid. All of the experiments were begun at 5 P.M. The solutions were checked daily for pH with a Leeds-Northrup potentiometer and they were kept at room temperature (approximately 22.7°C.). The pH values used throughout the experiment were as follows: 6.9, 6.0, 5.0, 4.5, 4.0, 3.5. The results were treated by Urban's constant process, assuming only two categories

* Accepted for publication October 15, 1941. This experiment was performed in the Laboratory of Physiology, University of Missouri. The senior author did the psychophysical work; the junior author prepared and standardized the solutions.

¹ Carl Pfaffmann, An experimental comparison of the absolute and constant stimulus methods in gustation, *Psychol. Bull.*, 31, 1934, 622-623; W. J. Crozier, Chemoreception, in C. Murchison's *A Handbook of General Experimental Psychology*, 1934, 1011-1012.

of judgment. The *PE* of the psychometric function was used as the measure of sensitivity.

Results. Fig. 1 shows the psychometric functions for all *O*s and both acids for judgments "less sour" (1-3 on the absolute scale). The ordinate is scaled in terms of percentages of judgments less and the abscissa in terms of the six pH values of the solutions used as stimuli, beginning with pH 6.9 (almost neutral) and progressing to pH 3.5 (definitely sour). By inspection of Fig. 1 it can be seen that the psychometric functions for all 6 *O*s showed the same thing; namely, a much greater

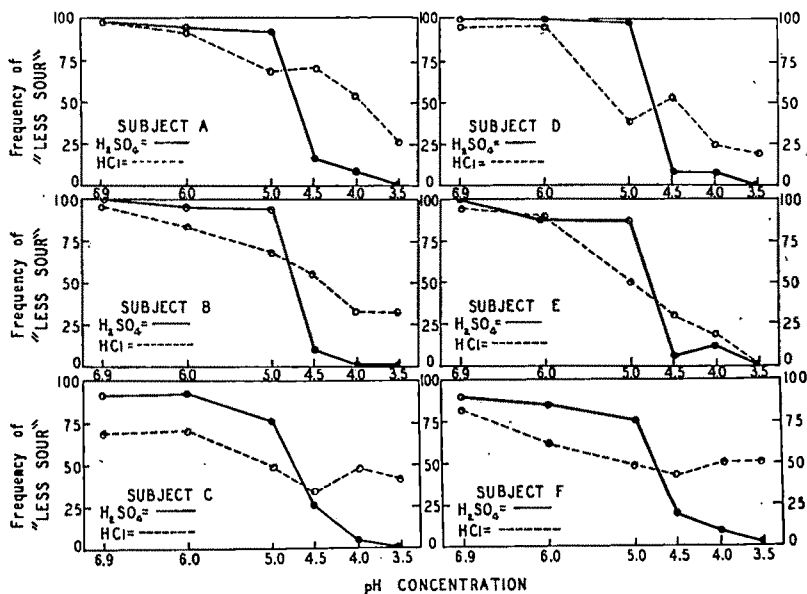


FIG. 1. PSYCHOMETRIC FUNCTIONS OF THE DISCRIMINATION OF THE TASTE OF SOUR FOR VARIOUS HYDROGEN ION CONCENTRATIONS OF HYDROCHLORIC AND SULPHURIC ACIDS

steepness for sulphuric than for hydrochloric acid. In every curve for sulphuric acid there is a sharp drop from stimulus 3 (a minus value) to stimulus 4 (a plus value). This characteristic is not present in the curves for hydrochloric acid. It is also important to note that, as a correlate of the differences in precision of the psychometric functions for sulphuric and hydrochloric acid, the curves for the former were more clearly ogival in form than those for the latter and involve fewer reversals. There were six reversals in the curves for hydrochloric acid whereas there were only two in the curves for sulphuric acid.

Table I presents for both acids (1) the limens for all the *O*s; (2) their measures of precision (*b*); and (3) the probable errors (*PE*s) of the limens, *i.e.* the measures of sensitivity. It can be seen from these values that the thresholds for the sulphuric acid are markedly constant for our *O*s whereas the thresholds for hydrochloric acid are widely divergent. It should be noted that the *b* values for the sul-

phuric acid are larger in the case of every *O* than those for hydrochloric acid. Thus the steepness of the ogive fitted to the data was considerably greater in the case of the sulphuric acid. We may conclude, therefore, that the sour judgments of sulphuric acid were more precise than were those of hydrochloric acid.

The results are conclusive in showing that differences in the discrimination of sour occur when the variable of hydrogen ion concentration is kept constant. They show, furthermore, the direction of these differences. Additional research should be undertaken to determine what factors operate to produce those differences. The factor of penetration-time is also one that might well be attacked with this method.

Since we did not control the order of presentation for the two acids, the objection might be raised that our differences were a function of practice. Fractionation

TABLE I

DISCRIMINATION OF SOURNESS OF SULPHURIC AND HYDROCHLORIC ACIDS

Limens represent hydrogen ion concentration (pH) at which frequencies of judgments were equally distributed between "more sour" and "less sour."
Subjects

		A	B	C	D	E	F
H ₂ SO ₄	Limen	4.86	4.83	4.98	4.67	4.88	5.08
	h	.97	1.33	.59	1.95	.95	.65
	P.E.	.09	.06	.14	.04	.07	.13
HCl	Limen	4.07	4.24	4.31	4.63	4.85	5.48
	h	.51	.45	.16	.47	.71	.17
	P.E.	.16	.19	.58	.18	.11	.51

of our data reveals, however, that that was not the case. Every group of six judgments, which included all the stimuli, was totalled for every *O* for every experimental day. The resulting values did not show any progressive effects; there were no significant differences among the daily averages of a given *O*, or between the averages of the first and last parts of the series for any given day.

The close agreement of the differential limens obtained by the absolute method for sulphuric acid and the uniform differences in the psychometric functions between the two acids were enough to justify the use of this method in further investigations of this nature.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY
OF CORNELL UNIVERSITY

XCI. AREAL AND TEMPORAL VARIATIONS IN PAIN SENSITIVITY

By JOHN I. LACEY, BEATRICE C. LACEY, and KARL M. DALLENBACH

Based for the most part on results secured by elementary laboratory students, this study is preliminary to controlled experimentation upon two problems: (1) areal variations in pain sensitivity under supraliminal stimulation—as a parallel to Strughold's investigation wherein sensitivity was measured by limens;¹ and (2) the effect of time-order upon the *Os*' judgments of pain-intensity.

METHOD AND PROCEDURE

Observers. The *Os* numbered 50, 29 men and 21 women. Of these, 35 were laboratory students, and 15 were volunteers from other classes in psychology.

The laboratory students worked in pairs, each member of a pair serving alternately as *O* and *E*. Every pair worked in a small room, free from disturbances. The experiments with the 15 volunteers were conducted individually by the junior authors.

Areas. Twelve areas on the left arm were stimulated, in accordance with the method of paired comparisons, with a constant-pressure needle-algesimeter. The areas were: (1) radial side of the second phalanx of the middle finger; (2) dorsal region between the thumb and the index finger; (3) dorsal wrist; (4) ventral wrist; (5) lower dorsal forearm; (6) upper dorsal forearm; (7) lower ventral forearm; (8) upper ventral forearm; (9) inside elbow joint; (10) outside elbow joint; (11) lateral surface of the upper arm; and (12) medial surface of the upper arm.

Instructions. *O* was blindfolded, and the following instructions were read to him:

"You will be stimulated at two areas on your left forearm. Report which of the two areas yields the more intense pain. If they are equal, report 'equal'; if you are doubtful, report that fact and the stimulation will be repeated. You will be told which two of the areas will be stimulated. Assist *E* by bringing the second area to be stimulated into the proper position (horizontal) as rapidly as possible."

The authors assured themselves that both *E* and *O* thoroughly understood the instructions, and were executing them successfully. *E* was cautioned to hold the algesimeter perpendicularly to the skin, and *O* was requested to bring the plane of the area to be stimulated parallel to the plane of the floor. These instructions were given to ensure that the full pressure of the stimulus-point would bear upon the spot stimulated.

Procedure. In the first time-order, the low-numbered area was stimulated first.

* Accepted for publication December 1, 1940.

¹ H. Strughold, Über die Dichte und Schwellen der Schmerzpunkte der Epidermis in den verschiedenen Körperregionen, *Zsch. Biol.*, 80, 1924, 367-380.

In the second time-order, the low-numbered area was stimulated last. A planned order of stimulation was used so that no area was stimulated anew until all the other areas had been stimulated. The experiments with an *O* were completed in one 2-hr. laboratory period.

Of the four important variables—intensity of stimulation, duration of stimulation, interval between the stimulation of the two compared areas, and interval between stimulation of every pair of compared areas—the first alone was controlled; the other three varied, but, as the authors believed, only within small limits.

RESULTS

(1) *Temporal variations.* The number of times every area was judged 'more intense' was tabulated separately for each time-order. Judgments of 'equal' were credited as $\frac{1}{2}$ point for each of the two stimulated areas. An increase in the number of points in the second-time order demonstrates a negative time-error, whereas a decrease signifies a positive time-error.

Two methods of appraising temporal variations in pain-sensitivity are available: (a) a tabulation of the *frequency* of negative and positive time-errors for every *O*, together with a computation of the average frequencies of such time-errors; and (b) a computation of the *magnitude* of the time-error, *i.e.* the difference between the number of judgments of 'more intense' in the first and second time-orders.

(a) *Relative frequency of positive and negative time-errors.* Of the 600 cases (12 areas for every one of the 50 *O*s), 360 were negative time-errors (increases in the number of judgments of 'more intense' in the second time-order); 137 were positive time-errors (decreases in the number of such judgments); and 103 showed no change. The average number of areas, in the set of 12, displaying a negative time-error was 7.20, S. D. 2.62. The average number with a positive time-error was 2.74, S. D. 1.89. The average number showing no temporal variations was 2.06, S. D. 1.59. The critical ratio of the difference between the frequencies of positive and negative time-errors was 9.76; between negative time-errors and 'no change,' 11.84; and between positive time-errors and 'no change,' 1.95. Thus, negative time-errors occurred with a reliably greater frequency than both positive time-errors and instances of 'no change,' whereas positive time-errors did not occur with reliably greater frequency than instances of 'no change.'

(b) *Magnitude of time-errors.* Of the 360 negative time-errors, the average magnitude (*i.e.* the average increase in the frequency with which an area is reported as 'more intense' in the second time-order) was 2.33, S. D. 1.56. Of the 137 positive time-errors, the average magnitude was 1.69, S. D. 1.22. The critical ratio of the difference is 4.8. This analysis, therefore, leads to the same conclusion as the previous one: negative time-errors are greater than positive time-errors.

(2) *Areal variations.* An index to the relative sensitivity of the areas investigated in this experiment was secured by averaging the number of times an area was judged as 'more intense' in both the first and second time-orders (thus cancelling the effects of the time-errors). Table I presents the results of these computations, with the areas arranged in order of decreasing sensitivity.

As can be seen from the "range" and "S.D." columns in the table, the relative sensitivity of an area is extremely variable. This variability is even more strikingly demonstrated in the last column. The figures for this column were secured in the

following manner. For each *O*, the relative sensitivity of the different areas was computed as explained in the paragraph above, and an ordinal number was then assigned to each area to show its relative standing—1 indicating that the area was the most sensitive of the twelve, 2 indicating that the area was the second most sensitive, and so on. The range of such ordinal numbers for each area was then determined. Thus, the area inside the elbow joint was the most sensitive area for some *O*s, sixth in order of sensitivity for others, and so on. These variations are due, perhaps, to the inexperience of our *O*s. Yet despite the large overlapping to be seen in Table I, the majority of the differences have critical ratios greater than 3. Table II presents the critical ratios for the differences among the various areas.

TABLE I
AREAL VARIATIONS IN PAIN SENSITIVITY

Area		No. judgments of "more intense"			Range of ranks
No.	Location	Av.	Range	S.D.	
9	Inside elbow joint	8.71	5.75-11.00	1.45	1-6
4	Ventral wrist	7.60	4.50-11.00	1.84	1-8
2	Dorsal region, thumb and index finger	6.52	1.50-10.75	2.36	1-12
1	Radial side, second phalanx, middle finger	6.28	0.25-11.00	2.87	1-12
8	Upper ventral forearm	5.73	2.75-9.75	1.26	1-11.5
12	Upper arm, medial surface	5.51	1.25-10.00	2.21	1-12
7	Lower ventral forearm	4.93	0.00-9.50	1.57	1-12
11	Upper arm, lateral surface	4.53	1.00-10.00	2.30	1-12
10	Outside elbow joint	4.28	0.00-9.00	2.07	3-12
3	Dorsal wrist	4.10	1.00-7.50	1.53	3-12
6	Upper dorsal forearm	4.01	0.00-8.25	1.47	2-12
5	Lower dorsal forearm	3.43	0.50-6.50	1.59	3-12

Scrutiny of Tables I and II reveals the following results.

(a) The area inside the elbow joint is reliably more sensitive than all other areas. All critical ratios are greater than 3.

(b) The ventral wrist is reliably more sensitive than all other areas ranking below it in Table I except the dorsal region between thumb and index finger, and the radial side of the second phalanx of the middle finger. For these latter areas, the probabilities of the differences having arisen by chance are very small, as is shown by the critical ratios of 2.74 and 2.55. To avoid awkward circumlocution, these differences will be spoken of as significant, as will all other differences which could have arisen by chance only 2 times in 100, or less (critical ratios of 2.00 or greater).²

(c) The dorsal region between the thumb and the index finger and the radial side of the second phalanx of the middle finger are not reliably different in sensitivity. Both these areas, however, are reliably more sensitive than all other areas ranked as less sensitive in Table I except the upper ventral forearm and the medial surface of the upper arm. Only the dorsal region between thumb and index finger is reliably more sensitive than these last areas.

² The point need not be labored that it is arbitrary to speak of a difference as significant only if the critical ratio is equal to or greater than 3. The probabilities of the differences between areas 4 and 1 and areas 4 and 2 having arisen by chance are sufficiently small (0.003 and 0.006 respectively) to enable us to conclude with a high degree of confidence that the differences are attributable to true areal variations in pain-sensitivity.

(d) The upper ventral forearm and the medial surface of the upper arm are not reliably different from each other in sensitivity. The upper ventral forearm is reliably more sensitive than all areas ranked as less sensitive. The medial surface of the upper arm is reliably more sensitive than all areas ranked as less sensitive except the lower ventral forearm.

(e) The lower ventral forearm, the lateral surface of the upper arm, and the outside elbow joint are not reliably different from each other in sensitivity.

(f) The lower ventral forearm is reliably more sensitive than the dorsal wrist and the upper and lower dorsal forearm.

(g) The lateral surface of the upper arm is not reliably more sensitive than the dorsal wrist and the upper dorsal forearm, but it is reliably more sensitive than the lower dorsal forearm.

(h) The outside elbow joint is not reliably more sensitive than the dorsal wrist and the upper dorsal forearm, but it is reliably more sensitive than the lower dorsal forearm.

(i) The dorsal wrist is not reliably more sensitive than the upper dorsal forearm, but it is reliably more sensitive than the lower dorsal forearm.

TABLE II

CRITICAL RATIOS OF THE DIFFERENCES BETWEEN EACH AREA AND ALL OTHER AREAS
(Critical ratios showing significant differences are in bold-face.)

Area	1	2	3	4	5	6	7	8	9	10	11
1. Radial side middle finger	—										
2. Dorsal thumb and index	0.46	—									
3. Dorsal wrist	4.74	6.09	—								
4. Ventral wrist	2.74	2.55	10.36	—							
5. Lower dorsal forearm	6.13	7.66	2.13	12.11	—						
6. Upper dorsal forearm	4.98	6.39	0.30	10.80	1.88	—					
7. Lower ventral forearm	2.91	3.95	2.69	7.79	4.74	3.04	—				
8. Upper ventral forearm	1.23	2.07	5.83	5.91	8.00	6.30	2.81	—			
9. Inside elbow joint	5.35	5.60	15.48	3.37	17.32	16.11	12.48	10.94	—		
10. Outside elbow joint	3.99	5.04	0.60	8.47	2.29	0.75	1.78	4.24	12.38	—	
11. Lateral surface of upper arm	3.36	4.26	1.11	7.37	2.78	1.36	1.02	3.24	10.87	0.58	—
12. Medial surface of upper arm	1.50	2.22	3.77	5.19	5.47	4.07	1.53	0.62	8.67	2.91	2.19

(j) The upper and lower dorsal forearm do not differ significantly in sensitivity.

These rather complex inter-relationships will be discussed in the next section.

DISCUSSION

(1) *Time-errors.* Time-errors have been noted in experiments involving successive comparison from the time of Fechner's weight-lifting experiments. The usual time-error has been negative.³ It has been demonstrated, however, that, for auditory stimuli at least, the sign of the time-error is a function of the temporal interval between the presentation of the first and second stimulus.⁴ Positive time-errors appear with short intervals; as the interval is lengthened negative time-errors come in with increasing effect, and then give way again to positive time-errors.

In the present experiment, we have demonstrated that the successive comparison of pain intensities is also subject to a time-error which in some cases was positive, in others negative. The occurrence of errors of both signs is probably to be at-

³ Cf. the discussion of time-errors in R. S. Woodworth, *Experimental Psychology*, 1938, 438-449.

⁴ *Ibid.*, 441 f.

tributed to the variable time-interval, which was not controlled in this experiment. Support to this conclusion is provided by the fact that negative time-errors occurred with reliably greater frequency (2.6 times as great) and magnitude (1.4 times as great) than positive time-errors. This result is to be expected when one recalls that the range of temporal intervals during which positive time-errors occur is much smaller than the range during which negative time-errors occur.

The investigation of temporal variations, in other words, has demonstrated that there is a strong tendency for a second pain to be intensified by an immediately preceding pain.

(2) *Areal variations.* In Strughold's experiment on areal variations in pain-sensitivity, measured by limens, two important conclusions were reached: the pain-limen is determined less by the number of pain points in an area than by the thickness and firmness of the epidermis, and, as a corollary, unprotected surfaces are much more sensitive to pain than are protected surfaces.⁵ The results of the present experiment, using supraliminal intensities, are in agreement with Strughold's results. The outstanding facts are: (1) the area inside the elbow joint is reliably more sensitive than all other areas; (2) the ventral wrist, the upper ventral forearm, and the dorsal region between thumb and index finger are all reliably more sensitive than the outside elbow joint, the dorsal wrist, the upper dorsal forearm, and the lower dorsal forearm; and (3) the medial surface of the upper arm is significantly more sensitive than the lateral surface.

One result which cannot be easily understood is that the upper ventral forearm is reliably more sensitive than the lower. The lower ventral forearm, moreover, is not reliably different in sensitivity from the lateral surface of the upper arm or the outside elbow joint, although it is reliably more sensitive than the dorsal wrist or forearm.

With the exception, then, of the lower ventral forearm, our results plainly lead to the general conclusion that pain sensitivity is greatest at 'vital areas' of the body, where the skin is thinnest and where blood vessels and nerves come close to the surface.

SUMMARY AND CONCLUSIONS

In a preliminary study, the relative sensitivity to pain of 12 areas on the left arm was investigated in 50 *O*s with a constant-pressure needle-algesimeter by the method of paired comparisons. The results may be summarized as follows.

(1) Both negative and positive time-errors occur in the successive comparisons of pain-intensities, the former in greater frequency (2.6 times as great) and with greater magnitude (1.4 times as great) than the latter. There is, therefore, a strong tendency for a first pain to intensify an immediately subsequent pain.

(2) The existence of time-errors of both signs is attributed to the fact that the interval between stimulation of the first and second area of a pair was not controlled.

(3) Despite great individual variability, the conclusion is justified that pain sensitivity is greatest at 'vital areas' of the body, *i.e.* where the skin is thinnest, and where nerves and blood vessels come close to the skin. This conclusion is in accord with the results of Strughold, who measured sensitivity by limens.

⁵ Strughold, *op. cit.*, 377, 379.

APPARATUS

A CLASSROOM DEMONSTRATION OF THE CONDITIONED RESPONSE

By JOHN P. FOLEY, JR., George Washington University

A number of attempts have been made to meet the need for a classroom demonstration of the conditioning process in the human subject, apart from the use of mechanical models, analogues, and dolls, which are not concerned with the conditioning process *in vivo*. Such demonstrations must be simple and must involve equipment which is economically constructed and easily operated. The resulting conditioning should be obtained quickly and unequivocally, should be observable by a number of students simultaneously, and should demonstrate some of the more basic principles of conditioning, such as reinforcement, experimental extinction, spontaneous recovery, generalization, and the like. A survey of the available techniques reveals that it is difficult to draw the line between demonstrational and exclusively research techniques, many having been used for both purposes.

In general, two types of conditioning situations have been used for demonstrational purposes; the salivary and the motor. The salivary, derived from Pavlov,¹ has been extensively applied to the human *S* by investigators in the Krasnogorski and Chuchmarev laboratories in Russia² and by Lashley in this country.³ In place of the necessary cannula or salimeter and somewhat involved procedure used by these investigators, Razran has substituted the use of dental absorbent cotton rolls, whose increase in weight is taken as an index of salivation.⁴ This adaptation of the technique of salivary conditioning has been used for class-room purposes, although it is more complicated than certain methods of motor conditioning to be described below.

The second method, *i.e.* motor conditioning, stems from Bechterev,⁵ who used a withdrawal response to electric shock as the unconditioned response. In the research use of this method, various motor activities have been employed, including activities accompanying eating (Krasnogorski's and Bechterev's laboratories, Mateer), grasping some object (*e.g.* bulb) to obtain food (Ivanov-Smolensky's laboratory), withdrawal from shock (Bechterev's and Ivanov-Smolensky's laboratories), and other motor (including verbal) responses. Perhaps the most widely used procedure for demonstrating human conditioning is Watson's modification of the Bechterev method,⁶

¹ P. Pavlov, *Conditioned Reflexes* (transl. by G. V. Anrep), 1927, 1-430.

² A description of the various methods and types of salimeter used in salivary conditioning is given by G. H. S. Razran, Conditioned responses in children: A behavioral and quantitative critical review of experimental studies, *Arch. Psychol.*, 23, 1933, (no. 148), 1-120.

³ K. S. Lashley, The human salivary reflex and its use in psychology, *Psychol. Rev.*, 23, 1916, 446-464; Reflex secretion of the human parotid gland, *J. Exper. Psychol.*, 1, 1916, 461-493.

⁴ G. H. S. Razran, Conditioned responses: An experimental study and a theoretical analysis, *Arch. Psychol.*, 28, 1935, (no. 191), 1-124.

⁵ V. M. Bechterev, *General Principles of Human Reflexology*, 1932, 1-467.

⁶ J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, 1919, 32-38.

in which *S* places his hand on a flat wooden base provided with binding posts and a palmar electrode; a saddle-shaped button immediately over *S*'s finger is attached to a receiving tambour located above the finger electrode, so that each movement of the finger evoked by a shock from the secondary of an inductorium is transmitted to the receiving tambour and recorded kymographically. This method is described in most laboratory manuals and elementary textbooks.⁷

Several investigators have described modifications of the Bechterev-Watson method. Schlosberg and Carmichael have described a method in which the unconditioned stimulus is an electric shock and the conditioned stimulus a loud buzz, a kymographic record being taken of the presentation of the stimulus, time, and the response.⁸ Barkley has reported a simplified version of the method, in which finger retraction to electric shock is conditioned to the sound of a bell.⁹ Humphrey modified the Watson apparatus in such a way as to indicate, by means of a light, whether or not *S*'s hand is resting upon the electrode base; the pairing of the unconditioned (shock) stimulus with a conditioned (auditory) stimulus thus establishes a conditioned withdrawal response which can easily be detected by a large class.¹⁰ Recently Henneman and Talley¹¹ have combined the Bechterev-Watson technique with the bulb-grasping response employed by Ivanov-Smolensky. Shock stimuli, controlled by an automatic interval-timer, are delivered by electrodes taped to a rubber bulb held in *S*'s hand, the changes in pressure (unconditioned response to shock) being recorded kymographically or by means of lights if the demonstration is before a large class. The method is reported to have the advantages that relatively weak shocks produce adequate responses and that "a blindfolded *S* is usually unaware of his manual reaction to the conditioned stimulus, hence attitude factors are lessened." Bousefield has described a simple technique employing common materials in everyday use.¹² A golf ball is dropped from behind a screen, of adjustable height, upon *S*'s extended hand, he having just enough time to respond by withdrawing at the sight of the falling ball. The dropping of the ball (unconditioned stimulus) is paired with an auditory click (conditioned stimulus), so that the click alone comes to elicit the withdrawal (conditioned response).

There are special difficulties encountered in attempts to demonstrate conditioning in the human *S*. Not only are certain simple reflexes, such as the abdominal, patellar, plantar, and Achilles, extremely difficult to condition,¹³ but results are complicated by the presence of voluntary and other ideational factors which complicate the situation. In 1935, the writer pointed out that "experimental work on conditioning in complex organisms, such as the monkey, ape, and particularly the human, is often

⁷ Also cf. C. H. Stoelting, *Psychological and Physiological Apparatus and Supplies*, 4th ed., 1939, 68.

⁸ H. Schlosberg and L. Carmichael, A simple apparatus for the conditioned reflex, this JOURNAL, 43, 1931, 120-122.

⁹ K. L. Barkley, A laboratory class demonstration of the establishment of a conditioned reflex, *J. Exper. Psychol.*, 15, 1932, 97-103.

¹⁰ G. Humphrey, A simple apparatus for the class demonstration of the conditioned response, *J. Educ. Psychol.*, 19, 1928, 61-63.

¹¹ R. H. Henneman and J. G. Talley, A classroom demonstration of the conditioned response, this JOURNAL, 54, 1941, 118.

¹² W. A. Bousefield, A simple demonstration of the conditioned response, *Science*, 90, 1939, 70.

¹³ E. R. Hilgard and D. G. Marquis, *Conditioning and Learning*, 1940, 35.

completely vitiated by the fact that certain implicit reactions, with their attendant stimulations, are frequently occurring simultaneously or temporally adjacent to the predetermined stimulations experimentally administered by the investigator. Any human subject in such a study, for example, will frequently report that certain 'mental sets,' 'thoughts,' or 'ideas' occurred to him in the course of the experiment, and it would be quite absurd blithely to deny the existence or stimulatory value of such symbolic activities."¹⁴

The variable results of Razran on salivary conditioning in the human S,¹⁵ and his failure to obtain consistent experimental extinction, have led Hilgard and Marquis to conclude, after a survey of the literature, that "the conditioned salivary response in human subjects is erratic and not predictable from the particular properties of the stimuli and amount of training."¹⁶ They elsewhere point out that "even the most involuntary responses are subject to facilitation or interference by voluntary sets and attitudes of the organism,"¹⁷ and add that "some conditioning experiments are unsuccessful because of the presence in the situation of strong antagonistic tendencies which interfere with the performance of the conditioned responses. Several experimenters have reported failure to secure conditioned finger retraction based on unconditioned shock stimulation because of a competing tendency which is established by general instructions to hold the fingers on the electrodes."¹⁸ The validity of this analysis is indicated by Razran's experiment in which regular and progressive acquisition and stable magnitude of conditioned salivary responses in the human S were obtained by requiring him to concentrate on the task of learning a manual maze during the presentation of the conditioning stimuli.¹⁹ In a further experiment, Razran²⁰ misled his Ss with respect to the purpose of the experiment and secured results conforming closely to those expected on the basis of orthodox conditioning principles.²¹ Leuba has recently proposed the use of hypnosis as a means of eliminating extraneous variables in the form of symbolic and other self-initiated stimuli in the human S;²² the successful and rapid conditioning of sensory responses (images) under hypnosis would appear to bear out this hypothesis.

¹⁴ J. P. Foley, Jr., A critical note on certain experimental work on the conditioned response, *J. Gen. Psychol.*, 12, 1935, 444.

¹⁵ Razran, *op. cit.*

¹⁶ Hilgard and Marquis, *op. cit.*, 261.

¹⁷ *Idem*, 34.

¹⁸ *Idem*, 35. This fact was impressively demonstrated to the writer several years ago in a laboratory demonstration of human conditioning with the Bechterev-Watson technique. The subject proved very difficult to condition, and orthodox results were never obtained. Several months later, the subject reported that while resting at home he was surprised to observe a definite finger-retraction at the sudden ringing of the doorbell, a conditioned response which was never unequivocally demonstrated in the previous laboratory situation.

¹⁹ Razran, Attitudinal control of human conditioning, *J. Psychol.*, 2, 1936, 327-337.

²⁰ Razran, A simple technique for controlling subjective attitudes in salivary conditioning of adult human subjects, *Science*, 89, 1939, 160-161.

²¹ Hilgard and Marquis, *op. cit.*, 262.

²² C. Leuba, The use of hypnosis for controlling variables in psychological experiments, *J. Abnor. & Soc. Psychol.*, 36, 1941, 271-274.

Apparatus. The present paper describes a modification of Bousefield's technique (described above) in the direction of extension and greater standardization, without materially increasing the cost of the apparatus or the complexity of the procedure. The apparatus consists essentially of a box, 65 cm. high, 28 cm. wide, and 24 cm. deep, divided by a horizontal partition (20 cm. from the top of the box) into upper and lower compartments. A hole in the center of the partition contains a cylindrical tube, 5 cm. in diam., which is flush with the top of the partition but projects down approximately 10 cm. into the lower compartment.

The upper or apparatus-compartment contains a plummet (a golf ball can be used) which is tied to a string, the string passing through an eyelet or pulley mounted in the roof of the compartment directly above the center of the tube-opening, and being fastened at the other end to the floor of the compartment. A release device (e.g. a metal clip or wooden clothespin) is also mounted on the floor of the compartment, so that the plummet can be drawn up into the tube, the string temporarily clamped in the release device, and the plummet suddenly released by pressing the release mechanism. The string should be of such length that the plummet will fall to within approximately 1 cm. of the floor of the bottom compartment. The upper compartment also contains a bell and buzzer (or other auditory stimuli) together with dry cells and push-type switches mounted flush with the floor. The compartment is open to the experimenter at the back, but is closed by an adjustable panel at the front, the panel sliding in grooves so that the amount of the lower (reaction) compartment visible to *S* can be regulated.

The lower or reaction-compartment is open at the front but closed at the back by a panel approximately 31 cm. high. This leaves an opening through which the experimenter, unseen by *S*, can observe *S*'s hand when placed in the box. This panel also contains a stimulus-light, wired in series with the dry cells and its separate switch above. A white square is painted or glued to the floor of the reaction-compartment, immediately underneath the tube and suspended plummet.

Procedure. The conditioning box, or "conduillotine," is placed on a table at which *S* is comfortably seated. *S* extends his arm and places his hand, palm downward, over the white square on the floor of the reaction-compartment. He is told that he is to watch for the falling plummet and to withdraw his hand so as to avoid being hit. The front panel should be adjusted until the duration of the visual exposure of the falling plummet is well adapted to *S*'s reaction-time. The light, bell, or buzzer is then presented several times, in order to demonstrate that there is originally no withdrawal response, after which it is reënforced by the falling plummet.²³ After several reënforcements, the light alone will evoke the conditioned withdrawal response.

Phenomena demonstrated. Conditioned withdrawal responses can be formed by this method with as few as a single reënforcement. *Experimental extinction* can be demonstrated by repeatedly presenting the light (conditioned stimulus) alone with-

²³ If both the light (or other) switch and the release mechanism are pressed simultaneously, the light will be presented shortly before the falling plummet, owing to friction in the latter mechanism, thereby roughly obtaining the optimal time-relations between the conditioned and unconditioned stimuli.

out reënförment from the falling plummet, the withdrawal response becoming weaker and weaker until it no longer occurs. *Spontaneous recovery* can also be demonstrated by presenting the unreënförmed conditioned stimulus after a lapse of time. *Generalization* can be shown by conditioning a withdrawal response to the bell and then presenting the buzzer without reënförment. *Differentiation* can also be established between the bell and the buzzer by the method of contrasts or differential inhibition. If *S* is suddenly stimulated by an extraneous stimulus following experimental extinction, *disinhibition* may be observed in the temporary appearance (increment) of the conditioned withdrawal response. It is also possible to demonstrate *higher-order conditioning* by reënföring the neutral bell with the previously conditioned light-stimulus.

As stated above, the present technique represents an extension and standardization of a procedure originally suggested by Bousefield. In their classification of conditioned response methodology, Hilgard and Marquis distinguish between *classical conditioning*, as represented by the typical Pavlovian reference experiment, and *instrumental conditioning*,²⁴ in which the reënförment is presented only after the conditioned response has been made and is contingent upon *S*'s behavior in the experimental situation. The latter type of conditioning is further subdivided into "reward training," "escape training," "avoidance training," and "secondary reward training," according to the nature of reënförment employed. The present demonstrational procedure might be considered as an example of instrumental avoidance training in so far as the conditioned withdrawal response to the light or buzzer, originally made only to the falling plummet, prevents the occurrence of a noxious stimulus.²⁵ On the other hand, if one considers the unconditioned stimulus for the avoidance response to be the "sight of the plummet," which is paired with the conditioned light or buzzer stimulus, the situation approximates the classical Pavlovian experiment.²⁶

The apparatus and procedure meet the common demonstrational demands of simplicity, economy, and compactness. Although the response is not quantified, as would be required for research work, the administration of stimuli and *S*'s withdrawal response can be observed readily by a large class. Perhaps the chief advantage of the technique lies in the rapid and unequivocal results obtained, in marked contrast to the difficulties frequently encountered in human conditioning. Since *S* must attend to the falling plummet, this active-set may have a distracting effect similar to that achieved by Razran in requiring his *S*s to learn a manual maze during the administration of the conditioning stimuli. The variety of the basic phenomena of conditioning that may be demonstrated by the apparatus described here is also a desideratum for classroom purposes.

²⁴ Hilgard and Marquis, *op. cit.*

²⁵ Strictly speaking, however, *S*'s unconditioned withdrawal response to the sight of the falling plummet would, if sufficiently rapid, prevent the occurrence of the noxious stimulus, even before the conditioned withdrawal response was established.

²⁶ The "sight of the plummet" itself might be regarded as a conditioned stimulus, having acquired its efficacy through reënförment from verbal instructions to avoid the plummet or from the "painful" stimulation resulting from the plummet's blow if *S* failed to withdraw his hand in time. In this case the method would appear to be an example of classical higher order conditioning.

APPARATUS NOTES

DEMONSTRATIONAL USES OF SMALL PROJECTORS

The writer has developed several demonstrations in the field of vision employing as the basic equipment one or more of the small projectors designed for the 2 x 2 in. lantern slide. Even the 100-w. models serve admirably under reasonably favorable conditions.

Typical of the uses to which such equipment may be put is the demonstration of the phi-phenomenon. For this purpose two 2 x 2 in. colored slides are prepared. Squares of red and green gelatin filter material are placed in the standard 24 x 36 mm. masks and then bound in glass. The particular choice of materials is not important, though high chromatic contrast is apparently desirable. The writer uses Wratten 29 for the red slide and Wratten 58 for the green.

Two lanterns are then focussed so that their projected images are contiguous and in the same vertical plane. An episcotister, with a sector of 120° , is placed close in front of the lanterns so that the light beams are alternately interrupted as the episcotister is revolved. The phenomenon of apparent movement appears readily upon the light screen. The effect is not critical as to frequency. Any convenient rate of alternation, such as from 2-5 r.p.sec. will produce it. Though the Os invariably report the perception of movement, the mode of appearance varies widely. The most common description suggests that the phenomenon is seen tri-dimensionally. The colors are seen as localized on the obverse and reverse sides of a rectangle swinging about an axis passing vertically through its shorter edge. Horizontally, this axis is placed at what would be the edge of contiguity if the two colored rectangles appeared on the screen at the same time. At either extreme of the 180° oscillation the localization plane is on the screen, the red and green sides appearing alternately. During the dark period this localization plane is reported as swinging out toward O, perpendicularly to the screen. Although the illuminated figures can be made by using clear slides, the Os do not commonly report the tri-dimensional effect, although apparent movement appears equally readily.

Without any additional apparatus several other phenomena can be demonstrated. A slight shift of the lanterns will make the projected figures overlap. The particular red and green filters already mentioned yield an excellent yellow fusion color. With the rotary shutter interposed, several of the simpler flicker frequency laws may be verified. If additional filters are available, the possibilities of the apparatus are considerably extended. The lanterns may be employed as spotlights to illuminate colored charts, photographs, or objects, demonstrating the effects of the chromatic composition of the illuminant on their visual properties. The Ishihara or Stilling color vision plates can be employed as test objects and be so illuminated as readily to develop the principles underlying their construction. When, for example, plates 20 and 21 of the Ishihara tests (7th edition) are viewed in blue light the figures seen by the red-green blind are readily visible. These plates are not readable by either normal or totally color-blind Os under the customary illumination. The comprehension of color vision as a discrimination process appears to be considerably facilitated by demonstrations of this sort.

University of Minnesota

WILLIAM S. CARLSON

A MINIATURE COLOR-MIXER

The S. H. Kress Stores retail a novelty toy—the Mechanical Sparkler—which has been found to be adaptable as a miniature color-mixer. The toy is made up of a revolving wheel (having flint-emery paper friction contact producing the sparklers) operated by a thumb-controlled serrated spring plunger connecting with a ratchet wheel assembly. The diameter of the wheel is 2.5 in. and the approximate overall length of the toy is only 5 in. The fact that a high rate of rotation can be obtained suggested attaching miniature color paper-disks to the front of the toy, which were found to mix satisfactorily and to give approximately the same effects as are found in using the large electric-driven color-wheels. A slight change (detaching the emery paper-flint attachments and substituting a masonite wheel) added steadiness and speed. A longer shaft was added to the wheel and a detachable cap was made to hold the paper disks in place. The masonite wheel was marked off in degrees to control the mixtures. This has resulted in a satisfactory and extremely economical portable color-mixer that answers the needs of a small laboratory or of field work.

Duke University

ISOBEL MOORE

NOTES AND DISCUSSIONS

THE SIGNIFICANCE OF FERAL MAN

Supposed instances of feral man have been brought to attention from time to time, and these cases frequently have been cited as proofs of certain theories of human nature. The theories which they are usually said to uphold are two: (1) That the formation of early habits has a relatively permanent effect on an individual; and (2) That deprivation of social contacts in the early years tends to prevent the later socialization of the individual.

References to wild children as evidence for these views have been numerous in late years. Among the authors who recently have made use of the material on feral man in the manner indicated above are Stratton,¹ Kellogg,² Park and Burgess,³ Dawson and Gettys,⁴ Kingsley Davis,⁵ Briffault,⁶ and Zingg.⁷ Zingg is the latest contributor, his publication having appeared only a few months ago in this JOURNAL. His article provides an excellent historical survey, and his tabular presentation of the feral cases is the most convenient summary of them available. His conclusions are not novel; they are in agreement with those of previous authors mentioned above. He states: "Deprived too long of human associations, or animal-conditioned too strongly, the sensitive potentialities of human development are permanently inhibited and the traces of animal conditioning are never completely lost."⁸

Since modern writers on the topic of feral man have been prone to assume the authenticity of the accounts of wild children, it seems advisable to bring into full view the weaknesses which are inherent in these documents. Unless this is done now, these stories will be accepted without scepticism, and soon will take a place in our textbooks as "established facts."

In discussing the value of the accounts of feral man I wish to question, in the first place, the soundness of the belief that children in the earliest years of infancy have been *reared by animals*. This is not an uncommon claim. Twenty-two of the 31 cases of feral children reviewed by Zingg are alleged to have associated with and to some extent to have been cared for by animals. Not all of these, however, are said to have lived with animals from a very early period. Those for whom this claim is not made I should like to eliminate from the immediately following discussion.

On that basis four of the 22 cases must be removed since their histories indicate that these four must have been several years of age before their association with animals could have begun. In one instance a boy was set to tending pigs, and was

¹ G. M. Stratton, Jungle children, *Psychol. Bull.*, 31, 1934, 596-597.

² W. N. Kellogg, Humanizing the ape, *Psychol. Rev.*, 38, 1931, 160-176.

³ R. E. Park and E. W. Burgess, *Introduction to the Science of Sociology*, 1922, 239-242.

⁴ C. A. Dawson and W. E. Gettys, *An Introduction to Sociology*, 1929, 603-604.

⁵ Kingsley Davis, Extreme social isolation of a child, *Amer. J. Sociol.*, 45, 1940, 554-565.

⁶ Robert Briffault, *The Mothers*, 1, 1927, 26-30.

⁷ R. M. Zingg, Feral man and extreme cases of isolation, this JOURNAL, 53, 1940, 487-517.

⁸ Zingg, *op. cit.*, 515.

so ill-fed that he ate with the swine, and, it is claimed, sucked the sows. It is hardly conceivable that a boy could have been delegated to guide the wandering of unfenced hogs until he was at least four or five years of age, hence the porcine influence began relatively late. The second case, a girl who was brought up in a pigsty, was apparently placed there and kept there by persons; this could hardly have been done at birth or even during the first two years. Two boys were reared by ruminants, one by sheep and the other by cattle. Since these species do not have dens to which they return, the children, if they did live with these species, must have been able to locomote in order to follow the herd. It scarcely seems possible that the Bamberger cattle-boy and the Irish sheep-boy spent their tenderest years with their foster parents. If they did not, one cannot readily attribute their beast-like characteristics to the environment of the earliest age.

The elimination of these four cases from among the records of those who are supposed to have spent their infantile period in non-human society still leaves a remainder of 18 cases. Of these, 13 are from India, 3 from Lithuania, and 2 from Germany.

What is the nature of the evidence that these children were suckled and fed by animals? It is entirely hearsay testimony, often reduced to writing many years after the capture of the wild child. While I have not had access to all of the original literature, so far as I can determine *not a single account has been written by an eye-witness of the fact that the child was captured in an animal den or in close company with an animal*. The fact that the persons who published the original accounts of children reared by animals were persons of unquestioned integrity should not be permitted to prejudice the case. All were writing what they had heard from others. The scientist's information in this case is no better than the hunter's, or peasant's or soldier's information.

The evidence that a child has been reared by animals usually consists of a statement that the child was found in an animal den or in company with animals. Even if these statements were well evidenced, they do not establish what they purport to prove. The child, being pursued, might have run into an animal's den. It need not have lived there. A child seen a short distance from an animal might have been perceived as being *with* the animal, particularly if both beast and child were trying to escape from a pursuer. It need never have seen the animal previously. Since the hearsay reports indicate only that children have been found in a den or in the immediate neighborhood of animals, they cannot be accepted as good proof of a real animal-child association.

A second kind of evidence for the rearing of an infant by animals is derived from reports of the stealing of an infant by a wolf and the recognition of the child at a later date. The interval between the abduction and the reported recapture is, in all accounts, a matter of years. Testimony as to the identity of a child who disappeared as a nursing and who was not seen for several years would be doubtful even without the intervention of a supposed acculturation to an animal mode of life.

In searching for the origin of the belief that a specific child was reared by beasts it would be relevant to examine the folk lore of the region from which came the original story. Unfortunately, I am not qualified to do this. Tylor,⁹

⁹ E. B. Tylor, Wild men and beast-children, *Anthrop. Rev.*, 1, 1863, 21-32.

however, noted that the myth of a child reared by animals is very widespread. The myth of Romulus and Remus is the best known version. Kipling employed this idea in his story of Mowgli in his tales of India. Ireland¹⁰ states that the wolf-child story was prevalent in India in Sleeman's time.¹¹ It is interesting to note that of a total of 14 reports of wolf-children 12 have come from India and most of these were described by Sleeman. Since wolves are found in an extremely wide area, the fact that wolf-children center chiefly in India is probably significant. Significant, too, is the fact that all of the three feral children reported from Lithuania were bear-boys. Bear-boys have been reported from no other area, although bears, like wolves, have a very extensive geographical distribution.

Because the idea of wolf-children is today current in India, if a mute, who could give no account of his past, were found in India at the present time, it is easy to guess the direction of speculation concerning his origin. Whatever events were related by the finder of the strange person, it is likely they would gradually be shaped to fit the folk legend. Bartlett has presented excellent evidence to the effect that stories when repeated tend to take on the cultural forms familiar to the raconteur.¹²

India possesses a large number of unfortunates to whom such a myth could be fitted. Ireland,¹³ making this point, indicates that, at least in Sleeman's time, there were no institutions for the mentally abnormal, and that idiots wandered freely.

Even without a traditional belief, peculiarities in a child's behavior might well suggest an animal-child association in terms of the formula: like causes like. To the naïve it is obvious that a child who eats raw meat has eaten with animals. Likewise a person who walks on all fours walks like an animal and may have learned from an animal. Abnormalities of behavior might readily give rise to the invention of such explanations. When it is recalled that today some persons in America are willing to believe that a child may look like an animal or may have some beast-like characteristic because its mother was frightened by an animal, we must not be too critical of the native who believes that a child who devours raw flesh has lived with a wolf. This interpretation, of course, would occur more readily to the vegetarian Hindu than to us.

It is not altogether certain that all of the wolf-children reported by Sleeman were genuinely believed by the natives to be wolf-children. Ireland has quoted a letter from the *Lucknow Witness* of June 19, 1874, which states that when the natives discovered that Colonel Sleeman was interested in wolf-boys they hastened to produce several idiots, positively declaring them to be authentic wolf-reared children. The desire to please and likewise the desire to pull the leg of the white man are not unknown among the darker-skinned races.

Zingg¹⁴ has cited the eminent English anthropologist, E. B. Tylor, as declaring for the authenticity of feral man. Zingg, however, failed to state that while Tylor did not hesitate to believe that individuals had lived outside of human society, he

¹⁰ W. W. Ireland, *Mental Affections of Children: Idiocy, Imbecility, and Insanity*, 1898, 422-433.

¹¹ W. H. Sleeman, *Journey through the Kingdom of Oude*, 1, 1858, 206-222.

¹² F. C. Bartlett, *Remembering: A Study in Experimental Social Psychology*, 1932, 1-317.

¹³ Ireland, *op. cit.*, 432-433.

¹⁴ Zingg, *op. cit.*, 491-492.

specifically stated his disbelief that children had been suckled by animals. Tylor's conclusion is as follows: "The whole evidence in the matter comes to this. First, that in different parts of the world children have been found in a state of brutalization, due to want of education or to congenital idiocy, or to both; and secondly, that people often believe that these children have been caught living among wild beasts, a supposition which accounts for their beast-like nature. . . . I cannot see that the whole evidence on the subject proves anything whatever, except the existence of the stories, and the fact that there have been and still are people who believe them."¹⁵

I hope it is clear that for my own part I am merely sceptical. No attempt has been made to prove that children have *not* lived with animals, but I have tried to show that there is no good evidence that this has occurred. The evidence is unconvincing. The burden of proof rests upon those who want to draw some conclusion from these accounts, and they have gone to no pains to support their acceptance of the stories.

The conclusion which those who uphold the belief in beast-reared children wish to make, is, as I stated earlier, that the absence of human contacts during the early years has almost irremediable effects. In order to make clear the exact nature of these views, let me cite in full some recent writers:

Stratton has written as follows: "There is evidence that children have lived either in complete isolation or in association with wild beasts, and after return to human society they have shown lasting defects in gait, speech and feeling. Nor is it highly probable that all of these children were feeble-minded at birth. Lack of association with adults during a certain critical period of early childhood, it seems likely, produces in some or all normal children marks like those of congenital defect."¹⁶ Kellogg has expressed himself in a similar manner concerning the impossibility of socializing children who have lived in isolation: "The inability to acquire the desired kind of behavior even with careful training is assignable to the fact that they had advanced to too mature an age to uproot the fundamental habits so basically entrenched by earlier experience. This explanation follows readily from the recognized importance of the very early years in psychological development."¹⁷ Among the writers who have expressed these views is Kingsley Davis, who has stated that "it seems almost impossible for any child to speak, think and act like a normal person after a long period of early isolation."¹⁸

These views do not necessarily put stress upon living with animals. Zingg, however, is inclined to think that association with animals is more serious in its effects than is isolation, for the reason that the (hypothetical) child living with sub-human companions forms habits and attachments which must be overcome when he enters human society. For the most part, however, it is claimed that isolation during infancy will in itself render the later establishment of basic social habits very difficult, if not impossible.

In the discussion thus far I have emphasized the uncertainty of the belief that children have been suckled and fed by animals. This uncertainty has a very direct

¹⁵ Tylor, *op. cit.*, 32.

¹⁶ Stratton, *op. cit.*, 596.

¹⁷ Kellogg, *op. cit.*, 162.

¹⁸ K. Davis, *op. cit.*, 564.

bearing not merely upon the interpretation of accounts of alleged animal-reared children but also upon the interpretation of accounts of children stated to have lived in isolation. For if we are not convinced that infants have been reared by animals, I believe it can be shown that we are not sure that any surviving child has ever been isolated from human society *during the early years*. Isolation is possible only beyond the period of infancy.

No one, I believe, will propose that a normal child could survive in isolation until it is beyond the second year of life. At what age could an infant live exclusively on an uncooked solid diet, consisting of soil, grass, berries, seeds, worms and whatever larger game it could catch? No pediatrician would attempt to answer the question experimentally. We know how high was the infant mortality of the first two years in the most enlightened countries until very recent times. What would it be without any care whatsoever? The infant on his own in the woods could not be likened to C. M. Davis' subjects in her experiment on the self-selection of diet,¹⁹ since her subjects had before them every element which they needed and none which would harm them, whereas the wild child would have before him but little food and a multitude of harmful objects.

In asking at what age a child could shift for himself, we are guessing in the dark, since we do not really know. The feral children do not provide an answer, for we do not know when any of the children became estranged from society. Amala, who was judged to be less than 2 yr. old, is the youngest ever to have been found; but she was with Kamala who was estimated to be 8 yr. of age. No feral child other than Amala has been estimated to have been less than 4 yr. old upon discovery.²⁰ If it be assumed that below the age of 3 yr. a child has no chance of surviving in isolation, this assumption has important implications for the doctrine of the permanence of early habits and of the deleterious effects of early isolation.

Linnaeus described feral man as *tetrapus* and *mutus*.²¹ Zingg, as well as others, still hold this view. The majority of Zingg's 31 cases were mute and ran on all fours when captured, according to the reports. (Hirsutism, the third trait mentioned by Linnaeus has been dropped by more recent authors. Wild men are not usually hairy.) Fear of man is another feral quality.

Now let us return to the 3-yr.-old lost in the forest. If he were a normal 3-yr.-old he would have been neither mute nor quadrupedal, nor anti-social. To the contrary, he would have had a fair mastery of the mother tongue, could have walked on two legs very well, and would have preferred human companionship.

Why then have many of the wild children which have been found run on all fours? Why have they shown no use of language and no comprehension of it? Why have they feared people and resisted capture?

Four lines of explanation are open. (1) One is to suppose the child was lost in early infancy and to reaffirm the belief in the suckling of infants by animals. That seems to be the only way of accounting for survival of nurslings in isolation. I have shown how poor is the evidence for such a belief. The best reason for the

¹⁹ C. M. Davis, Choice of formulas made by three infants throughout the nursing period, *Amer. J. Dis. Child.*, 50, 1935, 385-394.

²⁰ Zingg, *op. cit.*, 500-504.

²¹ Carl von Linné, *Systema naturae*, 10th ed., I, 1758, 21; cited by Zingg, *op. cit.*, 487.

belief is the condition of the feral children, which suggests that they were reared by animals. If, however, their condition can be explained on some other grounds, there is no need of accepting that suggestion. (2) A second explanation is that infants may survive even if they become isolated before the third year. Scientific caution would avoid accepting such an unwarranted assumption. There is no evidence to lead us to accept such a view, except the existence of the wild children. (3) An explanation of the muteness and the four-footed locomotion of feral children consists in supposing that normal locomotion and speech are soon lost when one ceases to associate with people. This would involve an abandonment of the theory of the permanence of early habits, which those who stress the data concerning wild children are trying to prove. (4) Lastly, idiocy may be urged as an explanation. This suggestion, as we have seen, was made by Tylor, and it has been made by others.

Let us see whether the characteristics of the lower-grade feeble-minded are the same as the characteristics of feral man.

(a) Feral man is ordinarily mute. Kamala, after painstaking training from the age of 8 to 16 yr. could speak only 50 words and her record is among the best.²² Deficiency in language is an outstanding trait of the lower-grade defective also. Dr. Singh's statement that Kamala did not appear to be feeble-minded cannot be taken as conclusive. Furthermore, the linguistic level of idiots and imbeciles is sometimes considerably below their mental level. Hunter, in Kalynack's *Defective Children* states: "A condition of congenital motor aphasia may be met with among them (feeble-minded), where the individual has fair intelligence, understanding all that is said to him, and yet his vocabulary may be limited to two or three words."²³ This is often the condition of the wild child after a period of training.

(b) Feral man upon capture, if not entirely mute, may make animal sounds. Tredgold says of mute idiots: "Their utterances mostly consist of inarticulate grunts, screeches, and discordant yells."²⁴ These could readily be perceived as bestial vocalizations.

(c) Feral man is untidy. Kamala, before she was carefully trained "served the calls of nature anywhere, anytime." Again Tredgold states that "the majority of mentally defective children are late in acquiring control over the bladder and rectum; indeed, in the lowest grades such control may never be developed."²⁵

(d) Feral man will eat things which civilized man considers disgusting. In this connection it is relevant again to quote Tredgold,²⁶ who was cited in a different connection by Zingg: "Some idiots will eat and drink anything which comes within their reach, including wood, leather, grass, earth, stones, even urine and faecal matter, or offal of the most putrid description."

(e) Feral man frequently is grossly insensitive to heat and cold. This too has its parallel among the mentally deficient. The following quotation also is from Tredgold: "In the lower grades this capacity (the temperature sense) seems to be

²² Zingg, *op. cit.*, 504.

²³ T. N. Kelyack, *Defective Children*, 1915, 47.

²⁴ A. F. Tredgold, *Mental Deficiency*, 3rd ed., rev., 1920, 202.

²⁵ *Ibid.*, 152.

²⁶ *Ibid.*, 105.

wanting, and such will sit in front of the hottest fire, under the most blazing sun, or exposed to the coldest wintry blast, without showing any concern."²⁷

(f) A feral child commonly shows little or no attachment to human beings, unless one person has for a long time carefully supplied his needs or unless two children have been associated as had Amala and Kamala. Morrison studied the degree of affection for persons of 228 inmates of an institution for the feeble-minded.²⁸ She found idiots to rank low in scores for affection. The correlations of affection scores with *MA* and *IQ*, respectively, were for this group 0.73 and 0.69.

(g) Feral man is said to walk on all fours. The locomotor habits of feral men have not been described in detail. To say that they walk on all fours probably involves an element of interpretation in the direction of perceiving a likeness to animals. It would probably be most correct to state that many wild children cannot walk upright. Neither can many idiots. It is customary in the case of the more helpless idiots simply to say that they cannot walk. When under care they are encouraged not to crawl about. If they were forced to move about it is possible that many would go on all fours. Many idiots have a spasticity of the legs which makes it difficult to bend the leg at the knee. Levy and Tulchin have proposed that a slight spasticity, ordinarily caused by birth injuries, is a possible cause of going on all fours in some children.²⁹

When it is proposed that reported instances of feral man are cases of congenital feeble-mindedness, two objections are raised. (1) The first of these is the improbability that *all* wild children should be congenitally feeble-minded, even though it is admitted that the mentally deficient would be more likely than would normal children to wander away, or to become lost and fail to return. To this objection it may be answered that the Songi girl was not feeble-minded.³⁰ It seems likely, however, that none of the other cases was fully normal in intelligence. In this connection, one must not overlook the possibility that the feral children may have been intentionally deserted, or lost, or driven from home and that normal children are not thus treated. It is not necessary to assume that the feral children were accidentally lost. Furthermore, if a *normal* child were found, he might not be classed as feral; if he could give some account of himself, he certainly would not be recorded as "wild."

(2) The second objection to the view that feral man has been, in nearly all instances, of low intelligence prior to his assuming the feral state is the argument that an imbecile, and particularly an idiot, could not shift for himself in a state of nature. Now in practically no case do we know how long the individual really shifted for himself. If he were taken some distance from home and released, or if he were driven from home, or if he wandered sufficiently far from home so that he was lost, he need not have supported himself for more than a day or a few days before being found. His muteness, his stupidity, would make it impossible for him

²⁷ *Ibid.*, 108.

²⁸ B. M. Morrison, A study of the major emotions in persons of defective intelligence, *Univ. Calif. Publ. Psychol.*, 3, 1924, 73-145.

²⁹ D. M. Levy and S. H. Tulchin, On the problem of "all fours" locomotion, *J. Genet. Psychol.*, 47, 1935, 193-203.

³⁰ I have not mentioned Kaspar Hauser in this connection because it is doubtful that he should be classed with the feral cases.

to account for his past. His family, wherever possible, might avoid claiming him. As I have indicated earlier, others might supply the wild-boy story. This was Blumenbach's conclusion concerning Peter of Hamelin.³¹ That such stories have been supplied, Zingg himself has shown in connection with the "baboon-boy."³²

The truth is that we do not know what were the life events of any feral man prior to the time he came under observation. On this account it cannot be proved that the so-called cases of feral man were feeble-minded from birth, since their history prior to their 'discovery' always remains a mystery. For the same reason it cannot be established that a period of isolation was the cause of the deficiencies in behavior recorded upon the individual's contact with society. In view of our ignorance concerning the history of alleged isolated children I suggest that these cases not be cited as evidence for any social or psychological theory.

We know that there are idiots and imbeciles. We know that alleged feral man has similar characteristics. Until we have better evidence it seems best to reserve judgment as to whether or not "feral" children were idiotic or imbecilic before they became "feral."

University of Virginia

WAYNE DENNIS

A REPLY TO PROFESSOR DENNIS

Professor Wayne Dennis has presented, in the note above, a very suggestive commentary to my article "Feral man and extreme cases of isolation."¹ His reply argues so skillfully against feral man that I should follow him were it true that "*not a single account has been written by an eye-witness of the fact that the child was captured in an animal den or in close company with an animal*" (italics his). This statement is irresponsive to mine that "the almost unique element in the recent case [the wolf-children of Midnapore] is . . . we have a careful record of nine years in human association after the rescue by the man who, in company with several others, saw the children in association with the animals which they killed before the children were brought again into human society."²

This is the crucial evidence which gave point to my whole article. Were it not for this evidence from Rev. Singh, himself one of the rescuers, I would agree that feral man would be a problem interesting, if true. On the scientifically inadequate data of the previous cases, I should have been more in agreement with Professor Dennis, would have reviewed the evidence as tempting, as did Dr. Ireland, whose work Professor Dennis mentions, but would have turned away from it in the end, as also Professor Dennis correctly quotes Tylor as doing. In my brief summary article I was unable to give space to fully review Tylor's curiously equivocal article which accepts as a probable feral case, one from the times of the conquest of Rome by the Visigoths, and presents Clemens and the other wild-boy of Overdyke and then equivocally concluded by rejecting the older classic cases, as though rejecting the whole, as Professor Dennis has fully quoted.

³¹ J. F. Blumenbach, Von Homo sapiens ferus Linn. und namentlich von Hamel'schen wilden Peter, *Beyträge z. Naturg.* 2, 1811, 10-44. Trans. and reprinted in *Life and Works of Blumenbach*, 1865, 325-340.

³² Zingg, More about the "Baboon Boy" of South Africa, this JOURNAL, 53, 1940, 455-462.

¹ R. M. Zingg, *op. cit.*, this JOURNAL, 53, 1940, 487-517.

² *Ibid.*, 496.

Incidentally, Professor Dennis makes a point that bear foster-mothers of humans are reported only from ancient Lithuania, neglecting Frazer's acceptance of reports of a recent bear-boy from India. The report is so poor that probably this case should be neglected. More striking and recent and better authenticated is the report from India in 1920 by E. C. Stuart Baker in a scientific journal of a leopard-child in India. This case should still be open for checking by some one in India.

Professor Dennis's article gives the arguments pro and con that the 31 cases cited as feral man are simply 31 idiots from the world's number of these unfortunates. One argument that Professor Dennis does not attack is mine based on the case of Dina Sanichar, an unquestioned idiot who lived from 1867-1895 in the European-staffed Sikandra Orphanage, where memory of him is still green enough for his picture and obituary to be sent me in 1938. That he was not only an idiot but also a wolf-child is indicated by unusually good records. When reports that a boy ran with wolves near-by were brought to an English magistrate, Mr. Lowe, he ordered the natives to smoke the wolves and the boy from their den. This was done and the child brought with the bodies of the wolves to the magistrate for payment of bounty. The magistrate ordered the child to be committed to the Sikandra Orphanage where, during his long domestication, he was visited by many people and many accounts of his idiotic behavior made. He was indubitably an idiot as these accounts and his picture clearly show. With this idiotic wolf-child for a comparison it seems easy to establish that the other well authenticated cases, like the wolf-children of Midnapore, were not.

If we must throw out the record of Dina Sanichar as unproven because the English magistrate in a hot climate did not go personally to smoke the boy out of the wolf's den, there remains the evidence of Rev. J. A. L. Singh, who gives a full and circumstantial account with names of other witnesses of the rescue of the two girls from the wolves. The girl's bodies gave evidence of long adaptation to quadruped locomotion, which at the age of especially the younger child, indicated long association with animals, in the thick pads on their knees and great toes which stood almost upright from adaptation to running on hands and feet. These features are also reported for other cases.

Publication of the full account of the Midnapore cases will do much to clarify some of these problems, now that three years of checking of the account by several scientists have not broken down any consequential features of it. It is appreciated that Professor Dennis does not try to do so, as some might be inclined to do, by simply saying that Rev. Singh is a liar anxious to exploit two idiots for his own profit. That would be grossly unfair to judge from my impression from correspondence for three years with Rev. Singh. Though an M.A. from Calcutta University, Rev. Singh is not a scientist of course, but need not on that account be called an incompetent observer. Observation by a scientist of so rare an event as the rescue of a child from animals is almost as remote as the scientific desideratum that a scientist observe feral man in his wild condition. Unfortunately there is no possible way in such cases for the scientist "to know the life events of any feral case prior to the time he came under observation," since no scientist or other observer can run with the animals—a limitation under which animal psychology also of necessity labors.

The only evidence available as to the life events of feral cases relative to the length of time of association with animals is either internal evidence, or the accounts of those few cases, unfortunately not perfectly authenticated, where previous marks on the childrens' bodies enable their identification. Professor Dennis, like Dr. Ireland, has done well in putting us on our guard against acceptance of any but the best authenticated of these Indian cases, because of the propensities of a mendicant class of Indians to exploit idiots or other unfortunates as fabulous. In India there is, furthermore, a myth of wolf-children in the cultural mileau.

A fact cannot be abolished, however, because a myth parallels it. Though anthropologists have long since lost interest in trying to find a factual basis for myths, it sometimes happens that a factual basis is discovered.³

Too great scientific caution obscures the truth as much as an attitude too lenient. We need not throw out the baby with the bath-water, as Rauber said in connection with feral man in 1885. Professor Dennis's commentary has not convinced me that there is no justification for testing the less well-authenticated cases of feral man by the *experimentum crucis* of the well-authenticated and full account we have of the wolf-children of Midnapore.

The complete account of the wolf-children of Midnapore, not yet available in published form, offers convincing evidence which opposes the following statements made by Professor Dennis. (a) The statement that "not a single account has been written of the fact that the child was captured in an animal den or in close company with an animal." (b) "I have tried to show that there is no good evidences that children have lived with animals." (c) "To say that they (these children) walk on all fours probably involves an element of interpretation in the direction of perceiving a likeness to animals. It would probably be most correct to state that many wild children cannot walk upright. . . . If they were forced to move about it is possible that they would go on all fours." We have photographic evidence as well as accurate verbal description by Rev. Singh to show that Kamala actually did run on all fours. (d) "The child may not have supported himself for more than a day or two before being found." (e) "Furthermore if a normal child were found he might not be classed as feral; he would give some account of himself." This could hardly have been expected of Amala, an approximately 18 month old child who from her physical state must be judged to have been living with the wolves for several months at least. (f) "Scientific caution would avoid accepting such an unwarranted assumption as the suggestion that infants may survive even if they become isolated before the third year. . . ." The case of Amala, photographically documented, requires no *assumption* that a child under three could survive when so isolated.

³ In India where the folk-belief in wolf-children, reflected in Kipling's story of Mowgli, has been brought into the question of feral man by Professor Dennis, Professor Linton, Chairman of the Department of Anthropology at Columbia, told me of a pertinent incident in the work of one of his students among the primitive Todas. This student had ran across the mythological motif: "Dog has saved child from serpent. Father sees bloody mouth thinks the dog has eaten the child, and kills dog." (Stith Thompson, Llewellyn and his dog, *Motiff Index of Folk-Literature*, I, 331). Among the Todas this is appropriately transformed into a story of the faithful mongoose. In checking this story from one village to another, the anthropologist found in one village that a woman had actually killed a mongoose under those circumstances, thus giving independent rise to the story.

In conclusion it should be evident that neither Professor Dennis nor the writer has any axe to grind in this matter. No one is attempting to prove a thesis; the interest is merely to report what happened. A three-year check of Rev. Singh's diary account of the rescue and lives of the wolf-children of Midnapore, leads the writer to believe that it is authentic.

University of Denver

ROBERT M. ZINGG

PERCEPTUAL RESEARCH IN THE UNITED STATES

The Sub-Committee on Perceptual Problems of the Emergency Committee in Psychology of the Division of Anthropology and Psychology of the National Research Council deemed it important, as one of its first steps, to survey the work being done in perception at the present time and to determine what institutions were equipped for research and the fields of their equipment and what

TABLE I
RESULTS OF 115 RESPONSES TO 164 QUESTIONNAIRES

Perceptual fields	Problems in progress	Institutions available	Persons available	Titles in bibliography
Vision	97	57	109	196
Audition	19	26	36	42
Kinesthesia	3	11	12	21
Cutaneous	3	8	12	22
Olfaction	1	2	1	7
Gustation	—	—	—	15
Vestibular	4	5	5	—
Organic	—	2	—	—
Miscellaneous	11	7	18	12
No field indicated	—	1	23	—
Totals	138	119*	216*	315

* Including duplications of institutions and personnel.

psychologists considered themselves competent to do research and the fields of their special competency. Hence, questionnaires were sent to 164 laboratories and departments of psychology in universities and colleges of the United States to gather this information. After more than three months, only 115 responses have been returned though the questionnaire was sent out in the name of the Committee and its purpose indicated in a covering letter. All of the important research institutions did, however, respond. It is not believed, therefore, that a reply from the remaining 49 departments would materially change the results.

These returns, analyzed to show research in progress relating to perception are given in the accompanying table. The results are of interest in emphasizing the relatively large amount of work being done and the relatively large number of institutions and persons available for research on problems of vision as compared with research in other modalities. These results will be found in the first line of the first three columns of the table. Results for the other modalities will be found below in the same columns. Audition is fairly well represented, but a very bad second to vision in all three respects listed: *i.e.* problems in progress, and institutions and personnel available. Except for vision and audition, and possibly kines-

thesis, the available pool which one could tap for research facilities in the other fields seems to be surprisingly small. It is interesting to note that gustation is not represented in any one of the three columns.

Obviously there are many duplications in the second and third columns which have to do with available institutions and personnel. The total of the second column, 119, is the sum of reports from only 67 laboratories. Similarly, the total of the third column, 216, is the sum of reports from only 186 persons. This number is only 6.4% of the membership of the American Psychological Association! A total of 30 institutions, which returned the questionnaire, reported neither research in progress, nor laboratory facilities, nor available competent personnel.

One other completed function of the Sub-Committee on Perceptual Problems was to prepare a statement of the important basic research in the perceptual fields and to indicate important practical applications of perceptual data to military situations. Each of the nine members of the Sub-Committee prepared this highly selected bibliography and the descriptive comment in the area of his particular competency, except for vestibular phenomena which was the function of another group interested in aviation.¹ One may believe that such a group of specialists, everyone of whom has made considerable contribution to the experimental literature of his particular area, would include all highly important papers. A bibliography of 315 titles for the entire field of perception, except for vestibular phenomena, resulted from the collation of the work of the several men.

The analysis of these titles by fields will be found in the last column of the table. The results indicate values very similar to those for the other three columns. Almost two-thirds of the titles are concerned with vision; audition is a poor second and the number of titles in the other fields is very much less.

One further comment upon this bibliography may be of interest. Of the 315 titles reported, 171 (54.2%) represent reports of American work, 88 (28%) came from Germany, and 37 (11.8%) are British. All of the other countries combined are represented by only 19 titles (6% of the total) as follows: Japan, 5; Italy, 4; Russia, 4; France, 3; Scandinavia, 2; and Spain, 1.

A consideration of all of these facts would seem to indicate that much work in the field of perception has been done in America and also that, in the United States, there exists adequate facilities and a considerable number of competent investigators for the study of a great variety of visual and auditory problems but a much less adequate basis—both with respect to material and personnel—for research in the other perceptual fields.

University of Pennsylvania

S. W. FERNBERGER

PSYCHOLOGY IN JUNIOR COLLEGES

President J. C. Miller of the American Association of Junior Colleges announces the appointment of a Committee on Psychology in Junior Colleges which is to make an intensive study of the teaching of psychology in the 625 junior colleges in the country. This Committee is an outgrowth of a conference called by the American Association for Applied Psychology at Atlantic City in February to discuss instruction in psychology on the high school and junior college levels.

Dr. Louise Omwake of Centenary Junior College, Hackettstown, N.J., is the chairman of the Committee. The other members are: Miss Maybelle Blake, Brad-

¹ S. W. Fernberger, Perception, *Psychol. Bull.*, 38, 1941, 432-468.

ford Junior College, Bradford, Mass.; A. G. Breidenstine, Hershey Junior College, Hershey, Pa.; Mrs. Florence M. Johnson, Schuylkill Undergraduate Center, Pottsville, Pa.; Adolph M. Koch, Essex Junior College, Newark, N.J.; Benjamin Burack, Carl Schurz Evening Junior College, Chicago, Ill.; Clayton Gerken, Rochester Junior College, Rochester, Minn.; W. A. Owings, Textile Industrial Institute, Spartanburg, S.C.; I. W. Stam, Northern Montana College, Havre, Mont.; and Henry T. Tyler, Sacramento Junior College, Sacramento, Calif.

The Committee, in accordance with the aims of many junior colleges, *i.e.* to prepare students as more useful citizens in their own communities, will concentrate its work on the various phases of applied psychology. Subjects recommended for study as useful to the citizen in his community include child psychology; industrial and business psychology; psychology of social relations, politics, propaganda, and race; attitudes and information concerning the mentally defective, insane and criminal as well as the gifted.

The Committee will report its progress at the annual meeting of the American Association of Junior Colleges in February, 1942.

K. M. D.

A DEMONSTRATION OF INSIGHT: THE HORSE-AND-RIDER PUZZLE

The horse and rider puzzle furnishes an excellent demonstration of insight. Goldstein's attention was called to this puzzle by Sch  erer, into whose hands it first

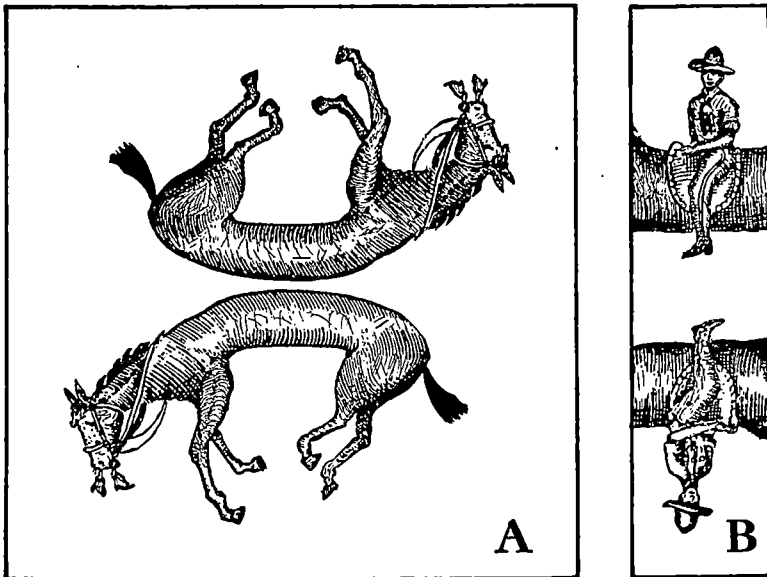


FIG. 1. THE HORSE-AND-RIDER PUZZLE

came as an advertisement. They, in their collaboration on the problem of shifting in perception and the thought processes, have used the puzzle in their clinical work and teaching. Later Boring introduced the puzzle with great success for demon-

stration in the elementary course at Harvard, employing photostatic enlargements of the two pieces in Fig. 1, with the large piece about 28 in. square.

The difficulty of solution seems to arise from the persistent pattern-organization of a concrete object. The whole-structure of the object presents a strong resistance with respect to the required abstracting process of shift, by means of which the natural aspect of the object-configuration and of its members must be disregarded and subsequently reorganized.

The square with the two horses, A in Fig. 1, and the oblong with the two riders, B, are cut out separately and S is required to place the two riders on the

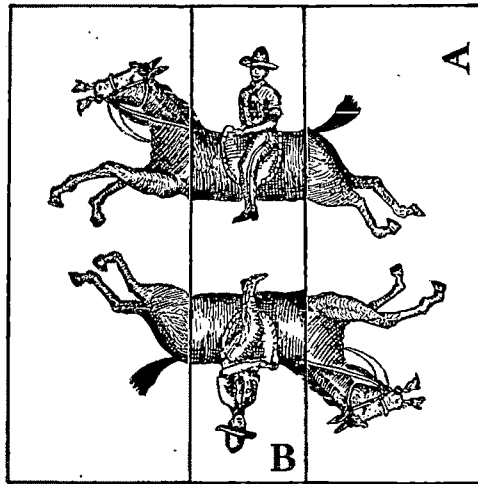


FIG. 2. THE PUZZLE SOLVED

horses accurately without bending or mutilating the two pieces. The task seems impossible of solution. The solution is, however, easy and the result is shown on this page in Fig. 2, which was made by photographing B superimposed upon A. The solution requires, of course, that A should be rotated in either direction through 90° with respect to B, or B with respect to A. The perceptual determinants are, however, so strong that even a person thoroughly familiar with the nature of the puzzle often finds himself still hesitating to believe, as he manipulates the pieces, in the validity of the solution until the final moment of resolution.

Another procedure, which Scheerer has found useful in experimental work, is to cut the two horses apart in A and to give the S three pieces to put together. This procedure is very striking when the three pieces are presented to the S, for example, by holding them between the finger-tips of both hands. Then the combination is broken up, out of the S's sight, and the S is asked to reconstruct from its component parts what he has just seen.

Columbia University
Tufts College
Harvard University

MARTIN SCHEERER
KURT GOLDSTEIN
EDWIN G. BORING

THE 'BEST' PSYCHOLOGISTS

Science Service has asked past-presidents of the American Psychological Association to name the six living psychologists in the United States whom they "consider to be the best" or "the most outstanding." The request, made "as a service to science and for the information of the public," suggests the following comment by a past-president.

The selection of the 'best' half-dozen psychologists calls for an attempt to compare the incommensurable by invoking the powers of the wise judge. This simple device for securing significant opinions by 'rating' was used long ago by Francis Galton in the pioneering days of the questionnaire; but Galton would, as I fear, be shocked at its subsequent exploitation by educators and some others.

In the first place, the 'judges'—in the case in hand—are themselves incomparable with respect to the task because their estimations will have been really determined at various times during the last half-century, while the standards and qualities of excellence have been profoundly changing and the age-relations of the judges to the men judged have been changing too.

In the second place, the judgments rendered in terms of 'outstanding' = 'best' are not either comparable since the meaning of the rating terms adopted by the several judges (variously based upon quality, amount or advertisement of research, writing, publication, teaching, public service, and so on) is unknown and uncontrolled. In addition, the word 'outstanding' strongly savors of repute while 'best' has another connotation.

Finally, the professions of men and women called 'psychologist' are not invariably commensurate. Individuals who might well enter the consideration of the judges may hold so little in common, with respect to training, career or accomplishment, that no single denominator is left for them save only an unspecified interest in something like 'human nature,' 'behavior,' or—for the moment—'personality.'

All of these factors of uncertainty and confusion confront the learned judge as he attempts the delicate task of setting a handful of his fellows on high in their 'profession.' Do not reproach him if he finds, in the midst of ambiguities and indeterminables, that the term 'best psychologist' has for him no intelligible meaning. Fairer than reproach would be the tempering of judgment by the quality of mercy should he return to the professional popularizers the incongruous tasks of 'serving science' and "informing the public."

Goucher College

M. B.

THE THIRTY-SIXTH ANNUAL MEETING OF THE SOUTHERN SOCIETY
FOR PHILOSOPHY AND PSYCHOLOGY

The thirty-sixth annual meeting of the Southern Society for Philosophy and Psychology was held in Washington, D.C., on April 10-12, 1941, with the University of Maryland as host institution. The Council of the Society met in executive session on the evening of April 10 with John Paul Nafe presiding. Three sessions in psychology and three in philosophy were held on the following day. A joint session on philosophy and psychology was held the morning of April 12. This was followed by the annual business meeting.

The initial psychology session of which John F. Dashiell served as chairman,

comprised the following papers: A reciprocal relationship between sensory and attentional factors in reaction-time (V. Coucheron Jarl); Individual differences in fusion frequency as an indicator of visual sensitivity (F. G. Tice); Cold sensitivity and its relation to the neurovascular mechanisms and other structures of the skin (B. von Haller Gilmer); The effect of skin temperature on cutaneous pain (Joseph Weitz); and Imagination (Emily S. Dexter).

John G. Jenkins was chairman of the second session on psychology. Papers read were: Factor analysis of oral group rational learning ability (Henry F. Dickenson); An evaluation of sleep motility criteria (M. M. Jackson); A study of the ability to awaken at assigned hours (J. H. Elder); Sleep motility in student pilots (F. A. Geldard and H. H. Manchester, Jr.); Pre-reward and post-reward performance in the 'latent learning' of an elevated maze (S. Rains Wallace, Jr., Morris G. Blackwell, Jr., and Iredell Jenkins); Analysis of Bernreuter's inventory as a predictor of success in certain vocations: A problem in scientific method (H. M. Johnson); and The University of Tennessee psychological clinic (Paul M. Fitts).

A third psychology session under the chairmanship of Thelma Hunt included the following papers: The socio-economic status of the homes of mentally superior and retarded children and the occupational rank of their parents (William McGehee and W. D. Lewis); The social psychology of World War II (Steuart Henderson Britt); A psychological study of voice recognition (Frances McGehee); The relation between subjective estimates of quality of certain food products and the cost of such products (Roy M. Dorcus and Jane Leeds); Relations between dominance and non-competitive behavior in female chimpanzees (Meredith P. Crawford); The significance of feral man (Wayne Dennis); and Some psychological aspects of yawning (Joe E. Moore).

Fritz Marti served as chairman of the initial session in Philosophy. Papers read were as follows: Logical classes and universals (Lewis M. Hammond); Evolutionary dialectic rationalism (Louis O. Kattssoff); Glanvill's appreciation of the method of doubt (James Albert Pait); The meaning of 'becoming' in Plato (W. C. Truehart); Individualism *vs.* individuality (George Morgan, Jr.); Universals and the philosophy of religion (Robert Leet Patterson).

The second philosophy session, which was under the chairmanship of William P. Warren, comprised the following papers: Concerning philosophy in Argentina (Marjorie S. Harris); Is history a science? (Leonard J. Eslick); The purge in National-socialist philosophy (Marten ten Hoor); The concept of peace (William S. Weedon); and On the attempt to bring rational control into the sphere of moral judgment (Robert W. Browning).

Christopher Browne Garnett was chairman of the final philosophy session. The papers read were: Functional realism in value theory (W. Preston Warren); *De Gustibus non est Disputandum* (John Ladd); Hippolyte Taine and the background of modern esthetics (Iredell Jenkins); Barbarism, primitivism, intellectualism, and art (Fritz Marti); The perfectionism of personalistic ethics (Edward Thomas Ramsdell); Platonic pragmatism (Helmuth Kuhn); Creative experience in science and art (Max Schoen).

The annual banquet of the Society was held on the evening of April 11 with 103 present. After an address of welcome by President Byrd of the University of Maryland, President John Paul Nafe delivered his address entitled "The quantifi-

cation of psychology." This address was followed by refreshments provided by the University of Maryland.

The joint session in philosophy and psychology, which was presided over by John Paul Nafe, met on the morning of April 12. Five papers were read. The first two were by invitation of the Society. They had the following titles. Philosophy and psychology: Philosophy in the Southern Society, 1906-1941 (A. G. A. Balz); and Ten years after twenty-five years in the Southern Society for Philosophy and Psychology (James Burt Miner). Other papers were: The meeting of philosophy and psychology (George Boas); Contemporary arguments for mind energy (D. Maurice Allan); and A genetic psychological concept of value (Axel Brett).

The annual business meeting convened immediately after conclusion of the joint session. Fritz Marti of the University of Maryland was elected president of the Society and Wayne Dennis and Harold N. Lee were elected to the Council for a three year term. Herbert C. Sanborn, in recognition of his long and faithful service to the Society, was elected an honorary member of the Council for life. Nineteen new members were admitted to the Society. These comprised eight philosophers and eleven psychologists. The Society voted to accept the joint invitation of Vanderbilt University and Peabody College to hold the next meeting in Nashville on April 2-4, 1942.

Vanderbilt University

NORMAN L. MUNN

THE TWELFTH ANNUAL MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The twelfth annual meeting of the Eastern Psychological Association was held at Brooklyn College, on April 18-19, 1941, with official headquarters at the Hotel St. George. The total attendance was 633 of whom 275 were guests. Seventy-three applicants for membership were elected, bringing the total membership of the Association to 788. The total income at the close of the fiscal year amounted to \$936.47, leaving a surplus for the year of more than \$500.00, the largest in the history of the Association. The large surplus was due primarily to the fact that the guest fee was raised from twenty-five cents to one dollar.

This year, seventy-three papers were contributed in eleven sessions devoted to Social Psychology, Conditioning, Sensory Processes, Rorschach Methods, Physiological Psychology, Learning and Memory, Personality and Abnormal Psychology, Auditory Processes, Personality, Brain Functions, and Educational Psychology. Three Round Tables were held on: Current Problems in the Rorschach Method of Personality Diagnosis; Level of Aspiration; and Formation and Change of Opinions and Attitudes. Three films were shown in a special Film Session entitled: Testing the IQ; Color Vision; and Problems of Inter-Sensory Relations. A feature of the meeting was the Employer-Employee Conference under the Chairmanship of Gardner Murphy. This conference was held for the purpose of discussing opportunities for employment and to bring together employers and candidates for positions. It was attended by over 200 individuals who were addressed by several speakers. Two important actions resulted from this conference. The first consisted of smaller informal conferences between candidates and employers, and the second was the expression of the sense of those attending that

the Association take steps to investigate the feasibility of establishing a clearing-house of information regarding opportunities for employment and qualifications of candidates. This second action was responsible in large part for the passage of a motion embodying this proposal at the Business Meeting the following day.

The annual dinner was held Friday evening in the dining hall of Boylan Hall. It was followed by an address of welcome to the Association by President Harry Gideonse of Brooklyn College, and the presidential address, "On the Professional Training of Psychologists" by Walter S. Hunter of Brown University. After the address the members of the Association were entertained at an informal gathering in the Lounge of the Library.

Several important actions were taken at the Business Meeting besides the routine acceptance of reports by the Secretary-Treasurer. It was voted to confirm the action taken by the Board in affiliating the Association with the American Association for the Advancement of Science and two representatives to its Council were elected: K. M. Dallenbach for a three-year term, 1941-44, and W. S. Hunter for a two-year term, 1941-43. The division of the office of Secretary-Treasurer into two offices was voted as a change in the by-laws. Other changes in the by-laws concern more specific mention of the territory covered by the Association, and provision for continuance of membership in the Association by application to the Board after a member has left the territory. The budget voted for the coming year included the sum of \$100.00 to be used for the purpose of a canvass by the Secretary of membership sentiment regarding plans for a clearing-house of information on candidates and jobs. The Association voted to hold the 1942 Annual Meetings at the Hotel Biltmore, Providence, Rhode Island, at the invitation of Brown University on April 17, and 18, and to hold the 1943 Meetings at Lehigh University, Bethlehem, Pennsylvania. Several other actions by the Board and by the members should be mentioned. A charge of ten cents will hereafter be made to those requesting programs in addition to those sent out in the mail. A motion by Kurt Koffka to send greetings to the British Psychological Society meeting in Nottingham at the time of the Brooklyn Meetings was unanimously carried. In order to improve the quality of contributed papers, it was voted that acceptance of the report of the Program Committee in which it was recommended that it be given power to reject abstracts constituted authority for such action.

Elections and appointments were as follows: Gardner Murphy, of the College of the City of New York, President, 1941-42; Lyle H. Lanier, Vassar College, Treasurer, 1941-44; Leonard Carmichael, Tufts College, and Edna Heidbreder, Wellesley College, Directors, 1941-44; third member of the Program Committee, J. J. Gibson, Smith College, 1941-44; representatives of the Association to the Fordham Centenary, A. T. Poffenberger, Columbia University, and Douglas Fryer, New York University; Auditing Committee to audit the financial records for 1940-41, H. S. Oberly, University of Pennsylvania, and R. B. MacLeod, Swarthmore College.

The General Session included three papers, one replacing the paper announced by Adelbert Ames who was unable to appear: Brain Areas Involved in the Reproductive Behavior of Vertebrates by Frank Beach, American Museum of Natural History; The Constitutional Basis of Temperament, William Sheldon, Harvard University; The Relationships between Anthropology and Psychology, Ralph Lin-

ton, Columbia University. After the General Session, an informal tea was served in the Library Lounge as the final event of the 1941 meetings. The opinion was unanimous that facilities for meetings, registration, and dining were superlative. The Local Committee, under the Chairmanship of Dr. Edward Girden, attended to every detail down to the smallest without a slip and earned the gratitude of everyone present.

By setting April 17-18 as the dates for the 1942 meetings the Board hoped that many would be able to attend by driving to Providence. For those who will not drive, it can be said that the Providence Biltmore Hotel where the meetings are to be held is within a block of railroad and bus facilities. We look forward to a well-attended meeting as the Association goes for the first time in its history to New England.

Bryn Mawr College

HARRY HELSON

THE SIXTEENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association held its sixteenth annual meeting at Ohio University in Athens, Friday and Saturday, April 11-12, 1941. The number of registered members and visitors was 358.

At the annual business meeting on Friday afternoon, 75 new members were elected, bringing the present membership list to 555. The election of officers was announced with James P. Porter, Ohio University, president for the year 1941-1942; and Dael L. Wolfe, University of Chicago, council member for the years 1941-1944. It was announced by the Council that the next annual meeting of the Association will be held at Washington University (St. Louis, Missouri), probably on Friday and Saturday, May 1-2, 1942.

Eleven sessions were held at which 71 scheduled papers were read. The sessions included those on experimental studies in personality, measures of personality, psychological tests, psychometrics, clinical psychology, social psychology, human learning, conditioning, animal behavior, physiological psychology, and industrial psychology. In addition, 10 symposia were conducted on Freudian mechanisms and frustration, contemporary social problems (morale), the relation of experimental psychology to clinical problems, "research I'd like to do in child development," learning as related to need and the subsequent motivation of such learned behavior, conditioning, the first course in psychology, business and industrial psychology, reports from the laboratories, and psychology and the national defense. Forty-six persons contributed to the formal portions of the symposia.

At the annual banquet held on Friday evening, E. S. Conklin presided in the absence of J. P. Guilford, the preceding president. Dean T. C. McCracken, Provost of Ohio University, gave the address of welcome. Elmer A. Culler of the University of Rochester gave the presidential address "On the mechanism of the acoustic analyzer," a critical review and summary of significant research on the functioning of the ear.

A resolution passed at the business meeting requested the Secretary to thank the Administration of Ohio University, the host department, and members of the local committee headed by Professor James P. Porter for the excellent facilities and entertainment provided at these meetings.

Northwestern University

ROBERT H. SEASHORE

Henry Head: 1861-1940

Sir Henry Head, who died in England on October 8, 1940, was a neurologist whose work was well known to psychologists. His two great fields of research were skin sensitivity and language and its disorders, both fields of primary significance for psychology. In both, his studies from the pathological angle contributed new insight and new data on crucial psychological questions.

Born August 4, 1861, Head was educated at the Charterhouse and then at Trinity College, Cambridge. He studied too at the University of Halle and with Hering at the German University of Prague. From Prague he returned to Cambridge, where he took his M.D. in 1892. His next thirty years were very active in clinical medicine as well as in research. He was physician to the London Hospital; he published the series of articles on sensation, and his influence on neurology was extended further by the post as editor of *Brain* which he held from 1905 to 1922. He was knighted in 1927, the year after the publication of his two volumes on *Aphasia and Kindred Disorders of Speech*.

In his preface to these books he notes that he was much interested in aphasia as early as his student days, but it was his work on sensation which absorbed the greater part of his attention to the time of the first World War; and it was this work which first stirred psychological controversy. In a paper written in 1905 with W. H. R. Rivers and J. Sherren, he began his report on peripheral nerve injuries and proposed his distinction of two forms of cutaneous sensitivity, the protopathic and the epicritic. The theory was based on the experiments on the sensitivity of his own forearm after division of the radial and external cutaneous nerves at the elbow, experiments to which he and his colleagues resorted because of the unsatisfactory nature of patients' reports and the obvious need for the judgment of a trained observer. Immediate checks on the work, notably by Trotter and Davies, Von Frey, and Boring, substantiated many of the facts Head had reported but raised great question as to their interpretation. The forms of protopathic and epicritic sensibility were not generally accepted, but the sharp new theoretical formulation Head thus projected against the experimental data stimulated new work. Head himself followed his early reports with studies of sensory disturbances in cord lesions and in lesions of the cerebral cortex. These he included with the research on peripheral nerve injuries in the two-volume work, *Studies in Neurology*, which he published in 1920. For the higher as for the peripheral levels he found evidence of functional hierarchies in nervous organization such as Hughlings Jackson had proposed. Damage to the cerebral cortex was shown to lead to sensation of a more "thalamic character" as removal of epicritic sensitivity exposed the activity of the protopathic.

From 1910 on Head concerned himself increasingly in the problems of aphasia. With the onset of the War in 1914, large numbers of cases of head injuries with aphasia as well as with motor and sensory loss presented themselves for examination; and it was on the basis of these cases that Head carried out the studies reported in *Aphasia and Kindred Disorders of Speech*. This work is a great landmark in the progress of knowledge on aphasia. It opens with the most vigorous and best account available of the nineteenth century work on localization of function in the cerebral cortex. No reader will soon forget Head's chapters on Gall's struggle for knowledge about cerebral localization, on Broca's demonstra-

tions before the *Société anatomique de Paris* of lesions in the left third frontal convolutions in aphasic patients, or of the "diagram makers" who were more interested, according to Head, in finding a case to fit the hypothetical diagram of cerebral connections and sites than in studying the case in an unprejudiced fashion.

Head's understanding of aphasia took its departure from his hypothesis that it was a disorder of symbolic formulation and expression, a hypothesis closely akin to Jackson's concept of aphasia as a disorder in propositionizing. Head did not exclude the possibility that the disorder might extend beyond language, but, on the whole, it is fair to say that his theory stands between the classic doctrine based on loss of images—motor, auditory, or visual—or of language deficit and theories, such as Goldstein's, which relate most types of aphasia to a more general change of mental functioning, that is, one which goes far beyond language.

Head departed from the classic doctrine too in his concept of types of aphasia. He rejected the terms *motor* and *sensory*, believing that language is an integrated function which can not be analyzed in terms of motion and sensation. He used instead terms descriptive of what he believed to be the predominant language disintegrations shown: the verbal, the syntactical, the nominal, and the semantic. The verbal form corresponds to motor or expressive aphasia. The nominal is like the amnesic. The other two forms represent various aspects of the disorders usually termed sensory or receptive. According to the judgment of other investigators these two do not stand out as clear-cut types, nor is the classification itself satisfactory for many of the cases encountered in general practice.

From these aphasic patients Head secured relatively few autopsies; he had to resort to determining localization by operative records, X-ray photographs, and reconstruction on cadavers. He was therefore able to present little new or incontestible evidence. This was a serious handicap, but despite the handicap—and despite the fact that his classification has not been accepted—the work is of tremendous importance, both for the acuteness of his analysis of various aspects of aphasic disorders, and for the wealth of clinical material presented in the two volumes.

In many respects his two works, on sensation and on aphasia, show similar interests and emphases. Both were prefaced by careful development of systematic test-batteries, through which Head hoped to take care of the complexity and variability in performance which he recognized in both sensibility and language. Both attempted to follow Jackson's "golden rule," to put down in the record what the patient did and avoid blanket terms. Both revealed Jackson's influence to an extraordinary degree, especially in the strong functional point of view and the concept of neural hierarchies, with the final performance reflecting not only the loss which comes as a result of destructive lesion but also the activity of lower centers. Head was clear in his evaluation of Jackson's influence on his work, but it should be said also that Head carried his systematic studies beyond the stages his great teacher had reached and produced a body of work which in its turn stimulated thought and new research.

These two publications were Head's only scientific books, but in 1919 he published a little collection of verses, called *Destroyers*. Some of the verses, mostly lighter ones, were written at a much earlier age. Those written during the War

show hope but not confidence. One is a lament for forced inaction and life which goes on beyond youth and strength; it reflected directly only Head's desire to take an active part in the War, but one reads it today with sorrow for it was also his fate to live beyond the time when he could continue his work. He has left many challenging problems to which other investigators may well turn.

Radcliffe College

K. E. McBRIDE

Frederick Kuhlmann: 1876-1941

Frederick Kuhlmann, Director of the Division of Research of the Minnesota State Department of Public Institutions, died in his sixty-sixth year on April 19, 1941, at St. Paul, Minnesota. He was born at Davenport, Iowa, on March 20, 1876.

Dr. Kuhlmann received his education under H. K. Wolfe at the University of Nebraska (A.B., 1899; fellow, 1899-1901; M.A., 1901) and under G. Stanley Hall and E. C. Sanford at Clark University (fellow, 1901-1903; Ph.D., 1903). He was lecturer at Clark from 1903-1907, and at the University of Wisconsin from 1905-1906 (on leave from Clark); instructor at the University of Illinois from 1907-1910; and director of the Research Bureau of the Minnesota School for the Feeble-minded at Faribault, 1910-1921. In 1921 he began in the position which he held until his death. At the time of his death he was vice-president and chairman of the clinical section of the American Association of Applied Psychology.

Dr. Kuhlmann was a thorough investigator, a friendly and loyal colleague, and a kindly disposed and tactful administrator. The writer remembers his courses in memory and abnormal psychology at Illinois as outstanding in content, organization, and careful preparation. He lectured while seated, seldom lifting his eyes from his notes. He was reserved and austere with students but when those barriers were passed a kind and friendly man was revealed.

Dr. Kuhlmann's doctoral dissertation, "Experimental studies in mental deficiency,"¹ done under Sanford's direction, set his interests for life. It led him first to the problems of mental imagery and memory; and his studies on those subjects are numbered among the most significant in those fields.²

In 1912, he published a version of the Binet-Simon scale adapted to American use;³ and in 1918, a new version extended downward to the levels of infancy;⁴ in 1922, a handbook on mental tests;⁵ in 1924, an outline of mental deficiency

¹ *Op. cit.*, this JOURNAL, 15, 1904, 391-446.

² The place of mental imagery and memory among mental functions, this JOURNAL, 16, 1905, 337-356; Recent studies of normal illusions of memory, *ibid.*, 16, 1905, 389-398; On the analysis of the memory consciousness, *Psychol. Rev.*, 13, 1906, 316-348; On the analysis of the memory consciousness for pictures of familiar objects, this JOURNAL, 18, 1907, 389-420; Problems in the analysis of the memory consciousness, *J. Philos., Psychol. & Sci. Methods*, 4, 1907, 5-14; The present status of memory investigation, *Psychol. Bull.*, 5, 1908, 285-293; On the analysis of auditory memory consciousness, this JOURNAL, 20, 1909, 194-218; A new memory apparatus, *Psychol. Rev.*, 1912, 73-78.

³ A revision of the Binet-Simon system for measuring the intelligence of children, *J. Psycho-Assthen., Monog. Suppl.*, 1, 1912, 1-41.

⁴ A further extension and revision of the Binet-Simon scale, *J. Crim. Law & Criminol.*, 8, 1918, 890-901.

⁵ *Handbook of Mental Tests*, 1922, 1-244.

for social workers and teachers;⁶ in 1927, in coöperation with Rose G. Anderson, the widely known Kuhlmann-Anderson intelligence tests (revised in 1930);⁷ and in 1938 and 1939, a new scale of individual intelligence tests, with complete test-outfit.⁸ In addition to these major works he published numerous other articles and reports on mental deficiency and intelligence testing.

K. M. D.

⁶ *Outline of Mental Deficiency*, 1925, 1-80.

⁷ *Kuhlmann-Anderson Intelligence Tests*, rev. ed., 1930.

⁸ A new scale of intelligence tests with some new measures, *Proc. Amer. Assn. Ment. Def.*, 43, 1938, 47-55; *Tests of Mental Development*, 1939, 1-314.

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

Human Nature and the Social Order. By E. L. THORNDIKE. New York, Macmillan Co., 1940. Pp. xx, 1019.

Nearly forty years ago Charles Horton Cooley, the social psychologist, published a volume by the same title as the present one. I doubt that Professor Thorndike ever read Cooley. At any rate the two books differ considerably. Cooley's is full of insight into the subtleties of human nature, full of knowledge of society and culture, and full of faith in the democratic world which he envisaged. Thorndike's reveals a distinctly quantitative standpoint, discusses highly specific traits and motives with only a modicum of appreciation of social-cultural factors, and exposes a projection of wishful thinking as to the future society in terms of biologically determined differences among individuals.

In 1902, when Cooley's work was printed, there was scarcely an academic psychologist, except G. Stanley Hall, John Dewey, and William James, who deigned to consider everyday human and social affairs as the proper topic of professional concern. Today psychologists, including the experimentalists, are blossoming out with discussions of societal problems on all fronts. Everyone, of course, takes up his new interest in terms of his own apperceptive mass, and one of the most recent converts to this 'new love' is the author of the work under review. Moreover, the present volume stems from a normative standpoint which would have been taboo in academic psychology twenty years ago. In his Preface, Thorndike expresses the hope that the "knowledge of psychology and of its applications to welfare would prevent, or at least diminish, some of the errors and calamities for which the well-intentioned have been and are responsible."

The treatment is divided into two large sections: Part I, in 15 chapters, summarizes and extends the whole scope of Thorndike's doctrines regarding abilities, wants, propensities, achievements, and valuations, and the means of measuring the same. As everyone knows this is a psychology of the individual and there is practically no reference to the processes of interaction and no sufficient recognition of the place of social-cultural conditioning in the rise of the personality. In terms of his concept of specificity and of individual differences the author builds a platform from which he examines, in Part II (23 chapters), a mass of concrete social problems. This covers an enormous range of materials on "special facts, principles, and applications." These include such diverse matters as "human nature and the science of philanthropy," the economics of wealth, supply and demand, labor and management, buying and selling, money and credit, ownership, and "the psychology of capitalism and alternative economic systems." There are also chapters on politics, leadership, law, and social reform.

Thorndike attempts to set up certain criteria in order to measure the adequacy or inadequacy of the civic, political, and economic behavior of individuals in our western society. In so doing, he moves back and forth with apparent ease from fairly objective to highly subjective judgments. The reader is often at a loss to know how he arrived at particular standards. The pages are full of discussion of the "goodness" of this societal datum or the "badness" of that.

He writes of the "good traits" of cities, or of the "goodness" of capitalism in the same vein in which three decades ago he wrote about "good" or "bad" handwriting among American school children. He comments at length about things being "better" or "worse," of people being "decent" or "foolish" with the assumption that these represent universally acceptable frames of analysis.

Interspersed with these are discussions of the statistics of economic conditions and of enlightening correlations among phenomena having to do with education, feeble-mindedness, birth and death rates, crime rates, and the like. It is just this admixture of interpretations rooted in judgments reflecting personal and class views and those growing out of more objectively determinable factors which make it difficult to evaluate this volume. The reviewer wonders what the professional economist would say of Thorndike's critique of managed currency or of his views as to the function of credit, or what the student of politics would say as to his utopia about college presidents and large entrepreneurs controlling, or at least helping to control, the political state. So, too, moralists and students of social change have long wrangled over the sort of items which he lists as making for the "good life." Most of these are phrased in terms of his individual psychology and many of them distinctly reflect our own particular cultural standards.

It is just in this neglect of social-cultural frames of reference that the students of society may easily overlook some of the larger contributions of this volume, for in spite of these limitations this book has considerable merit. First, Thorndike's stress on the biological, *i.e.* the inherent basis of wants and abilities, should serve as a counteractant in this day of over-emphasis on the environment, especially the cultural. Secondly, his insistence on the importance of individual differences is sound in view of the tendency to equate personality and social behavior with the common and general cultural characteristics to the neglect of the unique and specific in man. Thirdly, his critical psychological comments should stimulate a reconsideration of many of our traditional economic and political theories, to the profit, we hope, both of theory and practice.

Queens College

KIMBALL YOUNG

Extra-Sensory Perception after Sixty Years. By J. B. RHINE, J. G. PRATT, C. E. STUART, B. M. SMITH, and J. A. GREENWOOD. New York, Henry Holt & Co., 1940. Pp. xiv, 463.

According to the Preface, this book was written in order to furnish a comprehensive survey of recent ESP experiments. Such a review our authors believe should be useful to experimental psychologists and other scientists, potential investigators of extra-sensory phenomena, and to the public generally. Probably because of its various objectives, the book tends to be oversimplified in spots and fairly technical in others, as though the authors were never quite sure to which audience they should address themselves.

Even a cursory comparison of this volume with Rhine's 1934 monograph, "Extra-Sensory Perception," will show how far this peculiarly Duke enterprise has come in six years. The 1934 book was badly printed, badly written, and amateurish as regards methodology. Experimental conditions were poorly described, and the tables and text hard to follow. One gained the distinct impression, too, that Rhine believed firmly in ESP and would undoubtedly find it. The present volume is very different from its somewhat bedraggled predecessor. It is well and carefully written; it

bristles with mathematical formulas; and it makes a genuine attempt to present an unbiased picture of a controversy not always marked by sweetness and light.

Our authors pose as their primary problem the question of whether ESP does or does not exist. By way of introduction, they provide in Part I an account of various experiments on ESP. This is followed by a discussion of 35 "counter-hypotheses," that is, plausible explanations of extra-chance results which have been offered at various times in lieu of ESP. Six experiments are then assembled for which none of these hypotheses—singly or in combination—is, in our authors' opinion, a sufficient explanation.

Part II contains a review of criticisms previously leveled at ESP work as well as comments, favorable and unfavorable, from erstwhile critics who were invited to contribute to this volume. The last two parts of the book discuss the nature of ESP (its existence is assumed to have been proved); and several other problems such as the distribution of ESP in the general population, and the effects upon it of certain physical and psychological conditions.

Have the Duke experimenters proved the case for ESP? A recent review of this book by a writer on scientific subjects (himself not a psychologist) ends as follows: "the case for ESP is as good as a scientific case can be." There is no doubt but that the Duke workers consider their case proved. It must be admitted that the data which they present, and the arguments deduced therefrom, strike one as extremely plausible. I must confess, however, that I am still unconvinced that the evidence so far produced is really crucial, although I do not deny that the problem of extra-sensory perception is itself an important one. My arguments, or better comments, may best be assembled under several heads.

(1) What constitutes proof of the ESP hypothesis? The experimental unit in most of the Duke work is a pack of 25 cards consisting of five 'suits.' The five cards in each suit bear a simple drawing—circle, star, square, wavy lines, cross. Now suppose that in 1000 trials, the number of 'hits' is 260, or an average of 6.5 per pack. The expectation, on the basis of a purely chance hypothesis, is 200 hits or an average of 5.0 per pack. Such a result—which is statistically significant—does *not*, of course, establish the existence of ESP, though many uncritical people seem to think so. When observed data are compared with data to be expected on the basis of chance (for example, when the numbers of heads and tails which actually appear when five coins are tossed many times are compared with the expected numbers) the hypothesis under test is the "null" hypothesis. As R. A. Fisher has shown, a null hypothesis can be refuted or disproved, but it cannot be proved. If, therefore, the facts refute the null hypothesis—as in the above example—the next step is to look for reasons why this is so. ESP is *one* possible explanation; others, more probable a priori, are uncorrected errors or loose conditions in the experimental set-up, selection or distortion of data, errors in recording results, unnoticed sensory cues provided by the material, the apparatus, or the experimenter, tricks or "systems" of calling the cards, phenomenal memory for the cards called, bad shuffling or streaks of "luck" as shown by card runs, and many others. Now ESP, like any other scientific hypothesis, must be established inductively. That is to say, it must be shown that extra chance results are obtained in repeated trials when *all* other possible explanations have been eliminated. If only ESP is left, it then becomes the most likely hypothesis, and as such is established.

The Duke investigators are fully cognizant of the inductive nature of their problem. They write: "if there is *some* of the evidence which remains inexplicable by *all* of the hypotheses, taken singly and jointly, the hypothesis under investigation is thereby established by this fact alone" (p. 154). It has been contended by another reviewer that this statement is valid *only* when the experimenter is certain that *all* other possible explanations have been ruled out, the point I have made above. Ellson adds, further, that in the light of their statement the Duke investigators must be content with an ESP which is purely negative; namely, something other than the factors which have been controlled. I think this argument is valid.

When the Duke experimenters undertake to refute each of their 35 counter-hypotheses singly, their evidence is decidedly unimpressive. With so many possible hypotheses running wild, the control of one or of several—no matter how strictly done—can mean very little. Our authors are on firmer ground when they present six studies in which, they contend, *all* counter-hypotheses taken together are insufficient to account for the extra-chance results obtained. In my opinion this crucial test is not successful—or at best only partially so. One of the experiments cited (that of Murphy and Taves) showed no significant deviations from chance expectation; and it is hard to see how the covariation which our authors stress necessarily implies anything extra-sensory. The Warner and Riess experiments have been criticized by Kennedy and by Britt on what seem to me to be good grounds. The first (Warner's) involved only 250 trials and should certainly have been repeated. The second (Riess') gave the amazing average of 18 correct calls per 25 cards. Such a result as this is simply "too good to be true," and proves too much. Extra-sensory perception approaching knowledge is implied; and the experiment is hardly a test of the chance hypothesis. It may or may not be significant that the incredibly gifted subject in the Riess experiment later "disappeared" and could not be located; but certainly it does not strengthen the case for ESP. The other three experiments—all from the Duke laboratory—are perhaps more adequate than their description would indicate. It is quite impossible, even after a close reading, to tell how completely all of the experimental conditions were controlled.

In a list of six experiments, presumably crucial, we have a count of perhaps three in which the evidence favors ESP. Is not a stronger case than this necessary for proof?

(2) Are the ordinary probability methods adequate in ESP experiments? In early ESP experiments the regular procedure was to test obtained results (card 'guesses') against chance expectation as given by the binomial expansion. The binomial theorem assumes that the n operating factors which generate the distribution are similar, equal, and independent. For this reason, coins and dice are ordinarily used to demonstrate such chance distributions. Now when a person 'guesses' through a pack of cards, it is highly probable that his successive calls are not independent. For one thing, a person knows that there are five of each symbol and he may remember quite accurately how many of each kind he has called.

Criticism of the adequacy of the probability methods employed in ESP card calling appeared early and has led to a careful appraisal of the mathematics used in estimating divergences from chance. Criticism of the "independence issue" has, I believe, been satisfactorily answered. Moreover, the exact distribution has been found for the case in which two fixed decks are matched against each other (the so-called matching case). It is now possible to determine the effect of different ways of calling

the cards, and it can be shown that no one pattern is a priori more probable than any other. Early methods, though, technically incorrect, alter immaterially the significance of extra-chance results obtained.

I believe that the mathematical methods used at present in ESP work cannot be successfully challenged. I am unimpressed, nevertheless, by the "authoritative judgment released for publication by the president of the Institute of Mathematical Statistics" (p. 191) to the effect that the statistical methods used in ESP work are valid. Just why Rhine and his co-workers should quote this statement with so much pride is not entirely clear. Certainly any mathematician would be willing to certify that $(a+b)^2$ equals $a^2+2ab+b^2$ which is essentially all that the statement says. Such a pronouncement serves, of course, to awe the untrained reader to whom the case for ESP is so often addressed. It is no more relevant to ESP as a psychological fact than the statement by an eminent physicist that he has a soul is relevant to the theological issue of human immortality. An imperfectly adjusted chronoscope will give incorrect readings; but a perfectly adjusted one will not guarantee valid data. The prominent place given their mathematical endorsement by the Duke investigators smacks strongly of newspaper science.

(3) Are the ESP experiments trivial and unimportant as evidence of any 'real' ability, even when results are statistically valid? I believe the answer to this question is 'yes.' Consider, for instance, that the case for this hypothetical faculty which transcends the senses depends upon the 'guessing,' slightly better than chance, of five simple symbols equally represented in a pack of 25 cards. How would such evidence strike the totally unbiassed person, say the mythical Man-from-Mars? I recognize, of course, the demand for simplified and standard conditions in order to facilitate rigid laboratory control; but why rest the case on such evidence alone? Why not try pictures, complex designs, narratives which the subject has not seen? Why not report experiments in which the probability is 1/50 or 1/100 rather than 1/5? Or in which analyses are reported of the kinds of errors made? Are circles confused more often with squares (bounded figures) than with wavy lines? I know that many experiments of this sort have been tried and that my suggestions are far from original, but if results are consistently extra-chance, why not present them rather than rely upon simple card data? Hotelling has aptly remarked that "an effect of any sort in a statistical or experimental study can be real without being significant; it can be significant without being real." So a difference in height of 1/10 in. between two groups of children may be statistically significant, but if the groups are large and the overlapping considerable, of what 'real' value is it? C. E. Kellogg has assembled into one table the various experiments reported by the Duke investigators as favorable to the ESP hypothesis; it may be found on page 232 of the present volume. These data show that the continued imposition of stricter and stricter experimental conditions, elimination of sensory cues, errors in tabulation and the like, reduced the average number of hits from a high of about 5.50 to 5.20. Does this not suggest that still further control would reduce the average to the expected 5.00? Is there enough left even now to bother about?

When the Duke experimenters pass from such trifling results as those which they report to speculations as to the nature of ESP and its various attributes, their optimism seems hardly justified. I doubt if teachers need fear as yet that students will 'divine'

their examination questions. Certainly I have found very little evidence of it; I rather wish I could.

Columbia University

HENRY E. GARRETT

Sex in Development. By CARNEY LANDIS and co-authors. New York, Hoeber, 1940. Pp. xx, 329.

The preface states that this volume "grew out of a desire to evaluate the importance of psychosexuality in psychopathology." The research was planned largely as a check on psychoanalytic theory regarding the rôle of sex in the etiology of mental abnormalities, and was made possible by grants-in-aid from the Committee on Problems of Sex of the National Research Council. Eight co-authors collaborated in the investigation.

The aim of the authors was obviously one that called for a comparative study of normal and abnormal Ss. The abnormal group was composed of hospitalized patients of the following classes: dementia praecox, 41 (32 single, 9 married); manic-depressive, 41 (26 single, 15 married); psychoneurosis, 50 (35 single, 15 married); psychopathic personality, 10 (8 single, 2 married). A total of 142 abnormal S, 101 of whom were single and 41 married. The normal group included 109 single women and 44 who were married. The normals matched the abnormals approximately in religion, nationality, education, and social-economic status. Age range for unmarried was 15-30 yr.; for married, 22-35 yr.

The procedures used in obtaining the basic information about psychosexual development are in the opinion of the reviewer an important contribution to methodological technique in this field. The method was that of "controlled interview" in which all the Ss were asked the same questions and their responses were recorded verbatim. The questions numbered several hundred and covered much the same ground as did the questions used by Hamilton in his study of marriage. The procedure was somewhat less rigid than Hamilton's in that the questions were asked orally. To this point the technique has also much in common with that employed by Kelly, Terman, and Burgess and Cottrell in their studies of marital adjustments. But whereas these investigators treated statistically the responses of the Ss to individual questions, the present study uses the numerous individual responses in a given area as the basis for S's ratings on one or two variables in that area. The variables of the interview statisticized were thus reduced from hundreds down to a relatively small number. These relate to early sex information, sex aggressions, home background, menstruation, family ties, homoeroticism, masturbation, heterosexual experience, sexual affectivity, narcissism, psychosexual immaturity, the masculine protest, general adjustment, general compatibility in marriage, sex adjustment in marriage, general adjustment in marriage, childhood health, and present physical health. There were subratings in some of these areas but these do not greatly complicate the statistical problem.

The interviews were carried out by Dr. Agnes Landis. The ratings of the Ss on the above variables were all done by three other persons working independently from the interview records. Disagreements in the ratings were later ironed out by the judges in conferences. To reduce halo effects on ratings, a judge read at any one time only that part of an S's record dealing with a given area. The authors argue

that the single question brings out only a fragment of the picture, or may even distort the true picture if considered alone, and that it is possible to get nearer the facts by considering together the net results of all the questions in a given area. This method combined with the plan of having the records rated by judges who had not assisted in the interviews provides a promising technique. Information obtained in addition to the interview material included a general information inventory and results of a thorough physical examination.

Interrelations among the variables were measured by the contingency method, and Fisher's technique was used for determining the statistical reliability of differences where N was small. The authors are duly careful not to infer causal relationships from the mere fact of association between variables; instead, they set forth the alternative possible explanations of the association and let the reader take his choice.

The results of the investigation so far as the main problem is concerned can be summarized very briefly: the psychosexual histories of normal and abnormal Ss were in almost all respects practically indistinguishable. There were minor differences, but few that were statistically significant. Either the Freudians have greatly exaggerated the rôle of psychosexual factors in causing mental abnormalities, or the real facts regarding the psychosexual experiences were not brought to light by the interview method used. The authors do not deny that the conscious memories elicited by their questions may not tell the whole story, but it is possibly significant that the abnormal group gave very little more evidence of repression or emotional disturbance in answering the questions than did the normal group. One might say that the burden of proof is shifted more definitely to the shoulder of the psychoanalyst.

Comparison of the normal and abnormal Ss on anatomical and physiological variables showed no outstanding difference except a noticeable tendency in a few of the psychotic group to physical hypofunction. If there are physical stigmata of these forms of mental abnormality in women they are yet to be discovered.

Despite the negative findings on the primary problem, the results of the study throw valuable light on the general problem of psychosexual development and its relation to marital adjustments. Following are a few examples of relationships noted. In both normal and abnormal groups those reporting early sex aggression also reported most disgust about sex. Postpuberty aggressions seem to have no effect. Women not instructed in advance reported more emotional disturbance at first menstruation. Women still closely tied emotionally to their parents usually failed to develop strong heterosexual interests, were less well adjusted in marriage, and reported more illness during childhood. Only 22 of the 295 women failed to report "crushes" on others of their sex. The experience is not significant unless it is continued to the late teens. The normals and abnormals did not differ in homoerotic trends. The overt homosexuals showed more tendency to masculine body form than did others, but the number was too small to be conclusive. General adjustment to life tended to be poor for those who reported a history of excessive masturbation. About an equal proportion of normals and abnormals reported premarital sex experience. The few cases who suffered extreme guilt feelings as a result of such experience were all in the abnormal group. An attitude of disgust toward all sexual matters was reported by more normals than abnormals. "Premarital heterosexual intimacies appeared to be a mark of normal healthy development." (The statement

apparently refers chiefly to kissing and petting.) Marked retardation of psychosexual maturity was more common among the abnormals. The condition tends to be associated with underdeveloped body and poor marital adjustment. Good general adjustment is associated with favorable home background and with good marital adjustment.

In general this study agrees closely with Terman's regarding the correlates of marital adjustment. Income, occupation, presence or absence of children, amount of religious training, and number of siblings of the opposite sex have practically no significance. Of little if any importance are rated adequacy of sex information, sources of sex information, length of engagement, length of premarital acquaintance, and experience of sex shock. The one important disagreement has to do with family ties, and on this point it is probable that the questions used in the present study got at something rather different from what was tapped by Terman's ratings on parental attachments and conflicts. The reviewer is ready to accept the authors' conclusion regarding the apparent association between poor marital adjustment and inability to achieve independence from strong family ties.

A number of facts are presented which indicate significant differences in the psychosexual histories of the various types of abnormals studied. The reader will also be interested in the table of interscale correlations published in the appendix.

This study is of interest to the reviewer chiefly because of its bearing on the possible prediction of marital success and failure. He would like very much to see the techniques which the authors have developed applied to a population of several hundred married women. Such a study would go far to supplement, and doubtless in part to correct, the findings of other researchers on marriage.

The greatest weakness of the study so far as the validity of conclusions is concerned lies in the small number of Ss involved in most of the comparisons of subgroups. Unfortunately this is a weakness that the best statistics can not eliminate. The reader should bear in mind the fact that with very small numbers even group differences which figure as "statistically significant" are at best suggestive rather than conclusive, especially when, as in this case, one can not be sure that the populations studied are random samplings.

Stanford University

LEWIS M. TERMAN

Memorizing and Organizing. By GEORGE KATONA. New York, Columbia University Press, 1940. Pp. 318.

The problem of this volume is the validity of three assumptions regarding the primary form of learning: "1. Memorizing is the prototype of learning. 2. Understanding organized wholes is the prototype of learning. 3. Memorizing and understanding are distinct and independent processes" (p. 246). The objective is to show that the first assumption is not valid, and to support the avowed, but undefended, belief that the second assumption is correct.

There are two kinds of learning: senseless learning and meaningful learning. The former is characterized by "connections established by . . . drill (memorizing)," and the latter is characterized by "processes . . . such as 'apprehension of relations,' 'understanding of a procedure,' 'insight into a situation' (understanding, organizing)" (p. 5). The purpose is to demonstrate the superiority of learning by the method of "understanding," the measures of efficiency to be (a) the rate of for-

getting, and (b) the amount of positive transfer to a similar task. The complete elimination of the associationist's supposed identification of learning and memorizing follows from the statement that even memorization involves organization in a minimal degree.

The major tasks were "match tasks" involving two-dimensional reorganization of spatial forms, so that, for example, four squares made with 12 matches become three squares by moving three matches. These patterns are said to demonstrate the *Gestalt* laws of field organization. The experimental variable is the method of learning (teaching) employed in the practice period. The main methods are: (a) memorization, wherein the solution is shown without explanation, and (b) learning the solution by having it demonstrated in a way which presumably leads to "understanding" of the correct solution. Several methods of teaching by "understanding" are employed, but the only direct comparisons of the efficiency of these various methods are in a preliminary study of very low reliability (p. 88), and in a study of the effectiveness of the statement of the arithmetical principle involved and the method of "help" in which the Ss are first told to try to find the solution to the problem and are then shown the solution.

The major conclusions are: (1) The rate of forgetting is faster after memorization than after "understanding." (2) After memorization, performance on different match tasks is only slightly better than the performance of a control group, but after learning by "understanding" the performance on different tasks is almost as good as the performance on retests with the practiced tasks. (3) The amount of transfer after memorization is a function of the similarity of the practiced and tested tasks, but transfer after learning by "understanding" is unrelated to the similarity factor. (4) After learning by "understanding" repeated tests with new tasks result in progressive improvement in the performance on new tasks, but repeated tests with the practiced tasks reduce the efficiency of performance on new tasks. Of these conclusions, the difference in transfer effect after learning by memorization and "understanding" receives the greatest stress, because the studies of transfer emphasize the development of understanding of intrinsic relationships as the essence of learning. It is denied that any associationistic theory of transfer, or the theory of transfer through conscious generalization, is adequate to explain the results obtained, and the author proceeds to a *Gestalt* interpretation in the form of a theory of memory traces.

The theoretical arguments are frequently stimulating, but the definitions of some important concepts are non-operational and ambiguous. For example, the crucial work in Katona's definition of learning (p. 3) is "better," and this remains undefined. The definitions of learning by "understanding" and of "organizing" lack precision. "Understanding" consists of "grouping (organizing) a material so as to make an inner relationship apparent," and "the role of organization is to establish or to discover or to understand an intrinsic relationship" (p. 54). How to identify an "intrinsic relationship" in all instances of learning is not clear, especially since "understanding" need not imply even vague conscious formulation of the intrinsic relationship in words. Nevertheless, Katona says the Ss who were shown the solutions to match tasks by the method of "help" achieved organization and "understanding." The objective criterion of "understanding" seems to be high transfer-effect. The point is not that the definition is circular, but that the investiga-

tor of "understanding" and "transfer" does not have two independently defined terms the correlation of which can be determined.

The presentation of the association theory likewise suffers from lack of precision. There are the shades of the now elderly straw man of S-R constancy in the statement of the view, and the vigorous and mature statements of association theory by Carr, Robinson, Hilgard, and others receive no mention. In particular, Katona fails to make a clear distinction between the similarity of stimulus-factors, the similarity of set or attitude or process within the organism, and the similarity of the responses required in the practiced and tested performances, and he fails to make a clear distinction between the factors determining the *transfer of a response*, the transfer effect. In view of the neglect of these distinctions it is not surprising that Katona decides against the association theory on the ground that learning by memorization, where the problem solving attitude was deliberately avoided, produces low transfer effects, and that learning by "understanding," where the problem-solving attitude was deliberately suggested, produces high transfer effects. It would have been more defensible to attribute the various differences to a shift from a rote-learning attitude to a problem-solving attitude.

Quite apart from the controversy between associationism and Gestalt one may enquire regarding the reliability and validity of the experimental results reported. The only reliable major results are those summarized as conclusion (2) and (4), and the validity of these results suffers from certain uncontrolled sources of error. For example, the difficulties of the various match tasks are not controlled either through a reliable preliminary calibration or through a counterbalancing procedure. Also, in most of the experiments there is no control of the practice effect of the immediate test on the delayed retention and transfer measurements.

The most serious criticism, however, is that many of the major experimental results are unreliable. Thus, the reason for rejecting Katona's conclusion that retention is less after memorization than after "understanding" is that the differences on delayed tests are $16\% \pm 15\%$ (card-trick experiment), $10\% \pm 15\%$ (Experiment A), and $1\% \pm 13\%$ (Experiment C). Katona does not compute the reliability of any of these simple percentage differences, and the stated reliabilities of differences between weighted test scores are difficult to interpret because multimodal distribution of the weighted scores were regularly obtained.

University of Missouri

ARTHUR W. MELTON

The Varieties of Human Physique. An Introduction to Constitutional Psychology. By W. H. SHELDON, with the collaboration of S. S. STEVENS and W. B. TUCKER. New York, Harper & Bros., 1940. Pp. xii, 347.

In Rostand's play *Chantecler* the dog says: "I can feel barking within me the voice of every blood. Retriever, mastiff, pointer, poodle, hound—my soul is a whole pack, sitting in circle, musing." The authors of the book under consideration would say that this poor stage dog suffered from dysplasia or the uneven manifestation of components in the different regions of the body. They would also prophesy that in the personality sphere he would exhibit many mental conflicts.

The present book, however, is a serious one. It is, indeed, the first treatise issued by professional American psychologists in which body typology has been taken off the stage and the thesis proposed and supported that there is a close relationship

between bodily build and mental life. By most American scientists, Paterson's well-documented and well-reasoned book, *Physique and Intellect*, has been taken as a professional funeral oration well preached over the corpse of this idea. Sheldon, Stevens, and Tucker's book has, however, brought the corpse back to life. Essentially the point of view proposed is that the endless ways in which human bodies differ may be conveniently described in terms of components. The three components used in the Sheldon-Stevens-Tucker system take their names from the primitive germ layers of the growing organism. The first component, termed endomorphy, characterizes an individual whose bodily build is soft and round. The second, mesomorphy, means relative prominence of muscle, bone, and connective tissue. The third, ectomorphy, means relative accentuation of linearity and fragility. Again relevant to the germ-layer basis of the nervous system, the ectomorph is seen as having the largest brain and central nervous system.

The similarity of this set of categories to the categories previously proposed by the Continental psychiatrist, Kretschmer, should not make the reader identify the two systems of describing physique, because they are not identical.

In the present system the components are regarded as continuous variables which different physiques exhibit in different measured amounts. The numeral 1 is taken to represent the least possible degree and the numeral 7 the greatest possible degree of any one of the traits. Thus an individual may be described as 711. Such an individual is an extreme endomorph with minimal mesomorphic and ectomorphic characteristics. The 444 individual is at the midpoint of all scales. By objective measurement of photographs the authors have described 76 different patterns of these components or, as these numerical patterns are called in the book, "somatotypes." Some secondary characteristics such as gynandromorphy or bisexuality of form, fineness or coarseness of body texture, and hairiness are also considered in a complete description of the physique.

As parallel but not directly corresponding psychological types related to extreme bodily types are the following: with the component endomorphy, the temperament characteristic visceratonia; with the body type mesomorphy, the temperament characteristic somatotonia; with ectomorphy, cerebrotonia.

The technique of somatotyping begins by photographing three views of the individual after his clothes have been removed. These three pictures are taken on the same film and the pictures judged and measured. The procedure of measurement is somewhat complicated and cannot be described in this review. A machine has been devised which assists in the calculations necessary in part of this work. Without the machine just mentioned it is suggested that 1000 individuals could be somatotyped by two workers in about four months. The use of the special calculating machine devised for this purpose might cut the time required to one-fourth.

The authors promise a second volume to supplement the present book. It will deal in greater detail with the results of their studies of the relationship between bodily build and psychological manifestations. They now assert, however, that a surprisingly satisfactory relationship between human temperament and bodily build has already been established by them. That is, the round-bodied visceratonic is a comfortable person who likes people and good food. The muscular mesomorphy, with his somatotonic personality, is active and energetic. He is an extrovert in action. The thin, fragile ectomorph, with his cerebrotonic personality, is an introvert. His

history often reveals a series of functional complaints, including allergies and skin troubles. The individual who is predominantly ectomorphic meets his troubles by seeking solitude.

The authors conclude their book with the statement that "at the level of morphological description we have isolated some principal variables. We have discovered their inter-correlations in human physiques, and have standardized a method by which individuals may be somatotyped with precision and reliability. Upon this foundation it may be possible to build the superstructure of a science of human behavior."

The reviewer feels that it is too early for a critical evaluation of the new thesis of this book. It is clear, however, that the proposals of what the authors call "constitutional psychology" as described in the book cannot be dismissed without thorough study. In the next few years, the reviewer prophesies, we shall hear much more of "somatotyping." Much study will be given by various scientists to testing the detailed proposals of this book because they are of high importance in theoretical and practical work involving normal and abnormal human mental life.

Once the book has been read the typology suggested is so insistent that it is like whistling a tune after a light opera. Unscientific as amateur somatotyping may be, the reviewer cannot now help thinking of some of his friends as 117 or as 711, and such descriptions certainly mean more than saying that one is thin and the other fat.

Tufts College

LEONARD CARMICHAEL

General Psychology. By RICHARD WELLINGTON HUSBAND. New York, Farrar & Rinehart, 1940. Pp. xiv, 513.

A new textbook in the field of general psychology may fairly be judged from either of two viewpoints. How does it qualify, one may ask, as a presentation of the field of psychology-in-general? Or again, how does it qualify as a teaching device, as a text?

The chapters are grouped under these headings: Psychology and Its Aims (1 chapter), Genetic Background (2 chapters); Neural and Sensory Processes (3); Motivation, Emotions, and Personality (7); Individual Differences and Intelligence (5); Learning, Memory, and Thought (4). Thus this is another text evidencing the trend in psychology teaching in America today, in which motivational and personality problems are given more and earlier attention and the intellectual given less and later.

Inaccurate or misleading factual statements occur not infrequently. Samples are: the Greenwich observatory incident is incorrectly related (p. 7); Watson's list of three emotions is said "to have gained the most widespread acceptance" (47) and no challenging experiments are mentioned for 100 pages; the focussing function of the lens is incompletely drawn (Fig. 20)—a common mistake; "hot" is called a sensation (p. 123); touch sensations are "subdivided into rough, smooth, and sticky" (p. 126); thirst is said to be "of two types, true [represents genuine dehydration] and false" [when the throat is temporarily dry] (134); the rhythmic nature of sex drive is said to have been largely eliminated in human beings by the heating of houses and wearing of clothes (p. 135); smooth muscle fibers are

likened to the "skin" of a sausage (172). And other questionable assertions scatter themselves through other parts of the book—but these are details.

In its partial neglect of one type of material Husband's book is a bit like many others written in both general and special fields of psychology. This is the neglect to give the students more impression of how psychologists work, how their researches are carried on. Methods of investigators are given little if any attention; and, like those several texts in child psychology which are only handbooks of findings reduced in scale and scope, this book gives the reader only a few glimpses into the working laboratory or clinic.

The highly condensed chapter on "Functions of the brain and nervous system" raises again the question as to how much of that topic is really helpful and necessary to an understanding of the remainder of the book, and how much must be included in order to satisfy the teacher.

As regards general viewpoint the book is a bit puzzling. A superficial turning of the pages makes it clear that the author has tried to be abreast of contemporary work, for the citations of experimental lines of evidence are for the most part timely. If one looks, however, for basic explanatory principles, and for recognition of contemporary systematic developments that have interpretative significance, he is disappointed. The organismic background, the genetic viewpoint consistently maintained, the rich yield of theories and explanations of the Gestalt workers, the articulate and clean-cut principles being made ever more so in conditioning laboratories—these are examples of what one misses. It is a pedagogical question, of course, how much of contemporary theoretical systems can safely be put into a first book; but the reviewer believes that this one definitely has too many trees and too little woods. Although strongly of the conviction that it is bad to load the beginning student with elaborate discussions of schools, and to give him the impression that psychologists today wear their school ties and devote their papers to polemics to do or die for dear old System X, he does believe that some grasp of certain well-knit interpretative viewpoints may be helpful to the student in getting a more adequate impression of what psychologists are doing as well as getting something more than memorizable strings of factual details.

As a device for teaching, Husband's book has good and bad points. It clearly aims at enlisting the student's interest; and this is done without employing techniques of the newspaper columnist and the candid camera, but maintaining a dignity of presentation on a level with that of the student's texts in other college courses. At times the reader finds himself addressed in the second person; and throughout the style is informal and uncomplicated.

In places the handling of the material seems to border on the hasty and careless. Illustrations introduced may fail to be illuminating because they are not apt (p. 177), or appear under wrong headings (p. 146), or are insufficiently analyzed (pp. 133, 151). In one place a presentation of material is repeated in an absent-minded way with a minor variation in sequence of points (pp. 198 ff., 203 f.). Frequently assertions made are too broad or unqualified (pp. 140, 149, 216); and classifications betray a want of logic (pp. 9, 13-14). It would seem, then, that this text might arouse something less than complete respect and dependence on the part of alert and critical students: it is hardly designed for the "A's."

On the other hand, it is to be said that some of the shortcomings mentioned may

be looked upon as advantages in some cases. A generous number of tables, figures, and brief quotations set before the reader, and substantial bibliographies to each chapter, give the skillful teacher raw material which he can whip into effective and interesting presentations. But this he would certainly have to do!

As a bookmaking job Husband's text is satisfactory. The dull-finished pages are printed in clear, legible type, and bound between handsome covers into a light-weight book easy to thumb and to carry. Why, it remains to ask, do publishers ever use blind page headings: in this case the book title instead of chapter titles?

University of North Carolina

JOHN F. DASHIELL

How We Learn. By BOYD H. BODE. Boston, D. C. Heath & Co., 1940. Pp. iv, 308.

This is a general philosophy of learning rather than an outline of experimental facts. It might be called a general philosophy of mind with an emphasis upon learning and upon the educational implications of a new view of the nature of learning. The phases of the theory of mind are traced from the substantial mind of popular belief and of Descartes to the most recent. The theory of a mind substance is given as the basis of a belief in formal discipline. Related to the theory of the substantial mind is the general classical theory that mind develops on the basis of its own laws; and harmony with itself is made the guiding principle of education. Occasion is taken to disagree with President Hutchins' theory that education can be perfected by mere exercise upon any type of material. The preference for the classics can be justified by assuming a substantial mind and the correlated doctrine of formal discipline.

Formal discipline itself is discussed through three chapters. A good survey of the experimental and theoretical literature is given. The net outcome is to leave very little either of the faculty psychology or of formal discipline. Some transfer of learning is seen to be based upon formation of habits and the acquisition of good habits of study, but nothing of specific improvement of faculties, or of the faculties themselves, of course.

More theoretical chapters treat of the development from mind substance to mental states, then to behaviorism and finally into a somewhat modified Gestalt. Mind-substance is eliminated on the arguments of Hume and by showing that it does not explain free will, one of the reasons suggested by its defenders for retaining it. A discussion of the theory of mental states gives an opportunity for considering Herbart's theory of apperception and his five formal steps in the preparation of a lesson. The theory of mental states as independent entities is rejected because of the difficulties it makes in the appreciation of physical objects.

Behaviorism is then brought before the bar. That also receives a negative verdict. Behaviorism has the advantage of avoiding dualism, but does so at the expense of eliminating ideals and purposes of all sorts and leaving the individual a mere machine. Finally Lashley's experiments on learning with brain injuries are adduced to prove that reflexes or changes in the synapses cannot be the complete explanation of learning. Learning depends upon the entire cortex not upon any particular circuits in restricted areas.

In the last chapters the Gestalt theory of learning or a schematized modification of it is developed as the true statement of the nature of learning, under the heading of the pragmatic theory of mind. Dewey's theory of the reflex arc is extended to

form the model of the idea. His notion that the response aids in the determination of the stimulus is extended to make the response determine the nature of the environment. This introduces the notion of the field as including both the object and the mental processes. A change in any part of the field influences all other parts and so any change in body influences mind and vice versa. Through the field runs the influence of purposes which guides all adaptation and this is characteristic of all human behavior. Each step is at once progress towards a goal and a process of adaptation. "Conscious activity is carried forward to a goal by constantly searching out the conditions of the 'next step' all along the way."

In a chapter on Pragmatic Education this adaptive side of learning is emphasized. Köhler's apes are made the prototype of learning through purposeful adaptation. Insight as the appreciation of relationship is necessary in addition to or in place of trial and error and mere formation of habits. Insight changes the environment in learning as well as adapting the individual to the environment. In the school as outside, learning is a process of transforming experience in the interests of better control. The school "is a place where new experiences are provided in such a form as to best promote that reconstruction or reorganization of experience which is identified with education." The social aspects of experience are highly important. This aspect gives an opportunity to discuss the problems of education in a democratic state that involves adaptation of the state to changing needs as well as learning to live in the state as it is.

The argument is clearly stated throughout and will afford the student an interesting guide to the development of theories of mind and of education. It is written at a rather elementary level, an advantage for the student. The more mature individual would ask for a more definite formulation of the field theory. It may be no more than a new formulation of interaction in a vaguer analogy. This is probably too much to ask. The book can be recommended to the student and teacher as a survey of theories of mind and learning.

University of Michigan

W. B. PILLSBURY

The Psychology of Music. By MAX SCHOEN. New York, Ronald Press, 1940. Pp. vii, 258.

For many years materials have been accumulating for a comprehensive psychology of music based on the researches of estheticians, physicists, experimental psychologists, and historians of the art and science of music. In the present volume, Schoen offers an admirable outline of the field of musical psychology which summarizes and organizes the existing literature and presents valuable researches and conclusions of his own. The eleven chapters of this book offer a particularly well selected and well balanced assortment of material, with an economic arrangement in categories of musical discussion.

The first four chapters treat of tonal elements and their successive and simultaneous combinations in melody and harmony. They survey and evaluate competing theories of consonance and dissonance. Several chapters follow on the influence of music on imagination, ideas, and affective processes. A review of the literature reveals a common agreement that musical imagery is never specific and that musical synesthesias are arbitrary products of experience. On the other hand, affective responses to music are real though complicated by a great variety of extraneous

factors; mood effects can be definitely demonstrated, and the traditional view of the difference of response to major and minor modes receives considerable support. The modification of physiological responses and bodily reflexes involved in emotion is also marked, and data of this kind throw light on the nature of musical enjoyment and on musical esthetics in general.

A comparative study of various efforts to determine types of musical listeners reveals several useful classifications of which the one by Myers seems most generally serviceable. Investigations of musical types and of gifted musicians as well as the very nature of musical perception and response lead Schoen to conclude that the ultimate basis of esthetic response to music is a function which may be termed form-mindedness. The sources of this must be sought in innate or acquired sensitivity to the principal aspects of musical form, such as pitch, intensity, duration, timbre, consonance, and rhythm—all of which are open to exact experimental study. Investigations of this kind, together with studies of musical execution or productive response, offer a firm basis for an applied psychology of music, and the preparation of tests of musical talent and aptitude, as well as the creation of techniques for the accurate study of particularly important forms of musical production.

The second part of the work is, therefore, devoted to the applied psychology of music. It embraces such topics as musical heredity, the psychology of musical prodigies, and the development of musical talent in average children from infancy to adolescence. These lines of research offer general data for the definition and recognition of musical talent in general, and special musical aptitude in particular. The author presents a scholarly and critical survey of existing systems of tests, both standardized and unstandardized. The music tests of Seashore, Schoen, Ortmann, and Kwalwasser and Dykema are given special attention. The psychology of artistic singing is probably of greater general interest than any other topic of musical psychology, since vocal music is the most primitive form of the art, and is still the most widely practiced. In this connection, Schoen reviews the experimental literature of the vibrato. He is himself a pioneer in this field, and his discussion of the comparative singing of a number of leading vocal artists is illuminating, revealing the evolution of a specific technique for the rigorous and exact study of a form of musical production.

The book closes with a valuable chapter on the genetic psychology of musical talent, sensitivity, and expression. Ample bibliographies offer guidance to further study and the work will be indispensable to anyone conducting courses on the psychology of music. Schoen's book will also appeal to a wider circle of readers. Its clear style, orderly arrangement, and freedom from unnecessary subtleties will attract the music lover desirous of a comprehensive treatment of music, which may enlighten him, at one and the same time, on the physics, psychology, and esthetics of the subject. The professional musician, the music teacher, and the directors of musical institutions will find the work of great value. Psychologists may find it a convenient source of orientation for further experiments.

University of Cincinnati

CHARLES M. DISERENS

Psychology in Education. By H. SORENSON. New York, McGraw-Hill Book Co., 1940. Pp. 490.

The factual material used in this book has been selected from the more recent

reports of investigations and knit into the standard pattern of texts that are designed to cover the field of general educational psychology. Its individuality emerges from the author's honest over-simplification of the traditional problems. "The major objective of the writer was to produce a volume that would contain and interpret the fundamental psychological facts, principles, and theories applying to education" (p. vii). No emphasis has been placed on research techniques or on design of investigations. The topics of physical and social development are considered in some detail as are the problems of emotion and mental health. A discussion of the factors contributing to learning and related problems comprises most of the book—14 of its 20 chapters. A chapter on heredity, environment, and human development occupies 47 pages. The chapter on studying effectively, while avowedly oversimplified, is a valuable summary, and the one on transfer and mental discipline must leave the student with a clear picture of the controversy and at least a belief in the advantages of teaching for transfer and for application of generalizations, knowledge, and skill to practical life situations.

One topic is included that is not usually discussed in texts on educational psychology: the activity and project method of teaching. Here the results of 10 selected investigations dealing with informal and incidental learning are reviewed. A point is made of the fact that teaching methods based on motive and interest and not on formal drill really do induce effective learning.

The author has been extremely eclectic. He has followed no system, indicating in his preface that a systematic approach would preclude comprehensive treatment of the various problems. This situation has resulted in use of certain loose terms and expressions, and perhaps also has led to a tendency to name rather than to describe. "The altitude of a person's intellect is determined by the difficulty of the tasks that it can do, and the power of the mind is determined accordingly" (p. 156). The use of the impersonal pronoun is curious, and the abuse of such words as *determine* and *cause* is astonishing. "Our emotional states determine whether we are happy or sad, friendly or angry, confident or afraid. . . . The emotions accompanying pleasurable activity remove fatigue, whereas emotions of anger and distress cause us to be fatigued" (p. 51). The term attitude is not defined either in the text or glossary. It is, however, used in various contexts throughout the book.

The author says in his preface: "The capacity to learn academic material is probably fixed within narrow limits by nature;" later on he seems to reverse his position when he cautions that "serious danger lies in taking too deterministic a point of view" (p. 236). He stresses the need for recognition of individual differences in pupils, is opposed to teaching by drill, and is opposed to a goal of symmetrical training.

Three serious errors of omission are apparent. First, no discussion of attitude development or measurement is included. Then too, norms for the 1937 Revision of the Binet Scale are disregarded. This would not have been serious but for the fact that the author gives a description of the 1937 revision but uses the 1916 norms as the basis for interpreting performance. The important differences between the norms for the two revisions seem to have been overlooked. Finally, there is complete lack of reference to those phases of the field of student personnel work that pertain to educational orientation and to vocational orientation. Here, it seems to the reviewer, are techniques that have been developed by the psychologist to such a degree of

usefulness that there is growing demand for their application and increasing need for including them as part of the field of educational psychology.

University of Maryland

ROGER M. BELLOWS

Social Psychology. By OTTO KLINEBERG. New York, Henry Holt & Co., 1940. Pp. xii, 570.

The author's stated intention to write a comparative social psychology is responsible for the distinctive features of this entertaining textbook which will be welcomed by many instructors as a useful source of ethnological material relevant to social psychology. The extensive references following each chapter will also prove valuable in this connection. As a result of the generous use of facts regarding the manner in which people of different cultures satisfy their motives, express their emotions, use language, perceive and remember events, develop personalities, and come into possession of notions regarding what is normal and abnormal, the book possesses a freshness of approach that is often lacking in texts which confine themselves more strictly to conventional psychological subject matter.

There are other consequences of an anthropological orientation, the most obvious of which is the opportunity which it offers for driving home the importance of the social determination of motives and of behavior. A statement in the concluding chapter indicates this emphasis. "If there is one general conclusion which may be said to emerge from this survey of the available knowledge in the field it is that most of the characteristics as well as the institutions of modern society are to be explained in terms of social rather than biological factors" (p. 549). Klineberg devotes considerable attention to the biological basis of motives and personality and his evidence does not suggest that such factors are ever without importance. This must certainly be true if capacities such as learning and observation are considered dependent on characteristics of the nervous system. The facts do appear to indicate, however, that differences in the behavior development of persons of different culture are largely a function of social factors.

The book demonstrates effectively the importance of anthropology to social psychology. What is the contribution made by psychology? There are the usual discussions of methods of measuring personality, intelligence and attitudes, observations on emotional expressions, and scattered references to other sorts of psychological experiments, but on the whole the psychologist has contributed much less to the subject matter of the text than has the anthropologist. This fact is particularly striking with reference to the explanatory concepts used which are for the most part in terms of culture. It would seem to the reviewer that it should be one of the psychologist's special tasks to describe the mechanisms involved in modifications which take place in the individual as a result of his experience in a society. In other words, he should be concerned somewhere along the line with the way in which culture builds habits and forms attitudes, for such intervening processes must mediate the effects of culture on behavior and are indispensable if an attempt is to be made to predict the behavior of "the individual as related to other individuals," which, according to Klineberg, is the task of social psychology. It is the wholly unsystematic handling of this problem of socialization which, in the reviewer's opinion, is the weakest point in the text. There is no discussion of learning. The word does not even appear in the index. The term 'conditioning' is employed

several times but without any attempt to indicate its generality as an explanatory concept. Imitation and suggestion are discussed but the concepts are not used in any way to organize or simplify the data of the other chapters. Certain references are made to psychoanalytic principles such as repression and inferiority feelings but again this viewpoint is not developed sufficiently or applied generally enough to give a systematic slant to the book.

The Chapter on Attitudes provides one of many striking examples of the unrestrained eclecticism which results from the absence of some sort of systematic structure. Allport's analysis of attitude formation in terms of the accretion of experience, acceptance of ready-made attitudes, etc., is presented and apparently approved. To this analysis is then appended the psychoanalytic notion that social attitudes, such as radicalism, may be determined by revolt against family authority. Fourteen pages later Doob's seven principles of propaganda are listed to explain the modification of attitudes by propaganda without any attempt whatever to indicate the possible relation of this set of concepts to the explanations previously given. It is evidently too soon to expect any one to offer a completed system of social psychology but that is no excuse for the total lack of concern with systematic issues which mars the present treatment of the subject.

Randolph-Macon Woman's College

HELEN PEAK

Psychology. By ROBERT S. WOODWORTH. Fourth edition. New York, Henry Holt & Co., 1940. Pp. xiii, 639.

In spite of extensive revision, there is no fundamental change in the fourth edition of Woodworth's introductory textbook. It has been considerably expanded in substance, roughly by about 25% in number of words. One finds many changes in details, especially the enrichment of most topics by references to recent research, but the basic architecture remains virtually the same. Woodworth reaffirms, in effect, his settled convictions about how psychology should be written for the beginning student.

With respect to content, these convictions seem decidedly conservative in 1940, perhaps in considerable contrast to the impression created two decades ago. There are two introductory chapters, four chapters on individual differences and personality, and one chapter on each of the following topics: heredity and environment, the nervous system, learning, memory, motivation, affection, perception, vision, other senses, thinking, imagination, "personal applications." The author's attitude throughout is one of serious scientific concern for the essential issues in differential and general psychology. He makes no effort to turn psychology into *life*, which it was never meant to resemble. In fact, he fails to take advantage of an opportunity, in the second chapter, to give the student a general picture of man as a biological organism functioning in a social environment. This chapter is entitled "Man in his environment;" but unfortunately 25 of its 34 pages are devoted to the highly abstract topic of "set and attention."

The suggestion that any part of Woodworth's textbook is too "technical" may sound absurd, both to his admirers and to his critics, for the book is usually considered to be a "popularized" version of psychology. This notion is due probably to his style and to his informal attitude towards the reader, not primarily to choice of subject matter nor to the character of his thinking. He writes in lucid, simple

language, even about the most complex problems; but he writes with unvarying scientific intent. When he introduces systematic concepts, his disarming verbal manner softens the impact of the intellectual blow to the point of virtual painlessness. This characteristic of Woodworth's writing is at once the basis of its peculiar distinction and of its major limitation. When the subject-matter is somewhat amorphous, and in need of incisive conceptual articulation, it may fall apart entirely in his casual, non-technical discourse. Witness the chapter on "Personality." But if an important issue, such as "nature vs. nurture," requires discriminating elucidation in terms of diverse lines of evidence; or if a topic heavy with technical detail, vision for example, must be simplified for the college freshman—in both these situations Woodworth is without peer among psychological writers.

It is a happy circumstance that an author's painstaking and successful efforts bring his book to a peak of effectiveness should be matched by the professional achievement of his publisher. In sharp contrast to the nondescript appearance of its predecessor, the present edition is almost flawless as an exhibition of craftsmanship in textbook design.

Vassar College

LYLE H. LANIER

The Hopi Child. By WAYNE DENNIS. New York, Appleton-Century Co., 1940. Pp. xi, 204.

This description of the training and character of the Hopi child is a welcome addition to the literature on the relation of personality to culture. General discussion of the problem of cultural determinism is sufficiently advanced so that the present need is to accumulate detailed studies of limited aspects of the problem. Dennis' study, though not thoroughly directed toward specific hypotheses, is a move in the right direction.

Against the background of a native culture, resisting in various degrees in its several villages the encroachments of Western culture, are described the practices of child care and training. In addition to Dennis' observations, made in native villages during the course of two summers, are accounts of Hopi ways based upon native informants. The book concludes with personality sketches of Hopi children and a census of problem cases.

Four findings are of especial interest. Binding a child to a cradle board for the first six months of life, the traditional practice among the Hopi, has no noticeable effect upon motor development in general and certainly no effect upon the age at which the child is able to walk. Hopi children whose mothers, in defiance of custom, do not use the cradle board do not walk any earlier than their fellows who have been strapped down according to convention. In the second place, Mead's generalization about the relation between nursing frustration and thumb sucking needs to be amended. The Hopi mother nurses her baby whenever it cries and nursing is continued beyond the second year. Nevertheless thumb sucking is not an uncommon practice among Hopi children. In the third place, in comparison with our culture, children's crying is an infrequent occurrence. The simpler pattern of Hopi life with fewer frustrating situations is apparently responsible for the lack of wails and tears. There is no time-clock routine of feeding and sleeping; toilet training is started relatively late; few objects in the Hopi village are forbidden to the child because they are breakable; though children are given corporal punishment at times,

they are not constantly scolded and lectured. Finally, Malinowski's observation that the boy's resentment of his father is traceable less to sexual jealousy and more to repressive discipline, is confirmed by Dennis. The Hopi boy's hostility is directed not at his father but at his uncle who exercises the disciplinary function.

Princeton University

DANIEL KATZ

Personality and Problems of Adjustment. By KIMBALL YOUNG. New York, F. S. Crofts & Co., 1940. Pp. x, 868.

This book is a veritable one volume library on the psychology of personality. Its more than 800 pages (many of them in small type) combine "the standpoints and data of psychology, social psychology, and cultural anthropology" in thorough yet readable fashion. There is a good index and the bibliography is excellent. The book's inclusiveness and breadth will make it invaluable as a text in institutions where inadequate library facilities impede the use of source material, as well as a reference book for harried workers who desire an adequate, eclectic summary of a literature that is rapidly becoming overwhelming.

The first part of the book sets the psychological basis for personality in the individual. Here we find the usual subjects such as constitution, motivation, development, and social interaction. The literature on such matters as morphology, endocrine glands, learning, and the neural basis of behavior, is abstracted carefully and fully. If there are occasional statements that an expert in some of the special fields might object to, it is well to remember that the book is frankly designed as a text and that the tremendous amount of diverse material covered makes too exacting a treatment of fine points impossible.

The second section on adjustment deals with such topics as the teacher-pupil relationship, marriage, and delinquency. This part is helped by the inclusion of numerous case studies. The third section treats of the integration of the personality in society through religion, art, and avocation. While necessarily brief, and somewhat personal, this final part is stimulating and gives a broad orientation to the entire book.

One might say that Young's treatment of personality is "amorphous" and that it lacks incisive interpretation. It is not fair, however, to criticize a book which has been developed deliberately as an eclectic text on the same basis that one would a theoretical contribution. As a text, Young's book is excellent, and it should occupy a commanding place in the field of personality.

Wheaton College

WILLIAM A. HUNT

Child Psychology for Professional Workers. By FLORENCE M. TEAGARDEN. New York, Prentice-Hall, 1940. Pp. xxv, 641.

In child psychology, as in other branches of the subject, there is a widespread demand for books in which psychological data are applied to practical problems. Teagarden's book is, in effect, a textbook on applied child psychology for professional workers who are not psychologists, but who have in hand many of the practical problems which arise in dealing with children. Over a period of years the author has given a course on child psychology to a group of students consisting for the most part of social workers, home and school visitors, and public nurses. No existing textbook of child psychology seemed to give these students the material

which they required, and to meet this need the present book has been written.

In consequence, it differs somewhat from the usual textbook in child psychology both in subject matter and in method. As typical of the former we may mention a chapter on problems of adoption, one on behavior difficulties, and three on handicapped children, matters which, if they are treated at all, are certainly not treated so extensively in other textbooks on child psychology. Moreover, the discussion of the more familiar topics such as heredity, intelligence, emotion, and education, is directed toward practical problems in the growth and social adjustment of the child. The chapters on handicapped children are particularly to be commended for their emphasis on the influence of handicaps upon the development of the child's personality and for their suggestions for overcoming the harmful effects of such handicaps. The chapter on the sex life of the child appeals to this reviewer as especially sane and practical.

Dr. Teagarden has thus written a book to meet a rather specific need. It is not an exhaustive treatment of child psychology, as indeed it was not intended to be. Many complex problems of theoretical interest and importance are over-simplified, as an almost inevitable result of the desire to write a practical book. It is written in a simple, rather didactic style. A well chosen list of references for each chapter increases the usefulness of the book in the classroom.

University of Pennsylvania

MILES MURPHY

A Study of a Group of Children of Exceptionally High Intelligence Quotient in Situations Partaking of the Nature of Suggestion. By RACHEL MCKNIGHT SIMMONS. New York, Teachers College, Columbia Univ., Contr. Educ., No. 788, 1940. Pp. 112.

This study shows that a group of bright children were less suggestible than a group of dull children from the same school, the members of the two groups being matched in regard to age and sex. This difference between the two groups appeared in tests in which it would seem that intelligence could play no part. Two of these were the progressive weights test and the Chevreul pendulum test. Two other measures of suggestion were devised by giving as preliminary tests (the Otis Suggestibility Test and the Street Gestalt Completion Test) and then suggesting that incorrect answers be substituted for certain correct ones. While the bright and dull groups differed reliably in these four tests of suggestibility, there was a very considerable overlap between the groups in each test, whereas in *IQ* the highest of the dull group was 41 points below the lowest of the bright group.

Although the fact that bright children are on the average less suggestible than dull children in situations in which intelligence apparently gives no advantage is stressed by the author, she makes no attempt to explain the presence of this difference. The reviewer wishes to suggest that it may find its explanation in the school background of the two groups of subjects. Since the bright children were matched with dull children in regard to age, the two groups were unlike as to grade placement, the brighter children on the average probably being two grades above the dull. (That is not reported by the author.) In the reviewer's opinion, teachers in the first, second and third grades seldom suggest incorrect answers to questions. In the higher grades, he believes it is customary occasionally to encourage independent thinking by making incorrect suggestions, and seeing whether or not the child will recognize them as incorrect. Under such treatment, the child learns that the adult

sometimes intentionally misguides him, and hence he is more critical of adult suggestions. If this is the case, the resistance to suggestion of the bright children might be a by-product of their advanced grade placement.

The reviewer is also led to remark that while suggestibility has frequently been related to age, to sex and to intelligence, it has seldom been related to the subject's experience with suggestion, either in his school or in his home milieu.

University of Virginia

WAYNE DENNIS

A Quaker Childhood. By HELEN THOMAS FLEXNER. New Haven, Yale University Press, 1940. Pp. x, 335.

The author of this book describes from memory her experiences as the second youngest child in a large Quaker family of Baltimore during the years 1871 to 1888. It is a picture of a busy family, fired by strong ambition, high idealism, and a firm belief in their duty to aid Providence in making this a better world in which to live. Although well-bred and deeply religious, they were beset by the usual problems of teasing, competition, quarrels, jealousy, differences of opinion, favoritism (imagined or real), parental domination, relatives, education, finances, clothing, vacations, love, marriage, illness, and death. There are examples of failure to live up strictly to moral principles even in a rigidly religious atmosphere; and the gap between generations in religious conviction is amusingly illustrated by the hiding of the piano on those occasions when the maternal grandmother visited the home.

A psychologist will read with interest that the author's father "engaged in an informal survey of thought transference in a little group composed of Stanley Hall, then still a professor at the Johns Hopkins University, and another professor whose name I have forgotten." There is abundant evidence that the author always retained a distinct fixation upon her mother. The reviewer counted 51 specific references which show a heavy loading of emotionality. On the other hand, it would appear that although the father was accepted by the author as an important member of the family organization, he received respect rather than affection.

Of considerable interest is the amount of English poetry which was read and memorized: it would undoubtedly be difficult to find the equivalent today. Occasionally a selection would be encountered which did not fit too well the Quaker notion of proper poetic decorum! Psychologists interested in family relations will find in this book many familiar problems in a cultural setting perhaps seldom to be duplicated today. They may find support for the theory that there is a sort of universality in human problems but that the form or expression is a function of the environmental setting. The book may be characterized as humanistically descriptive and admittedly non-scientific.

Iowa State College

MARTIN F. FRITZ

Pupil Personnel and Guidance. By RUTH STRANG. New York, Macmillan Co., 1940. Pp. viii, 356.

This book is of interest to psychologists—especially educational psychologists, clinical psychologists, and social psychologists with experimental interests—by its implications rather than directly. Its value is in the educational goals which it sets up rather than in the specific methods which it offers in meeting its objectives.

The aim of the book, as stated by Dr. Strang, is "to direct educators' attention to the children whom they should serve with sympathy and objectivity. Skillful instruction, sensitivity to individual pupils in every class, individualized pupil-teacher relationships, continuous and casual building of standards of social behavior and information, attitudes toward vocations, co-operation with community agencies and self-direction on the part of pupils, all are shown to be essential parts of the effective program of personnel work."

The style is bland and it is only by careful reading that one becomes aware of the numerous time-bombs that the psychologists should find ways of setting off. If the aims as stated by the author were put into practice, the class-room teacher would need to know the answers to just about all of the problems of intellectual, emotional, and social growth.

I fear it is going to take many years of patient research before even a few so-called specialists know how to guide as well as instruct children according to Dr. Strang's standards. This doesn't mean that the challenge should not be accepted.

Northwestern University

GRACE E. MANSON

Psychological and Neurological Definitions and the Unconscious. By SAMUEL KAHN. Boston, Meador Publishing Co., 1940. Pp. 219.

This book is an unusual mixture of three more or less independent parts: a test, a series of 591 definitions, and an exhaustive bibliography. In the text material, which includes an Introduction and a Foreword by other authors, Dr. Kahn is interested in presenting a primarily psychoanalytic point of view together with a relatively simple explanation of it. At one point, he sets down a list of 15 specific reasons why one should undergo a process of psychoanalysis. He discusses at length the relationship between subjective and objective thinking, favoring, in true Freudian manner, all things which are subjective as more important to our lives. Another chapter includes a short history of psychoanalysis; still another a discussion of its relation to recent philosophy.

Far more valuable to the average reader than the text will be the glossary, which may well serve as a psychological dictionary. It includes excellent definitions of such widespread terms as 'abducens nerve,' 'claustrophobia,' and 'test, Binet-Simon's.' The material in the glossary is well chosen and well defined, and would serve adequately as a reference for the layman reading in psychological literature. Not quite so complete, though adequate and well chosen, is the bibliography. It is divided into three parts, dealing with psychoanalysis, mental hygiene, and educational psychology, respectively.

As a unit this little book is woefully inadequate, but as a brief introduction to psychoanalysis, plus psychological definitions, plus glossary, it will serve the student and the non-psychological professional person well.

Milwaukee County Guidance Clinic

GILBERT J. RICH

Statistical Analysis in Educational Research. By E. F. LINDQUIST. Boston, Houghton Mifflin Co., 1940. Pp. xi, 266.

This text in statistics has two main purposes: to put some of the applications of R. A. Fisher's methods into concrete and specific form for use in educational research, rather than in their more familiar setting of agricultural problems, and to develop

in the student a more careful consideration of the implicit assumptions involved in using any particular statistical technique. Where students are not expert both in their own field and in statistics, translation of the applications of those techniques from agriculture to education is not easy. Indeed, transfer might not take place at all without the aid of some such volume as that provided by Lindquist.

While analysis of variance is the chief consideration of the book, five topics seem to be particularly worthy of the attention of workers in educational research. Some of these are treated within a few pages, while others appear in various situations. These five topics are: (1) levels of significance; (2) design in educational research; (3) sampling in educational research; (4) implicit assumptions involved in the use of particular statistical techniques; and (5) interpretation and comparisons of several correlation functions.

The examples used to illustrate the techniques are from real studies and not laboratory constructs. They give clearer pictures of the use and dangers of several techniques. To read the book with profit does not require mathematical skill beyond simple algebra. Familiarity with the usual elementary statistical techniques is assumed. Some acquaintance with the literature of research in Educational Psychology is also a useful background for reading Lindquist's book.

Ohio State University

HAROLD A. EDGERTON

Studying Children In School. By EDNA W. BAILEY, ANITA D. LATON, ELIZABETH L. BISHOP. New York, McGraw-Hill Book Co., 1939. Pp. 177.

The authors have planned a book to aid adults in studying the children under their supervision. They point out the need for such studies and emphasize the fact that a child must be studied as a whole. To simplify the collection of data "a selected list of items on which information can be ordinarily obtained without too much trouble or expense and for which the significance in a child's life can be more or less certainly pointed out" is given. The list is detailed and suggestions are made as to the interpretation and significance of the data collected. Exercises, which use existing case studies and other data, are given as preliminary training to first-hand studies of children. These exercises form the subject-matter of Chapter IV which serves as a workbook. Chapters V to VIII present, respectively, "Study of Child in Preschool," "Study of Child in Elementary School," "Studying Youth in Secondary School," and "Studies of a High School Population in a School Environment." These studies function as illustrative material which may be used in the workbook exercises. Chapter III, "Characteristics of Age Levels," gives scales for the interpretation of the data collected. The material in the book is carefully worked out and should be of practical value in training teachers and prospective teachers in studying children—a function which too many teachers perform poorly or not at all.

William Smith College

CLAIRE C. DIMMICK





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INTERSENSORY EFFECTS AND THEIR RELATION TO MEMORY-THEORY

By ROBERT WARD BURNHAM, Rutgers University

Since the classical experiments of Müller and Pilzecker on retroactive inhibition,¹ there has been a growing tendency to account for all mnemonic losses in terms of some principle of interference or interaction. The older associationistic explanations in terms of disuse, typified by Ebbinghaus' interpretations of his own researches,² have been outmoded by more recent contributions.

Even in the more restricted mnemonic field represented by research on the negative time-error, the earlier theories of Borak³ and Köhler,⁴ which postulated a factor of "sinking" or disintegration, have been replaced by interactive theories of assimilation evolving primarily from the work of Bentley, Lauenstein, Guilford and Park, Pratt, and of Needham.⁵ One of

* Accepted for publication June 28, 1941. This study is an abbreviated report of the experimental section of a thesis submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Rutgers University.

¹G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtnis, *Zsch. f. Psychol.*, Ergbd. 1, 1900, 1-300.

²H. Ebbinghaus, *Memory: A Contribution to Experimental Psychology*, 1885. (Trans. 1913 by H. A. Ruger and C. E. Bussenius, 1-123.)

³J. Borak, Über die Empfindlichkeit für Gewichtsunterschiede bei abnehmender Reizstärke, *Psychol. Forsch.*, 1, 1922, 374-389.

⁴Wolfgang Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *ibid.*, 4, 1923, 115-175.

⁵I. M. Bentley, The memory image and its qualitative fidelity, this JOURNAL, 11, 1899, 1-48; O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 17, 1932, 130-177; J. P. Guilford and D. G. Park, The effect of interpolated weights upon comparative judgments, this JOURNAL, 43, 1931, 589-599; C. C. Pratt, The time error in psychophysical judgments, *ibid.*, 45, 1933, 292-297; Time-errors in the method of single stimuli, *J. Exper. Psychol.*, 16, 1933, 798-814; The law of disuse, *Psychol. Rev.*, 43, 1936, 83-93; J. G. Needham, The effect of the time-interval upon the time-error at

Pratt's studies,⁶ however, has shown that there is a limit to interaction among traces at a psychophysical level. This limit is probably reached within a single sensory modality, and is almost certainly reached when heteromodal stimuli are interpolated. Pratt has also shown that the same kind of changes still occur in these non-interacting traces, and has therefore argued that such changes must be referred to some older principle of fading or disuse. In other words, the principles of interaction and of disintegration are both needed to account for the fate of certain traces. Before it can be argued that assimilation (interference, or interaction) is the *sole* condition underlying the fate of mnemonic traces, it must be shown that all psychological processes are capable of mutual interference.

In this connection an assortment of investigations has accumulated bearing on the effects produced in sensory phenomena of one modality by the simultaneous excitation of another modality. Although Ryan⁷ and Gilbert⁸ have reviewed extensively many of these studies, most of the investigations have never been appraised critically as to their validity. The results are by no means univocal. Some experimenters report cross-modal effects, others do not. In the case of positive results the evidence is far from clear, especially since, in many instances, either the method employed or the statistical treatment of the data is open to question.

Since sensory processes are the ontogenetic antecedents, so to speak, of mnemonic traces, it becomes important to discover the extent to which interference occurs at a sensory level. The problem, therefore, of the present investigation was to determine the effects upon various sensory thresholds of the simultaneous excitation of heteromodal sensory processes.

Experiments were performed to determine the effect of sound upon visual acuity, the effect of sound upon the flicker-fusion limen, the effect of sound upon the extent of the retinal color fields, and the effects of different intensities of light upon the upper threshold for pitch.

EXPERIMENT I. THE EFFECT OF SIMULTANEOUS SOUND UPON VISUAL ACUITY

In previous experiments upon visual acuity Kravkov and Gotoh reported that increased or decreased visual acuity was evident as a result of

different intensive levels, *J. Exper. Psychol.*, 18, 1935, 530-543; Interpolation effects with different time-intervals, *J. Exper. Psychol.*, 18, 1935, 767-773.

⁶ C. C. Pratt, Interaction across modalities: I. Successive stimulation, *J. Psychol.*, 2, 1936, 287-294.

⁷ T. A. Ryan, Interrelations of the sensory systems in perception, *Psychol. Bull.*, 37, 1940, 659-698.

⁸ G. M. Gilbert, Inter-sensory facilitation and inhibition, *J. Gen. Psychol.*, 24, 1941, 381-407.

simultaneous auxiliary stimulation.⁹ Whether visual acuity was increased or decreased depended upon the color of the test-squares and the background. With white squares on a black background it was reported that visual acuity decreased in the presence of a sound stimulus. With black squares on a white background it was reported that visual acuity increased in the presence of an auxiliary stimulus. Hartmann reported that an extraneous stimulus had a facilitating effect upon visual acuity regardless of the color of the test-squares or background.¹⁰ In none of these experiments was the evidence adequate for the conclusions drawn.

It is relatively unimportant, however, for the present study to know whether previous studies did demonstrate a reliable cross-modal effect. If it can be shown

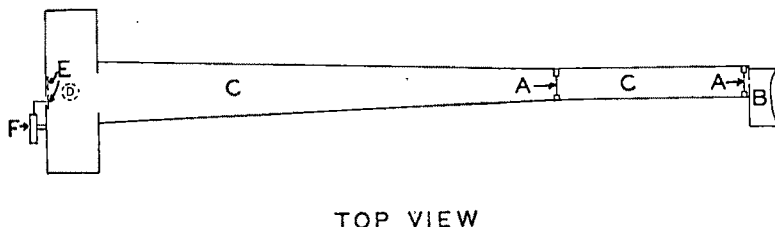


FIG. 1. DIAGRAM OF THE APPARATUS FOR VISUAL ACUITY

- | | |
|---------------------|---|
| A = Reducing lenses | D = Lamps, above and below test-squares |
| B = Head rest | E = Test-squares |
| C = Tube | F = Mechanism for moving squares |

that there are conditions under which there is not intersensory effect, either at a sensory or a mnemonic level, then it may be concluded that some principle other than interference or interaction must still be given a place in memory theory. It has been assumed in this study that with proper control of the attention-variable it might be possible to demonstrate a lack of so-called interaction. The following experiment was performed with such control. An attempt was made to measure visual acuity under conditions of simultaneous auditory stimulation and to compare the results with those obtained when measuring visual acuity in the absence of sound stimulation. By the absence of sound stimulation is meant the best approximation to silence which it was possible to obtain.

Apparatus. An apparatus was designed which was somewhat similar to that used by Hartmann. Fig. 1 gives a schematic diagram of the set-up. It consisted of a

⁹ S. V. Kravkov, Über die Abhängigkeit der Sehschärfe vom Schallreiz, *Arch. Ophthalm.*, 124, 1930, 334-338; Changes in the visual acuity of the eye under the influence of the illumination of the other or of acoustic stimuli, *J. Exper. Psychol.*, 17, 1934, 805-812. C. Gotoh, Über die zentrale Beeinflussung der Sehschärfe (Einfluss des Lichtreizes auf ein Auge, auf das Auflösungsvermögen des andern). (Univ.-Augenklin., Chiba) *Acta Soc. Ophthalm. Jap.*, 35, 1931, 887-890, with German summary 79-80. (Japanese) Abstracted in *Zentralb. f. d. ges. Ophthalm. u. ihre Grenzgebiete*, 26, 1931, 224.

¹⁰ G. W. Hartmann, II. Changes in visual acuity through simultaneous stimulation of other sense organs, *J. Exper. Psychol.*, 16, 1933, 393-407.

black cylindrical tube (C) 1.5 m. long and 15 cm. in diameter, with two optically appropriate lenses (A) so placed that the images of the test-squares (E) were greatly reduced. This reduction was necessary because of a lack of laboratory space. The squares for testing visual acuity were white and 3 cm. on a side. They appeared on a black background. The compartment shown in Fig. 1 at the left end of the tube was 30 cm. wide and 50 cm. high, and contained lamps (D) above and below the test-squares, so that the field was evenly illuminated. The illumination was reduced by rheostat control to a level at which the test-squares appeared on the average to be together when they were approximately 0.05 cm. apart. In order to keep shadows out of the tube a small rectangular aperture, 3 cm. high and 10 cm. wide, was inserted at the left end of the tube. This permitted the squares alone to be seen without permitting light to shine along the walls of the tube.

Fig. 2 is a detail drawing of the mechanism for moving the squares apart or

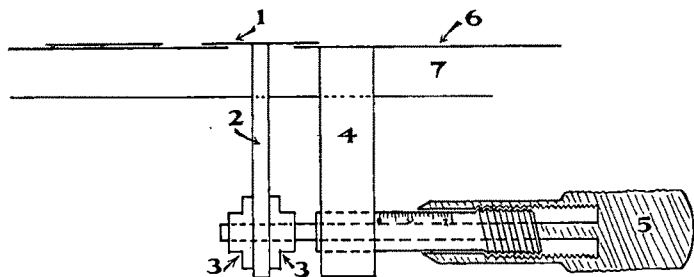


FIG. 2. MECHANISM FOR SEPARATING SQUARES IN THE VISUAL ACUITY APPARATUS
 1 = movable square; 2 = rod from square to micrometer; 3 = collars; 4 = rigid socket to hold micrometer; 5 = micrometer; 6 = black background; 7 = sliding shelf for rod (2) to move on.

together. The squares were made of heavy brass pasted over with white paper. It can be seen that one square, shown to the left of 1 in Fig. 2 was immovable. The other square (1) was fastened to one end of a heavy copper bar (2) 0.5 cm. x 1.0 cm. in cross section and 6 cm. long. This bar moved on a smooth sliding surface (7) and had a circular hole, 0.5 cm. in diameter, drilled near the end opposite to its brass-square-attachment so that it could fit over the moving rod of a micrometer device (5). The copper bar was held in place by two collars (3). The micrometer device, reading accurately to hundredths of a millimeter, was held in place by a rigid steel socket (4). By turning the handle of the micrometer (5) the square (1) could be moved toward or away from the stationary square. To S both squares appeared to move. The field (6) upon which the white squares were viewed, was of dull black cardboard. S sat at (B) in Fig. 1 which consisted of a head rest and a circular aperture, 2 mm. in diameter. The small aperture functioned as an artificial pupil. Not shown in the diagram is a black cloth hood which fitted around (B) and over S's head to keep out light.

The source of sound was an oscillator, and the tonal stimulus used was 775 ~ at an intensity of approximately 60 db. above threshold. This degree of intensity was used because it proved to be non-painful, but was of such intensity that adapta-

tion never became complete for any S. The sound was introduced into S's ears by means of earphones.

Procedure. Prior to any observations S sat for 15 min. to become adapted to the visual conditions of the experiment.¹¹ Earphones were worn for observations made both with and without sound. Ten groups of 10 observations each were made to measure visual acuity of the right eye. Half of these 10 groups of observations, in alternation, were made with sound stimulation, and half were made without sound stimulation. As an aid to the maintenance of a reasonably constant level of attention in the sound groups, the sound was turned on at the beginning and remained constant throughout the period of 10 observations. Observations were begun when the sound had been on for 2 min. A total of 50 observations were made for every one of 5 Ss, for stimulating condition, *i.e.* with and without sound, making 500 observations in all. Half of these observations were made while E

TABLE I
RESULTS OF VISUAL ACUITY EXPERIMENT
(N=50)

S	Means		σ		σ_M		$\sigma_{M_1-M_2}$	M_1-M_2	CR
	sound	none	sound	none	sound	none			
A	3.781	3.753	.404	.368	.057	.052	.077	-.028	.362
B	3.601	3.580	.442	.442	.063	.063	.088	-.012	.136
C	3.761	3.787	.374	.344	.053	.049	.072	.026	.362
D	3.799	3.793	.368	.362	.052	.051	.073	-.006	.082
F	3.701	3.703	.344	.346	.049	.049	.069	.002	.029

moved the squares apart and half of them were made while the squares were being moved toward each other. A modified method of minimal change was utilized, with subliminal steps of 0.05 mm.

Instructions. The instructions were as follows:

Report the instant at which the squares appear to you to be together or appear to separate, depending upon the direction in which they are moved, which the experimenter will announce before each trial. During the sound series pay no attention whatsoever to the sound. Keep your attention fixed firmly upon the squares. Pay no attention to anything at all but the squares. Attend strictly to the squares.

Subjects. The 5 Ss were all men who were taking the first semester of an elementary course in psychology.

Results. Means, standard deviations of the distributions, and standard errors of the means were determined for every S, both for the 'sound' and the 'no-sound' conditions, using generally accepted methods as recommended by the Dunlap-Kurtz manual.¹² Differences between the means

¹¹ That 15 min. was a sufficient period of dark adaptation is proven by the fact that arithmetic means, determined for each successive group of 10 readings, did not become less as the experiment progressed.

¹² J. W. Dunlap and A. K. Kurtz, *Handbook of Statistical Nomographs, Tables, and Formulas*, 1932, 1-163. The formulas we used for the standard errors and the critical ratios are as follows. Standard error of the mean: $\sigma_M = \sigma/N^{1/2}$; standard error of a difference, uncorrelated data: $\sigma_{M_1-M_2} = [(\sigma_1^2/N_1) + (\sigma_2^2/N_2)]^{1/2}$; critical ratio; $CR = (M_1-M_2)/\sigma_{M_1-M_2}$.

obtained with and without sound, standard errors of the difference, and critical ratios (CR) were obtained for every S. The results are presented in Table I. The values given there are in millimeters. The zero point was approximately 3.0 mm.¹³

It can be seen from the standard deviations of the distributions that there is no tendency toward a greater spread of scores with 'sound' as compared to the 'no-sound' condition. This fact gives evidence for the elimination of distraction and for the maintenance of a uniform degree of attention. The CRs were all fractional. They ranged from 0.029 to 0.362 and hence demonstrate no significant difference between visual acuity when measured with and when measured without sound. This bears out the hypothesis that proper control of attentional factors is fundamental to an elimination of a demonstrable difference.

EXPERIMENT II. THE EFFECT OF SIMULTANEOUS SOUND UPON CRITICAL FLICKER FUSION FREQUENCY

Several studies¹⁴ have reported various effects of auxiliary⁸ stimuli upon the critical frequency of flicker. These effects included: (1) a higher critical frequency of flicker for foveal vision in the presence of sound stimuli; (2) a lower critical frequency of flicker for peripheral vision in the presence of sound stimuli; (3) strengthening of flicker due to auditory beats; and (4) changes in critical frequency for different monochromatic rays when various auxiliary stimuli were present.

In Von Schiller's experiment there were no quantitative results, and in the other four experiments the data were so scanty that no statistical criterion of the reliability of differences could be established. In none of the experiments was a direct attempt made to control attention. It is, therefore, possible that these positive results may be explained at least partially in terms of a changing degree of attention or shifts in attention. As in Experiment I, the important thing, however, is to demonstrate that there are conditions under which no effect is present. This was done again by

¹³ The exact zero point, *i.e.* the point on the scale at which the squares were physically together, is not known. Since comparisons, however, are all that are of importance in this study, the zero point has no significance.

¹⁴ S. V. Kravkov, (The action of auditory stimuli on flicker fusion) *Fiziol. Zh. USSR*, 19, 1935, 826-834; Action des excitations auditives sur la fréquence critique des papillotements lumineux, *Acta Ophthal.*, (Scand.), 13, 1935, 260-272; On some correlations of different receptors in our color vision, *Compt. Rend. (Doklady) de l'Acad. des Sci. de l'URSS*, 22, 1939, 67-69; P. von Schiller, Das optische Verschmelzen in seiner Abhängigkeit von heteromodaler Reizung, *Zsch. f. Psychol.*, 125, 1932, 249-289; F. Allen and M. Schwartz, The effect of stimulation of the senses of vision, hearing, taste, and smell upon the sensibility of the organs of vision, *J. Gen. Physiol.*, 24, 1940, 105-121.

controlling and holding relatively constant the degree of attention. Attention was directed strongly to the flicker phenomenon being observed.

Apparatus. The apparatus employed in this experiment to measure critical flicker fusion frequency was basically similar to that used by Kravkov.¹⁵ It consisted primarily of a rectangular white cardboard box as shown in Fig. 3. A motor (5) was placed at one end of the box and had mounted on its shaft a black and white sector disk (6). This disk had four white and four black radial sectors arranged alternately around its center. In front of the sector disk was placed a white screen having an aperture, 1 cm. in diameter, through which flicker could be observed. At a distance of 90 cm. from the screen was the front of the box having an aperture of 2 mm. This aperture served as an artificial pupil for *S* and was surrounded by a headrest (2). Two 30 candlepower, 6 volt automobile lamps served as the source of illumination for the sector disk, see (4) in Fig. 3, and a light source (3) surrounded on

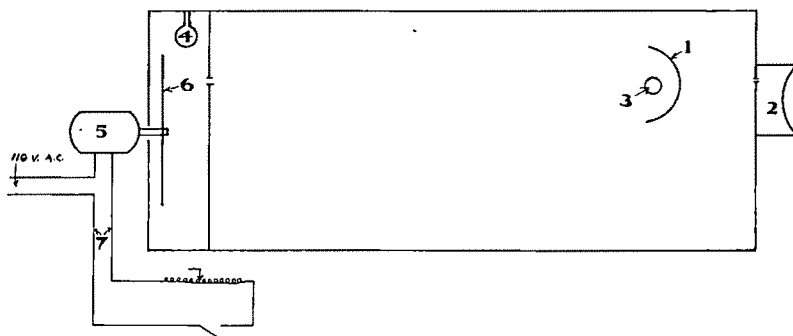


FIG. 3. DIAGRAM OF THE FLICKER APPARATUS

1 = shield for light; 2 = headrest; 3 = source of field illumination; 4 = source of illumination of the sector disk; 5 = motor; 6 = sector disk; 7 = electric circuit with variable resistor.

the side toward *S* by a shield (1), was equated roughly in brightness to that of the flicker source. Illumination was constant throughout the experiment at a photopic level.

A variable resistor was placed in the motor circuit as shown in Fig. 3 to change the speed of the motor. Since no tachometer was available the slider of the resistor was made to move on a threaded rod, making fine adjustments possible, and a pointer was placed on the slider in such a manner that it moved along an arbitrary scale calibrated in thirds of an inch and tenths thereof. All results were reported from this scale. Hence a value of 14.97 would indicate merely 14.97 thirds of an inch from the lower end of the rheostat. Higher scores indicate a fusion point at a greater speed and hence a more sensitive discrimination. Since only comparisons are needed between limens with and without sound, no attempt was made to convert the values into rate of flicker. The sound used was an oscillator tone of

¹⁵ S. V. Kravkov, *op. cit.*, *Acta Ophthal.*, 1935.

775 ~ at a supraliminal intensity of 60 db. Auditory intensity limens were obtained separately for each subject before any visual observations were made.

Procedure. S was seated at the headrest and a black cloth hood, which was fastened around the headrest, was placed over his head and fastened around his neck to keep out extraneous sources of illumination. Before observations were made S was given a 5-min. period of visual adaptation under these conditions with his right eye at the 2-mm. aperture. Earphones were constantly worn during the observations made both with and without sound. Each of 5 Ss was given 10 series of 10 observations using the right eye. Sound stimulation was introduced in alternate series. As before, during the sound series, the tone was turned on for 2 min. before observations were begun and remained on and constant throughout the series. For every one of the 5 Ss a total of 50 determinations of the fusion limen, for each stimulating condition, was obtained.

A modified method of minimal change was used with steps of 0.2 of a unit. Half of the observations were made when increasing the speed of the motor, and

TABLE II
RESULTS OF FLICKER EXPERIMENT
(N=50)

S	Means		σ		σ_M		$\sigma_{M_1-M_2}$	M_1-M_2	CR
	sound	none	sound	none	sound	none			
G	14.97	15.34	1.690	1.640	.239	.232	.333	-.37	1.11
H	12.82	13.42	1.500	1.630	.212	.231	.314	-.60	1.91
K	13.17	13.02	1.130	1.430	.160	.202	.258	.15	.58
L	12.92	12.91	1.270	1.460	.180	.207	.274	.01	.04
M	13.71	13.45	1.440	1.140	.204	.161	.260	.26	1.00

half of them were made when decreasing the speed of the motor. The rheostat slider was left in each successive position for 3 sec. to enable the motor to arrive at its new speed for each setting. This was sufficient time to make up for the lag in the motor. Since an eight sector disk was used, the speed of the motor at which fusion occurred was low enough so that there were no auditory cues from the motor.

Twenty preliminary trials were given every S in order to demonstrate the fusion effect. Every S was instructed, for the experimental observations, to report the instant that flicker ceased or appeared depending upon the direction of the change in speed of the motor. During the sound series, every S was told to disregard the sound completely and to keep his attention firmly fixed on the flicker phenomenon. He was also instructed to keep his attention firmly fixed on the flicker phenomenon in the 'no-sound' series.

Subjects. The Ss, five in number, were men who were taking the elementary course in psychology.

Results. The results were computed by the same methods as those used in Experiment I and are presented in Table II. The critical ratios range from 0.04 to 1.91. It can be seen that a statistically reliable difference is not obtained between the mean readings taken under conditions of sound stimulation and the mean readings taken in the absence of sound stimulation. This evidence may be interpreted to mean that attention remained

relatively constant and that there was no indication of cross-modal interaction.

EXPERIMENT III. EFFECT OF SIMULTANEOUS SOUND UPON THE EXTENT OF THE RETINAL COLOR FIELDS

Yakovlev has reported that simultaneous heteromodal stimulation produced changes in the extent of the retinal fields for orange-red, green, and blue, and that there was no change in the extent of the retinal field for red.¹⁶ Inspection of his data revealed that too few observations were



FIG. 4. VIEW OF THE COLOR-FIELD APPARATUS WITH S IN POSITION
The cloth hood is not shown. The oscillator is at the right.

made for a reliable demonstration of the effects he reported.¹⁷ There was also not proper control in his experiments of the factor of attention. In this third experiment the present investigator attempted to demonstrate that, by controlling attention, so-called intersensory effects do not appear, and that there is no change in the extent of the retinal color fields under conditions of simultaneous sound stimulation.

Apparatus. For measuring the color zones an apparatus was constructed in the form of a square box, 60 cm. on a side, as shown in Fig. 4. On top of the box was

¹⁶ P. A. Yakovlev, The influence of acoustic stimuli upon the limits of visual fields for different colors, *J. Opt. Soc. Amer.*, 28, 1938, 286-289.

¹⁷ At the written request of the present investigator, Yakovlev sent samples of his data. They showed that Yakovlev's conclusions were based on from two to five observations in each instance and that the range of his results was well within that found in the present experiment wherein negative results were obtained.

cut a semicircular slot, 7 mm. in width and 30 cm. in diameter. The slot was marked off in degrees from 0 to 90 in both right and left horizontal directions away from the medial plane. With *S* in place beneath the slot, his (experimental) eye would thus be in the geometrical center of the slot, and the zero mark would be directly in front of and above his eye at a distance of 15 cm. in the medial plane. A rectangular hole, 27 cm. in width and 34 cm. high, was cut in the front of the box

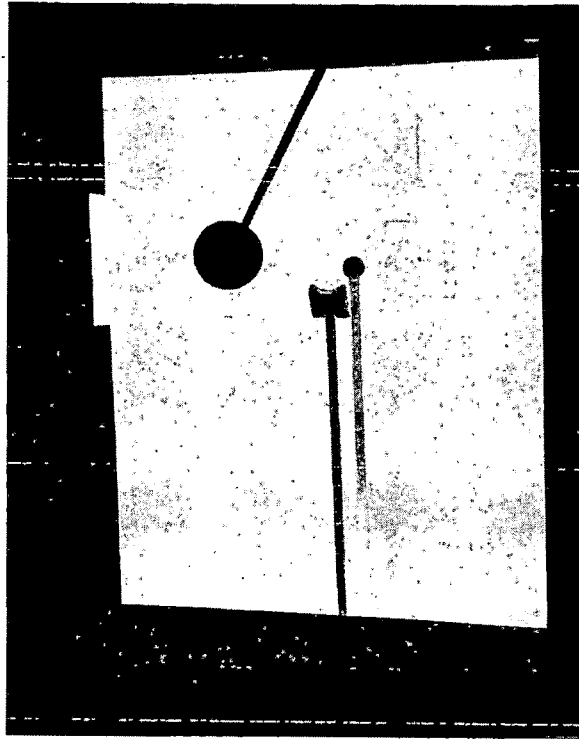


FIG. 5. VIEW OF THE FRONT OF THE COLOR-FIELD APPARATUS
The headrest, fixation-mirror, eye patch, and filter holder are shown.

for *S*'s head, and boards were placed inside and around the hole much in the manner of a window frame. This left a space around *S*'s head, but out of his field of vision, for a series of lights which served to illuminate the ground. A pointer-arm on top of the box, pivoting by a bearing on the center of the semicircular slot, held a rod, as shown in Fig. 4, on the end of which was a square filter holder, 4 cm. on a side (see Fig. 5). In the center of the filter holder was a circular hole, 2 mm. in diameter, which served as a stimulus patch. Eastman Wratten filters were used giving red, orange-red, green, and blue stimuli. The standardized characteristics of the filters were the same as those reported for Yakovlev's experiments, and were as

follows: for red, no. 29, transmission of visible wave lengths greater than 610 $m\mu$ with maximum transmission at 700 $m\mu$; for orange-red, no. 26, transmission of wave lengths above 590 $m\mu$ with maximum transmission at 680 $m\mu$; for green, no. 53, transmission of wave lengths from 480 to 630 $m\mu$ with maximum transmission at 540 $m\mu$; and for blue, no. 47, transmission of visible wave lengths below 540 $m\mu$ with maximum transmission at 440 $m\mu$.

A small round mirror, 1 cm. in diameter, was placed directly in front of S's eye and at a distance of 7.5 cm., so that by fixating the image of his eye in the mirror (this image is optically as far behind the mirror as is the eye in front) visual accommodation was set for 15 cm., the distance of the stimulus patch from the eye.¹⁸ An adjustable head rest was fastened beneath the center of the stimulus-arc. At one side of the box, near the front and outside S's field of vision, was a hole through which E could reach to change filters in the filter holder. The inside of the box was painted with flat white paint and gave a pronounced effect of spatial depth such as is obtained with a *Ganzfeld*.

The illumination was constant at 15 ml. and extraneous stimuli were virtually eliminated. When S was in position at the headrest, only the image of his eyeball, with the pupil serving as a fixation-point, was visible to him. Fixating the image of his own pupil, rather than some lifeless black dot, incidentally served as a decided aid to S in preventing eye movements.

The slender rod on which the mirror rested, and the filter-holder with its attached rod were painted white and blended almost completely with the ground. When S was in position for observations, these parts of the apparatus were barely visible as separate objects, and the stimulus-patch stood out sharply in contrast to the ground.

As in the first two experiments, the sound stimulus was a pure tone of 775 \sim , produced by an oscillator and having an intensity of approximately 60 db. above threshold. Auditory intensity limens were determined for every S prior to each experimental session.

Procedure. During each experimental session a black cloth hood was fastened over the front edge of the opening for S's head, and 5 min. of visual adaptation were given to every S with his head in the lighted box before observations were made. The hood remained fastened around S's head throughout each series of observations and served to keep out of the box extraneous sources of illumination. Earphones were worn for observations made both with and without sound. S's right eye was used in all observations and the left eye was covered.

Five Ss were given 20 observations per experimental period for 10 periods, with each of the four colors. Of these there were 10 observations per color per experimental period in each of the two horizontal meridians (usually designated as 0° and 180°) that it was possible to measure with this apparatus.¹⁹ Hence 80 observations were recorded for every S during a single period, making a total of 4000 observations for the 5 Ss in 10 periods. For every S for each of the four

¹⁸ K. M. Dallenbach, An aid for steady visual fixation, this JOURNAL, 42, 1930, 116.

¹⁹ Since these experiments were performed, the present investigator has perfected an apparatus which will measure all visual meridians while maintaining controlled background and stimulus conditions. See R. W. Burnham, A hemi-perisphere for visual measurements, *J. Exper. Psychol.*, 27, 1940, 333-336.

colors per meridian there were, then, a total of 50 observations for each condition, *i.e.* with and without sound. Half of these observations were made when moving

TABLE III
RESULTS OF RETINAL COLOR-FIELD EXPERIMENT
(N=50)

Color and direction	S	Means		σ		σ_M		$\sigma_{M_1-M_2}$	M_1-M_2	CR
		sound	none	sound	none	sound	none			
Red (right)	N	32.02	31.98	5.52	5.60	.78	.79	1.11	.04	.04
	P	33.58	34.18	3.52	3.68	.49	.52	.71	-.60	.85
	Q	36.86	36.70	6.60	4.32	.93	.61	1.11	.16	.14
	R	36.06	37.18	4.32	5.00	.61	.71	.94	-1.12	1.19
	T	42.98	41.90	6.00	6.08	.85	.86	1.21	1.08	.89
Red (left)	N	19.18	19.74	4.48	4.68	.63	.66	.91	-.56	.62
	P	18.78	19.70	3.24	3.96	.46	.56	.72	-.92	1.28
	Q	23.06	23.70	5.04	4.60	.71	.65	.95	-.64	.67
	R	18.94	19.02	3.88	3.80	.55	.54	.78	-.08	.10
	T	22.30	23.26	5.88	5.60	.83	.79	1.14	-.96	.84
Orange-red (right)	N	33.42	34.14	5.28	5.00	.75	.71	1.03	-.72	.70
	P	35.22	35.62	3.20	4.76	.45	.67	.81	-.40	.49
	Q	40.10	37.82	5.32	4.44	.75	.63	.97	2.28	2.35
	R	39.10	39.94	2.68	4.32	.61	.55	.72	-.84	1.17
	T	46.90	46.62	4.96	5.16	.70	.73	1.01	2.28	2.25
Orange-red (left)	N	20.14	20.58	4.04	4.40	.57	.62	.84	-.44	.52
	P	19.66	20.02	3.16	3.48	.45	.49	.67	-.36	.54
	Q	25.22	24.94	5.12	4.76	.72	.67	.98	.28	.29
	R	19.74	20.06	3.28	3.64	.47	.52	.70	-.22	.31
	T	23.38	24.06	5.44	5.04	.77	.78	1.10	-.68	.62
Green (right)	N	21.74	21.90	2.68	3.20	.38	.45	.59	-.16	.27
	P	21.22	21.46	2.56	3.68	.36	.52	.62	-.24	.39
	Q	29.78	28.98	3.96	3.12	.56	.44	.71	.80	1.15
	R	25.78	25.74	2.60	3.28	.37	.47	.60	.04	.07
	T	28.34	27.30	5.00	5.48	.71	.78	1.05	1.04	.99
Green (left)	N	16.14	16.42	4.16	4.00	.59	.57	.82	-.28	.34
	P	15.14	16.54	2.72	3.60	.38	.51	.64	-.96	1.50
	Q	21.46	21.26	4.28	4.12	.61	.58	.84	.20	.24
	R	16.26	16.26	3.20	3.56	.45	.51	.68	.00	.00
	T	19.50	20.50	4.80	5.12	.68	.72	.99	-1.00	1.01
Blue (right)	N	38.06	40.42	5.52	4.88	.78	.69	1.04	-2.36	2.27
	P	37.14	38.66	3.28	4.48	.46	.63	.78	-1.52	1.95
	Q	48.86	47.38	3.76	5.06	.53	.84	.99	1.48	1.49
	R	49.18	49.42	4.20	4.92	.60	.70	.92	-.24	.26
	T	50.54	48.86	5.24	4.84	.74	.69	1.01	1.68	1.66
Blue (left)	N	23.46	23.94	4.08	4.32	.58	.61	.84	-.48	.57
	P*	24.22	25.75	3.16	2.80	.45	.44	.63	-1.53	2.43
	Q	30.82	29.82	4.28	4.04	.61	.57	.83	1.00	1.20
	R	26.06	27.02	3.04	3.16	.43	.45	.62	-.96	1.55
	T	28.74	29.34	3.80	4.12	.54	.58	.79	-.60	.76

* N in this case is 40.

the color into the field of vision from the periphery, and half the reverse. The color presented and its direction of movement were determined by a prearranged random order of presentation. A modified method of limits was employed.

During every other period, making a total of five out of the total of 10 periods, sound was presented with the visual stimulus. The sound was of an intensity low enough as to be not painful and high enough so that no *S* became adapted to it. The fact that the oscillator sounded constantly throughout each 'sound' series aided in maintaining a constant degree of attention. Attentional factors were further controlled by specific instructions to every *S* to disregard the sound completely and to keep the visual observations entirely in the focus of attention. Each period averaged 50 min. in length.

Subjects. Four of the *Ss* were men, students taking the elementary course in psychology. The other *S* was a woman, a student who had completed an elementary course in psychology.

Results. The results of Experiment III are given in Table III. The *CRs* ranged from 0 to 2.43. Out of 40 *CRs* only 4 are greater than 2.00, 11 range between 1.00 and 2.00, and the rest are fractional. Although the *CRs* range somewhat higher here than in the two previous experiments, they still do not represent a reliable difference in these thresholds as determined with and without sound. There is, again, no indication from the *CRs* of cross-modal interaction.

EXPERIMENT IV. EFFECT OF DIFFERENT INTENSITIES OF LIGHT UPON THE UPPER THRESHOLD FOR PITCH

With respect to auditory thresholds, several experiments have been performed.²⁰ Hartmann reported that various levels of illumination had certain effects upon pitch discrimination and upon differential intensity thresholds. Upon calculating the *CRs* for Hartmann's results, this writer found, however, that none of them were significant.²¹ The highest one was only 1.89. In other words there was actually no evidence for cross-modal interaction. Child and Wendt reported statistically reliable evidence for a small effect upon the audibility threshold of a flash of light. They, however, stated that the possibility of attention as an explanation of the results had not been eliminated.

Experiment IV was designed to determine the effect upon the upper threshold for pitch of different intensities of light, controlling the factor of attention and holding it as constant as possible.

Apparatus. As a means of determining the upper threshold for pitch a modified Galton whistle was used. The whistle was activated by a controlled stream of air

²⁰ G. W. Hartmann, The facilitating effect of strong general illumination upon the discrimination of pitch and intensity differences, *J. Exper. Psychol.*, 17, 1934, 813-822; I. L. Child and G. R. Wendt, The temporal course of the influence of visual stimulation upon the auditory threshold, *ibid.*, 23, 1938, 109-127.

²¹ R. W. Burnham, A note concerning Hartmann's studies of intersensory effects, *J. Exper. Psychol.*, 29, 1941, 81-84.

from a compressed air-hose. Changes in pitch were made by a rotating rod in the whistle which was calibrated in hundredths of a millimeter. The rod used was the same as that illustrated in Fig. 2. The rod merely fitted into a tube which was appropriately cut to serve as a high-pitched whistle. The whistle was held firmly by a clamp at a distance of 1 m. from S's left ear. S's head was held fixed in a headrest. Observations were made under three illuminating conditions as follows: (1) when a 100 w. bulb in a reflector was placed directly in front of S's eyes; (2) with normal daylight; and (3) with S blindfolded.

Procedure. Using a modified method of minimal change, each of 5 Ss made 150 observations; 50 under each of the specified conditions of illumination upon the upper threshold for pitch. The whistle was manipulated by E and the length of the rod determining pitch was changed in steps of 0.02 mm. at 2-sec. intervals,

TABLE IV
RESULTS OF EXPERIMENT ON THE UPPER PITCH LIMEN

B=Condition 1, the bright visual stimulus; N=Condition 2, normal daylight; D=Condition 3, blindfolded. Under "Comparison," BN means the comparison of Conditions 1 and 2, BD of Conditions 1 and 3, and ND of Conditions 2 and 3. The figures are hundredths of a millimeter from the scale of the rotating governing rod.

S	Means			σ			σM			Com- parison	σM_1-M_2	M_1-M_2	CR
	B	N	D	B	N	D	B	N	D				
U	157.62	158.38	158.22	4.96	10.88	3.52	.70	1.54	.50	BN	1.69	.76	.45
										BD	.86	.60	.70
										ND	1.62	.16	.10
W	154.23	154.89	154.70	5.12	9.72	4.15	.72	1.37	.59	BN	1.58	.66	.42
										BD	.93	.47	.51
										ND	1.49	.19	.13
X	149.89	150.60	150.32	6.38	14.31	6.72	.90	2.02	.95	BN	2.21	.71	.32
										BD	1.31	.43	.33
										ND	2.23	.28	.13
Y	155.42	155.89	155.74	4.23	11.61	3.83	.60	1.64	.54	BN	1.75	.47	.27
										BD	.81	.32	.40
										ND	1.73	.15	.09
Z	161.02	161.74	161.53	7.34	11.01	5.55	1.04	1.56	.79	BN	1.87	.72	.39
										BD	1.31	.51	.39
										ND	1.75	.21	.12

giving S ample time to report the threshold when it was reached. Half of the observations under each lighting condition were made in an ascending direction and half of the observations were made in a descending direction. Every S was instructed to keep his attention strongly directed to the sound, and to disregard the light or other visual stimuli. In the case of the daylight condition, S faced a gray cardboard screen which covered most of his field of vision.

Subjects. Three of the Ss were men; the other two were women. All were adults. The two women and one of the men had not been to college and had never had a course in psychology. The other two men were college students who had completed a course in elementary psychology.

Results. Results are presented in Table IV giving means, standard deviations of the distributions, and standard errors of the means obtained under bright, normal, and dark conditions. CRs were determined for comparisons between the bright and normal (BN), bright and dark (BD), and normal and dark (ND) conditions. It can be seen that all the CRs were

fractional and demonstrate no cross-modal effect of a reliable nature. It is to be noticed that, under normal conditions of daylight illumination, during which time *S* was seated in an ordinary room facing a gray cardboard screen, the standard deviations are about double those for the other conditions, indicating probably some distraction-effect. Since the field of attention was narrowed considerably under the bright and the dark conditions, this result might have been anticipated. There is no evidence for a shift in degree of attention as shown by the low critical ratios.

DISCUSSION AND CONCLUSIONS

It is well known that sensory acuity may vary as a function of degree of attention. The greater the degree of attention, the lower the threshold, and vice versa.²²

It can be seen that, in experiments upon so-called cross-modal interaction, it is important to control and hold as constant as possible *S*'s degree of attention. Otherwise any effect that might appear could be ascribed either to changes in degree of attention, to cross-modal interaction, or to a combination of the two. In other words, it could not be stated with assurance that cross-modal interaction was entirely responsible for the effect observed. In the present experiments attention was held as constant as possible and the results showed no effect which could be called interaction across modalities. Since these experiments duplicated in a more acceptable form certain previous experiments which reported *unreliable* positive results, it seems safe to conclude that, under certain conditions at least, cross-modal interaction is not present.

Interaction at a sensory level refers to changes in the perception of a sensory item in one modality as a result of simultaneous excitation of the same or of other modalities. In other words, excitation of one receptor may simultaneously alter the excitability of another receptor. At a mnemonic level, however, interaction refers to the changes in originally perceived items which are produced by later impressions. This means that later excitations affect the remnants or traces of earlier excitations. At a sensory level, then, there are excitations affecting excitations, and at a mnemonic level there are excitations affecting the remnants of excitations. There is reason to believe that some specific relation exists between an excitation and its trace, since mnemonic reproductions to a greater or lesser degree resemble the original perceptions of which they are reproductions. The degree of resemblance is presumably a function of the amount of effect that these

²² L. E. Travis, Changes in auditory acuity during the performance of certain mental tasks, this JOURNAL, 37, 1926, 139-142.

later excitations have upon the retained traces of earlier excitations. Since there must be a definite relation between an excitation and its trace, one might expect to find similarities between any interactive effects which occur at the level of excitations and those which occur at the level of their traces.

The traces of excitations in psychophysical determinations of time-errors are most directly comparable to the excitations produced in determinations of sensory thresholds. Therefore, if they are found at all, similarities in interactive effects should be most apparent between these two, *i.e.* the traces of excitations in psychophysical determinations and the excitations produced in determinations of sensory thresholds. The traces for which there is evidence of interaction or interference in experiments on retroactive inhibition are of a much more complex nature and are not to be considered here.

There is evidence from the work of Pratt, Lauenstein, Bentley, Guilford and Park, and Needham for interaction among the traces of excitations from the same modality and the extensive work of Spencer²³ on Heymans' law²⁴ shows that changes in sensory thresholds are found when an auxiliary stimulus from the same modality is present. The limit to interaction in both cases, *i.e.* at the level of traces and at the level of excitations, is found when heteromodal stimuli are appropriately employed as shown by the present sensory experiments and by the mnemonic experiments of Pratt.²⁵

There are, then, similarities in the interaction and in the limit of interaction of simple sensory excitations and of the relatively simple traces found in psychophysical determinations. There does not, however, appear to be a limit to the interaction of the more complex traces produced in experiments on retroactive inhibition.

Since it has been demonstrated that there are psychological processes which do not give evidence of interaction, and since these processes are more or less directly related to their mnemonic after-effects which, under similar conditions do not give evidence of interaction, then it may be concluded that not all memory phenomena can be explained solely in terms of a principle of interaction (or interference). This demonstration of similarities between excitations and their traces brings out the important fact that memory should not be so completely divorced from sensation and perception as it so very often is in theoretical discussions. There may be

²³ L. T. Spencer, The validity of Heymans' law, this JOURNAL, 36, 1925, 427-433.

²⁴ G. Heymans, Untersuchungen über psychische Hemmung, *Zsch. f. Psychol.*, 21, 1899, 321-359; 26, 1901, 305-382; 34, 1904, 15-28.

²⁵ Pratt, *op. cit.*, *J. Psychol.*, 1936.

other clues concerning the manner in which memory operates which are to be found at a sensory level. They await only the ingenuity of an avid experimenter.

SUMMARY

(1) Modern memory theory tends toward explanations in terms of interference or interaction and excludes the older idea of disuse or disintegration. Certain psychophysical experiments question the ubiquity of interaction as an explanatory principle. Unless all psychophysical processes are capable of mutual interference, then interference can not be used as the sole underlying principle in memory.

(2) Since sensory processes are directly related to their mnemonic after-effects, any evidence against interaction either in sensory processes or in their after-effects would give support to a belief that more than one principle is needed to explain all of the phenomena of memory.

(3) Experiments are reported dealing with the possible effects of sound upon visual acuity, the flicker limen, and the extent of the retinal color-fields, as well as an experiment dealing with the possible effects of different intensities of light upon the upper pitch limen. No reliable evidence was obtained for cross-modal interaction under the conditions of the present experiments.

(4) It has been concluded that interaction (or interference) alone is not adequate to explain all of the phenomena of memory.

PROGRESSIVE CHANGES IN MEMORY TRACES

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The investigation of the changes which memory traces undergo took a turn when Wulf in 1922 claimed that the traces themselves present intrinsic tendencies toward a better Gestalt.¹ Later investigations by Perkins² and Allport³ confirmed this view. Recently, however, Brown⁴ and more certainly Hanawalt⁵ have found little support for Wulf's theory. The present paper attempts to answer some of the objections raised by Hanawalt.

PROCEDURE

The procedure devised by Hanawalt deviates from the method of the previous studies. Although the arguments for Hanawalt's method are not quite convincing,

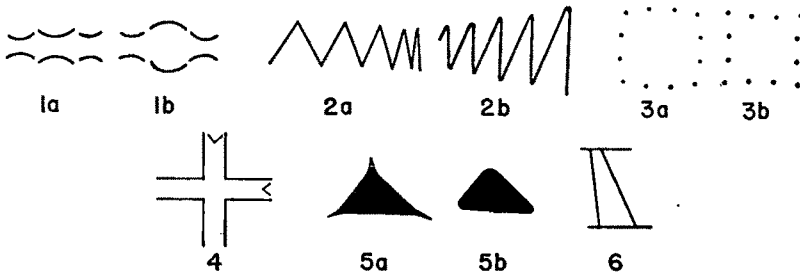


FIG. 1. THE DESIGNS USED AS EXPOSURE-MATERIALS

it has, nevertheless, certain technical advantages and was adopted here in order to make the two experiments comparable.

Several classes in psychology and one class in English at Wheaton College participated. All the Ss were women. They did not know the purpose of the experiment nor did they know that they would be called upon again, after the learning period, to reproduce the figures. There were 162 Ss: Group A of 82 and Group B of 80.

The material consisted of the Designs 1-6 shown in Fig. 1. There were two sets

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¹ F. Wulf, Über die Veränderung von Vorstellungen, *Psychol. Forsch.*, 1, 1922, 333-373.

² F. T. Perkins, Symmetry in visual recall, this JOURNAL, 44, 1932, 473-490.

³ G. W. Allport, Change and decay in the visual memory image, *Brit. J. Psychol.*, 21, 1930, 133-148.

⁴ W. Brown, Growth of "memory images," this JOURNAL, 47, 1935, 90-102.

⁵ N. G. Hanawalt, Memory trace for figures in recall and recognition, *Arch. Psychol.*, 1937, No. 216.

of six designs each corresponding to the two groups of Ss. Group A was tested with Designs 1a, 2a, 3a, 4, 5a, and 6, and Group B with those labeled b and also with Designs 4 and 6. The designs were presented on white cardboard, drawn in ink, with size varying between 3 in. and 10 in. Each was shown for 20 to 25 sec. with an interval of 1 min. for completion of the drawing from memory. The instructions were taken from Hanawalt's paper⁶ with minor changes in the wording. They call for copying the design with emphasis on accuracy. After the first presentation the copying was repeated under the same conditions. The order of presentation of the six designs corresponds to their numbers and was the same in both presentations. This was the learning period which furnished two sets of copies from each S.

For the reproduction test each group was subdivided into four subgroups. One subgroup in each group was tested immediately after the learning period, the other subgroups after three days, two weeks and six weeks respectively. This procedure results in eight independent groups, tested after different intervals of time. In the recall periods the Ss had to reproduce the designs in any order, again with accuracy stressed, the number of drawings remembered being unimportant. (The wording of instructions was always taken from Hanawalt.) After 6 min. the Ss were asked for any pertinent comments and after an additional 3 min. a recognition test was given. For this test a series of numbered drawings was prepared for each of the original designs and the Ss noted the drawing which most closely resembled the one shown in the learning period. For technical reasons the recognition test and the verbal reports were omitted in the immediate-recall periods.

RESULTS

Reproduction. The experiment furnished 1278 drawings, of which 468 were copies, 231 immediate reproductions, 171 reproductions after three days, 203 after two weeks, and 205 after six weeks. The question to be answered in this: Is there any progressive and directed change of the reproductions as compared with the copies of the learning period?

Design 1. Only one dimension of change was considered in each design except in Design 1 where changes of both gaps and curves were tabulated. As change in the gaps were listed all those reproductions in which some or all gaps were closed or in which there was one reproduction with an increase in the number of gaps with preservation of the flow of the lines as a whole. In Table I "Changes of curves" indicates the cases in which either the direction (inward *vs.* outward) or the number of curves was altered, or both. The numerous instances of change toward symmetry, levelling, or pointing were not recorded. Here, as in all subsequent tables, only those designs are listed as changed which did not have the same deviation in the copies, a practice which eliminates from consideration any changes during perception. The second copy was always used as reference because it may be regarded as the end result of the learning period.

The important finding for Design 1 is that the directed change depends on the structure of the pattern. Design 1a changes only in the curves, whereas Design 1b changes mostly in the gaps.

⁶ Hanawalt, *op. cit.*, 20-21.

Design 2a. In each drawing the height and the length were measured and the length divided by the height. This quotient indicates 'sharpening' by a lower value, 'levelling' by a higher one. The quotient for the reproduction was then subtracted from that for the second copy so that a positive difference indicated sharpening. The distribution of differences finally was transformed to an arbitrary scale of 7 steps with step 4 representing a difference of zero ± 0.50 . The average scale position for each group is tabulated in Table II. An average significantly above 4 means sharpening.

Design 2b. Table II also gives the average difference in degrees between the 'opening' of the reproductions and that for the respective second copy. The 'opening' is the angle between upper and lower border of Design 2b.

Designs 3 and 5. These designs were not measured but judged by a naïve judge. The procedure in the case of Design 3a was as follows. The judge received a reproduction and the respective second copy and was asked which of the two drawings was the rounder one. Roundness was judged on a scale of 1 to 3 (with provisions for judgment of equal). This results in a scale of seven steps, three steps for roundness on either side of 'equal.' A scale-value of '4' indicates equality. The judgments were passed at least twice on different days, with some groups three times. There was good correlation between judgments on different days. In the same way Design 3b was tested for 'pointedness.' Design 5a was tested for being 'collected,' a term used by the judge as indicating balanced unity and continuity between the center and the pointed ends of the design. In Design 5b the judgments were based on the degree of departure from triangularity and on rounding-out. The average scale positions of the reproductions for the four designs are listed in Table II. Since the judge did not see the original, the departure of the copies from it was not measured.

Design 4. The position of the little Vs in the bars of the cross was noted. Table III contains only differences from the second copy, since one case where the Vs were changed in both copy and reproduction is not counted. The table gives the frequency of each change.

Design 6. In Design 6 the inclination of the left vertical bar against the base line was measured. If this angle is below 88° the line looks slanting as in the original, if it is between 88° and 92° it looks perpendicular, and beyond 92° it looks slanting to the other side. Table IV records the drawings as separated into the group 'perpendicular or over' (an angle of more than 87°) and the group 'slanting' (an angle below 88°). This separation is carried through for both copies and reproductions. The table shows essentially this: (1) many Ss draw the left bar perpendicular in the learning period (lower half of the table); (2) as time passes an increasing number of Ss change from slanting to perpendicular or over (upper half of the table); (3) few Ss change from perpendicular copies to slanting bars in the reproduction and the fraction of those who do so shows no trend to increase in time (lower half of the table).

On the whole, then, Tables I to IV show that, as time elapses, a change increases in one direction.

Recognition. Koffka made the hypothesis that the same changes as in

reproduction could be demonstrated in recognition.⁷ Since then Zangwill⁸ and Hanawalt⁹ have investigated this question with inconclusive results. Their unsuccess is hardly surprising. If progressive changes are taking

TABLE I
CHANGES IN REPRODUCTION OF DESIGNS 1a AND 1b
The percentage of changes is shown for immediate reproduction and reproduction after 3 days, 2 weeks, and 6 weeks.

	Design	Immed.	3 days	2 weeks	6 weeks
Number of cases	1a	18	14	17	13
	1b	21	16	12	12
Changes involving gaps	1a	0%	0%	0%	0%
	1b	0%	0%	17%	33%
Changes of curves	1a	0%	21%	41%	61%
	1b	5%	13%	25%	0%

TABLE II
CHANGES IN REPRODUCTION OF DESIGNS 2, 3, AND 5
The average of the differences between each reproduction and its second copy is listed under "diff." The differences for Design 2b are measured in degrees; for the other figures an arbitrary scale of 7 steps is used, with a scale value of 4 representing no difference.

Design		Immed.	3 days	2 weeks	6 weeks
2a	No. cases	17	14	19	22
	Diff.	4.1	4.8	5.2	5.0
	Sigma	.73	1.27	.950	1.00
2b	No. cases	21	15	16	24
	Diff.	1.8°	2.3°	1.8°	9.0°
	Sigma	7.46	5.54	7.39	14.6
3a	No. cases	18	14	21	24
	Diff.	4.8	4.7	4.9	5.3
	Sigma	.67	.66	.860	1.09
3b	No. cases	21	15	17	19
	Diff.	3.9	4.6	4.9	5.1
	Sigma	.98	.88	1.12	.89
5a	No. cases	18	14	22	17
	Diff.	4.0	4.6	4.8	4.6*
	Sigma	.88	1.05	1.08	1.40
5b	No. cases	21	16	17	10
	Diff.	4.4	4.3	4.6	4.8
	Sigma	.66	.99	.78	.62

* This distribution is distinctly bimodal with one maximum at step 3 and a higher maximum at step 6.

place they will appear only if the material prepared for testing by recognition contains a specimen which is more similar to the changed trace

⁷ K. Koffka, *Principles of Gestalt Psychology*, 1935, 494.

⁸ O. L. Zangwill, An investigation of the relationship between the process of reproducing and recognizing simple figures, with special reference to Koffka's trace theory, *Brit. J. Psychol.*, 27, 1937, 250-276.

⁹ N. G. Hanawalt, *op. cit.*, 68-70.

than the original. Moreover, in mass experiments, such material must be provided for different Ss. Finally, there are problems in scoring and interpreting the results. The present material could furnish some instructive examples of similar failures but it seems more profitable to confine

TABLE III

CHANGES IN REPRODUCTIONS OF DESIGN 4

Only the position of the Vs is charted. In the column to the left 1=V in the upper bar of the cross, 2=V in the right bar, 3=V in the lower bar, and 4=V in the left bar. 0 means no V. The original is thus 1200. Note that out of 16 possible combinations only 7 are found among 133 reproductions and only the first 4 combinations appear more than once.

Position of the Vs	Immed. 39 cases	3 days 28 cases	2 weeks 29 cases	6 weeks 37 cases
1200	100%	100%	86%	27%
1234	—	—	10.5%	40%
0000	—	—	—	16%
1030	—	—	—	11%
0204	—	—	—	3%
0034	—	—	—	3%
1230	—	—	3.5%	—

discussion to Designs 4 and 6, where the results were unequivocally positive and where the largest groups are available, since these two designs were common to both series of drawings.

The recognition-series for Designs 4 and 6 are shown in Fig. 2; the results are tabulated in Tables V and VI. It has been argued that the

TABLE IV

CHANGES IN REPRODUCTIONS OF DESIGN 6

The upper half of the table lists those cases in which the second copy had a slanting left bar; the lower half those with the left bar 'perpendicular or over.' The reproductions are split up according to the same criterion.

	Immed.	3 days	2 weeks	6 weeks
No. cases 2nd copy <88°	26	14	18	13
reprod. <88°	96%	93%	67%	23%
reprod. >87°	4%	7%	33%	77%
No. deteriorated	—	—	—	1
No. cases 2nd copy >87°	11	11	10	4
reprod. >87°	91%	91%	70%	100%
reprod. <88°	9%	9%	30%	0
Total no. cases	37	25	28	18

choice in recognition might be guided by the reproduction preceding it and that the agreement between the two tests might be a mere carry-over from one to the other. The two tables have, therefore, separate listings for the total of all recognitions and for those recognitions not accompanied by reproductions. The agreement between the two groups is as good as can be expected from such relatively small samplings. Only the 2-wk. and

6-wk. periods were split up in this way because after three days most Ss recalled all the designs.

Most of the few cases in which recall and recognition differ from each other seem to be due to lack of suitable material in the recognition series. In Design 6, for instance, there were 15 disagreeing cases out of 120 or 13%.

Except for the demonstration of the trend in recognition, such results

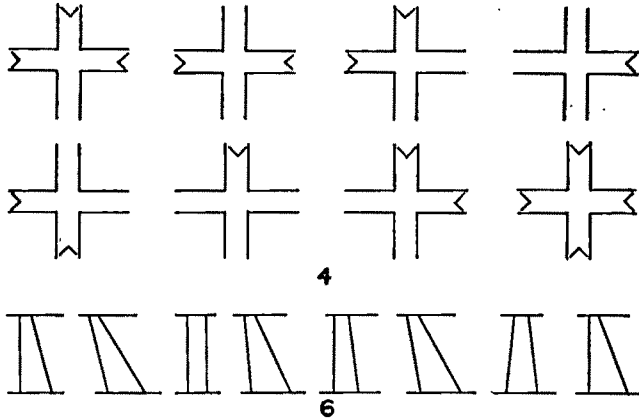


FIG. 2. SERIES OF DESIGNS FOR TESTING RECOGNITION OF DESIGNS 4 AND 6

are not new. They demonstrate, however, that even with Hanawalt's method the trend can be observed.

DISCUSSION

Fading. The theory of Hanawalt, also expounded by Woodworth,¹⁰ is based on two principles: association with familiar objects (*vide infra*) and "fading plus reconstruction." According to this latter hypothesis the trace fades out in time and the S tries to reconstruct the pattern by throwing in such completions of the incomplete trace as seem to him 'reasonable.'

It is difficult to confront this hypothesis with the Wulf-Koffka assumption that the forces set up in organizing the visual field remain effective and mold the trace into a more stable structure long after external stimulation has ceased. Since almost any change can be construed as a 'reasonable reconstruction' the decision will rest upon predictions in cases where the structure of the design is sufficiently known. Such knowledge has increased considerably since Wulf's paper and the following discussion is a restatement of some phases of the Gestalt theory. At the same time it

¹⁰ R. S. Woodworth, *Experimental Psychology*, 1938, 81.

tends to show how the theory can deal with the material in a less general fashion than an explanation by fading and reconstruction.

Perception of a visual field is a process of organizing the field into units according to the factors established by Wertheimer.¹¹ The resulting

TABLE V
RECOGNITION IN DESIGN 4

The numbers at the left have the same meaning as in Table III. Note that three asymmetrical designs of the series are not listed (cf. Fig. 3). They were never chosen by anyone of 119 Ss.

Position of the Vs	3 days	2 weeks		6 weeks	
	30 cases	Total 42 cases	No. reprod. 13 cases	Total 47 cases	No. reprod. 10 cases
1234	—	12%	23%	36%	50%
0204	—	7%	15%	21%	20%
1204	—	2%	8%	2%	—
1200	100%	79%	54%	30%	30%
1004	—	—	—	11%	—

units differ to a vast and measurable degree in strength of organization and this difference in turn influences the changes in memory. The whole-qualities of a strong unit are preserved or enhanced at the expense of those geometrical relations which do not constitute whole-qualities.

Not everything that appears together on paper is, however, a strong

TABLE VI
RECOGNITION IN DESIGN 6

Separate listing of those recognitions that were not accompanied by reproductions.
Cf. Table IV.

	3 days	2 weeks		6 weeks	
		Total	No. reprod.	Total	No. reprod.
No. cases 2nd copy < 88°	19	26	8	33	19
recog. < 88°	84%	73%	62%	30%	32%
recog. > 87°	16%	27%	38%	70%	68%
No. cases 2nd copy > 87°	11	16	6	15	11
recog. > 87°	64%	81%	100%	80%	73%
recog. < 88°	36%	19%	0%	20%	27%
Total no. cases	30	42	14	48	30

whole. Comparison of Designs 1a with 1b shows that 1b is perceived more strongly as a unit than 1a. The relationship of the curves in 1b is, therefore, preserved in memory much better than in 1a. A fading theory which disregards the structural differences between the two designs cannot explain this difference in the fate of the two traces. (A fading theory which does consider structural differences necessarily leads to the assumption of autochthonous stress.)

¹¹ M. Wertheimer, in W. D. Ellis, *A Source Book of Gestalt Psychology*, 1938, 71-88.

Much misunderstanding has arisen from the use of weak perceptual units in experiments on memory changes. The laws of wholes do not apply to mere perceptual aggregates. Although such loose arrangements are usually not entirely lacking in organization, they are, nevertheless, ambiguous in that several different directions of change result in memory. Wulf's patterns which Hanawalt used are mostly designs with little integration and, therefore, not well suited for group experiments which test strong trends common to several Ss.

In weak units two types of changes are possible: (1) *Changes of arrangement*. The parts may change very little, yet assume a more stable arrangement. Thus in Design 1a the parts arrange themselves more symmetrically, or in Design 6 the left bar assumes a vertical position between the two horizontals. (2) *Changes of parts*. A design may have a structure which strongly dominates the arrangement of the parts while the parts remain indifferent to the whole; the law of the whole does not reach down to the parts. For instance, in Design 2b the slope of the design may be preserved or enhanced, while the loops assume parallel positions or are replaced by material taken from a different figure, e.g. see Fig. 3, sample reproductions 1 and 8.

The direction of change in memory may be one toward a more consistent structure or toward a more strongly integrated whole. Several such 'tendencies' have been established, as, for instance, the tendencies toward closure and toward symmetry; but those tendencies can be regarded as operating only if their action is in the direction of increased consistency or integration of the whole. For instance, in Design 1a there is no tendency toward the closure of gaps as there is in Design 1b. In 1b, Wertheimer's factor of good continuation¹² unites the partial curves into a unit and, therefore, closure is consistent with the structure of the design, it is "prostructural."¹³

The 'tendency toward symmetry' likewise is conditioned by the principle of increased integration. Why do more than 70% of the reproductions of Design 4 become more symmetrical, while Designs 2a, 2b, or 6 show no such tendency? Evidently Design 4 becomes not only more symmetrical, but also in so doing, more integrated. The cross in Design 4 contains two pairs of empty spaces, a situation which demands that Vs be placed in either no pair or one pair or both pairs; and that is essentially what happens.

The symmetry in Perkins' material is of the same kind.¹⁴ His designs

¹² Wertheimer, *op. cit.*, 81.

¹³ Wertheimer, *op. cit.*, 84 f.

¹⁴ F. T. Perkins, *op. cit.*

consist of two parts which are symmetrical as to position but not as to shape, or vice versa. Any change toward increased symmetry at the same time constitutes an increase in integration by making equal parts symmetrical or symmetrical parts equal. An easy way of testing this interpretation would be to create an asymmetric modification of Design 4 which could achieve integration without becoming symmetrical.

A design may be fairly well integrated as a whole, yet not strong enough to have its last detail determined by the organization of the whole. In such cases changes of detail occur independently and in various directions, since there are usually several sets open to *S* and several possible stabilizations of the detail. It is entirely feasible to predict the actual directions and their relative frequencies, but that degree of certainty

TABLE VII
INCIDENCE OF CHANGE IN NUMBER OF DOTS IN DESIGNS 3a AND 3b

Design		Immed.	3 days	2 weeks	6 weeks
3a	No.	18	14	21	24
	%	6	7	14	29
3b	No.	21	15	17	23
	%	0	0	6	9

requires designs the properties of which have been studied extensively by other methods. A more modest achievement is the prediction of the relative frequencies of such changes in two related designs.

In Designs 3a and 3b there are, aside from changes of form already mentioned, many cases of redistribution and change in the number of dots. The frequency of changes in the number of dots depends on how strongly the structure of the whole design integrates the individual dots. Such integration is known qualitatively for these two patterns. In studies of the influence of different changes on the similarity of designs, dotted polygons like Design 3 were investigated.¹⁵ One finding was that, the more acute the angles, the more function rests on the individual dot and the less the dots merge into what is perceived as 'material'.¹⁶ In the cases of Designs 3a and 3b we should expect individual or 'form' function to be more pronounced in 3b, which has acute angles, than in 3a, which has obtuse angles. Correspondingly in 3b the number of dots should change less readily than in 3a. This expectation is confirmed by the experiment (Table VII).

Such analyses show how the direction of the change can be predicted

¹⁵ E. Goldmeier, Über Ähnlichkeit bei gesehenen Figuren, *Psychol. Forsch.*, 21, 1936, 146 f.

¹⁶ E. Goldmeier, *op. cit.*, 177 f.

from the action of the same forces that create perceptual organization. Gestalt theory assumes that these forces do not cease to act once the trace has formed, but that they continue to effect changes until a stable structure is reached.

Association with familiar objects. Although it is difficult to explain the details of directed changes by fading-plus-reconstruction, it is equally

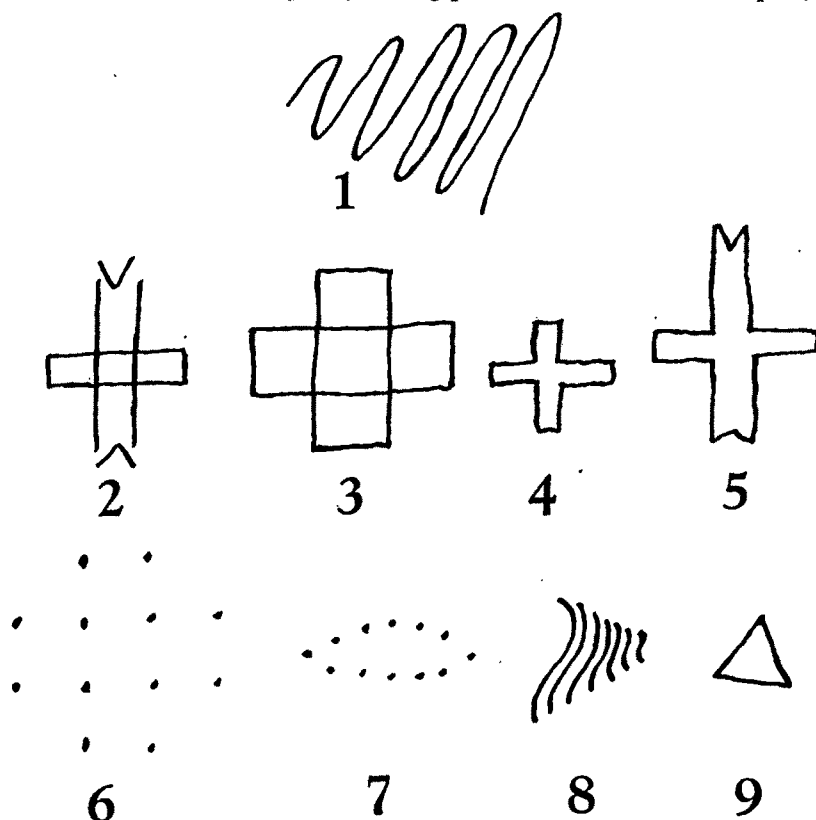


FIG. 3. SAMPLE REPRODUCTIONS OF THE DESIGNS

Sample 1, reproduction of Design 2b; Samples 2-5, simplification, elaboration, and closure in reproduction of Design 4; Samples 6-9, assimilative reproductions (Sample 6, material from Design 3b with form from Design 4; Sample 7, material from Design 3b with form from Design 1b; Sample 8, material from Design 1b with form from Design 2b; and Sample 9, material from Design 6 with form from Design 5a).

difficult to adduce proof for the second assumption frequently made: fading-plus-outside-associations. It is true that practically all these reproductions have shown some degree of outside influence. From such findings

it has been argued that 'familiar objects' are associated to the trace, establishing themselves more firmly as time passes. The inference is that a *familiar* object naturally would gain predominance because it has occurred frequently in past experience.

This opinion is well founded in associationist theory but very little in experimental fact. Gibson gives a list of objects associated to designs in his experiment. While some of these objects might be accepted as familiar ones, many certainly are far from it: "Woman's torso, footprints in the sands of time, battle axe, loaded doll, blastula stage, pair of horns." Why should such unfamiliar concepts gain predominance?

Neither has the fact of predominance been established. Carmichael, Hogan and Walter¹⁷ and Hanawalt and Demarest¹⁸ have shown that in *ambiguous* patterns outside material directs the change if introduced strongly by E, especially if the availability of the trace is lowered by using a series of 12 drawings. The fact that the organization of ambiguous designs *can* be directed does not imply, however, that *spontaneous* organization of less ambiguous patterns *is* directed by *spontaneous* affiliation, nor does it prove these affiliations to be stronger than autochthonous tendencies.

The question can be answered by the statistics of the distribution of outside material. If association were the moving force, there should be a stable distribution of each type of outside material in all verbal reports at the different periods. *If the outside material is affiliated according to structural similarity, then the trend in the trace will correspond to a trend in the material affiliated.*

In Design 4 there are a number of reports likening the design to a street intersection. After three days 21% of the reports mention "street intersection," but, after two weeks and six weeks, this figure drops to only 5% in each group of reports. On the other hand, there is one type of change which destroys the similarity to a street intersection, the closure at the ends or continuation through the center (see Fig. 3). This change is found after three days in 3% of the reproductions, after two weeks in 11%, and after six weeks in 37%.

This result stands up no matter at what time the outside material was affiliated to the trace. The reports were given after the reproductions were

¹⁷ L. Carmichael, H. P. Hogan and A. A. Walter, An experimental study of the effect of language on the reproduction of visually perceived forms, *J. Exper. Psychol.*, 15, 1932, 73-86.

¹⁸ N. G. Hanawalt and I. H. Demarest, The effect of verbal suggestion in the recall period upon the reproduction of visually perceived forms, *J. Exper. Psychol.*, 25, 1939, 159-174.

made. If they originated at the time of learning, then the association with street intersections must have disappeared when the pattern changed to more stable forms; if it accrued later, it was added only as long as it fitted in with that stage of the changing trace.

Statistics as required here are difficult to compile from unguided introspections. Many Ss have nothing to report spontaneously and urging may provoke confabulatory reports; many of them report only generalities ("interesting," "simple"); and, finally, the classification of pertinent reports is not always easy. Thus the number of 'diagnostic' reports of one category is small. The figures given above are based on a total of only 54 reports obtained with 94 reproductions of which five (9%) mentioned street intersections. These factors combine to decrease the validity of single findings. On the other hand, if similar instances are found, the validity of even small samplings increases. Therefore, another such analysis may be added.

There were some reports likening Design 6 to a Roman numeral, while others dealt with slant or convergence of the verticals. The significance of either type of statement is open to discussion but this much is certain; if the reports are pointing to motive forces, their distribution should be stable; if they reflect the condition of the trace, depending on its actual state at the time of introspection, there should be a trend; and, if there is a trend, then both types of associations can not themselves be the factors bringing about the change. After three days, two weeks and six weeks there was a total of 13, 15, and 11 such reports respectively. Slant or divergence was noted in 15%, 20%, and 64% of the reports respectively; and Roman numerals in 23%, 13%, and 0% respectively. These are small numbers but show again a strong trend in both types of reports.

Decreased availability of traces and the effect of outside influences. Two common misconceptions of the Gestalt theory of traces are (1) that it denies forgetting and (2) that it denies the occurrence of outside influences upon a trace. To bring out the disputed autochthonous changes, however, an experiment must be so designed as to reduce forgetting and outside influences since they both interfere with autochthonous change. Forgetting in such memory experiments is to a large extent not due to disappearance of the trace but only to decrease in its availability. The investigations by Köhler and Von Restorff¹⁹ indicate that there are three factors which reduce the availability of a trace: (1) long series of designs, (2) uniformity or similarity of designs, and (3) the use of designs which

¹⁹ W. Köhler, *Dynamics in Psychology*, 1940, 139-144; Köhler and H. von Restorff, *Zur Theorie der Reproduktion*, *Psychol. Forsch.*, 21, 1935, 56-112.

have no firm structure. All three of those factors are found in Hanawalt's material to a greater extent than in the material used here: eight as against six designs to a series and obviously in Hanawalt's experiment, more uniform and less structured designs. As a result the number of designs remembered is about 10% less after different intervals of time than in this experiment. The same differences become apparent by comparing previous investigations, as Allport's survey shows.²⁰ Such comparisons make it obvious that the same conditions which increase unavailability also increase outside influences on the trace.

The reason why poorly structured material has that effect can be gathered from introspective reports. Gibson used very long series of simple designs and his observers looked for "objects by means of which the figure could be 'understood'."²¹ This procedure led to an abundance of verbalization (change in the direction of associated words) and of object assimilation (change in the direction of an associated object). Along with verbalization and object assimilation there is a corresponding amount of figure assimilation (combination of features of different designs in one reproduction), which also occurs with decreased availability of the trace and seems to be due to partial availability of traces.

Gibson, whose material is most favorable to operation of outside influences, also found many reports indicating the importance of structural properties of the designs, such as: "very irregular, medieval, jutting out" and "thing with an indentation." Again it is not familiarity that provokes affiliation of objects and verbal material but the structural properties of the material, which help to overcome reduced availability by strengthening the organization of the design. In addition, there exists a different mechanism, not considered here, where the trace is completely or almost completely lost but some previous association is remembered, and a reproduction is arrived at by guessing what the association might have stood for. These cases are easily recognized by the dissimilarity of the reproduction from the original.

In the present experiment there were four cases of figure assimilation, two of object assimilation and 10 of verbalization. The low incidence of outside influence conforms to the different material used. The 10 cases of verbalization were excluded from Table II because they obviously represent a different mechanism. Their exclusion did not alter the table materially for they constitute only 1.2% of the data.

²⁰ G. W. Allport, Change and decay in the visual memory image, *Brit. J. Psychol.*, 21, 1930, 133-148.

²¹ J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

The four cases of figure assimilation (Fig. 3, samples 6-9) are remarkable because, in at least three of the four cases, the combination is one of 'material' from one design with the 'form' from another. Apparently with more complex material, such as is used here, the traces break up at a point which is 'prostructural'.²² If a trace becomes partially available, the separation of the form from the material is the one most easily done without destroying whole properties.

Figure assimilation is independent of other autochthonous changes which may occur simultaneously. For instance in Fig. 3, sample 3, the cross becomes symmetrical by losing the Vs, the curves in Fig. 3, sample 8, lose their gaps, and the pattern of Design 1b appears closed in Fig. 3, sample 7. (See the legend to Fig. 3.)

SUMMARY

(1) The present experiment is in its method a close repetition of an experiment by Hanawalt. The chief difference lies in the material employed. The designs here are more complex, more diversified as to both material and form, and the number of designs is reduced to six in a series. These factors combine to increase the availability of the trace as well as the strength of the resulting organization.

(2) Under these conditions autonomous changes appear clearly. The trend in the changes was found both by reproduction and by recognition, and good agreement between the two methods was noted as far as the material allows of comparison.

(3) Autonomous changes are directed toward outstanding or *prgnant* values and toward consistent structure of the whole. Both these tendencies express merely the continuity of action on the trace of those forces which bring about visual organization.

(4) Outside material enters into relationship with the trace, not by merit of its hypothetical familiarity, but on the basis of structural kinship with the trace. An analysis revealed that there is a change in the type of effective outside material, corresponding to a trend observed in reproduction and recognition.

(5) The failure of Hanawalt and others to find autonomous changes is due to the type of material used—chiefly to their use of too extensive series of drawings, too similar and unstructured designs, and their failure to anticipate correctly the change in the material for recognition.

²² Wertheimer, *op. cit.*, 71-88.

ARTICULATION IN AUTOMATIC MENTAL WORK

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Early psychologists regarded inner speech, or articulation, as a coordinated habit of all mental activity, such as thinking, reading, spelling, writing, speaking, memory recall, dreaming, and hallucinations. There was, however, some opposition to this theory as might be expected. Verification of the conception in thinking came at the turn of the century in experimental studies of muscular movements of the throat. The results of this research implied that movements of the tongue existed during thought.¹ Various motor theories were formulated upon this basis, of which the more popular as recalled today were Dunlap's reaction hypothesis, Watson's motor theory of thinking, and Washburn's motor theory of consciousness.

These theories and the early findings were contradicted by later experiments the results of which indicated that laryngeal and tongue movements are not universally present during thought.² When thinking was equated with speech they were found not to be the same.

All the investigations into muscular movements of the throat during thinking are, however, open to question on technical grounds. A criticism of them has been made by Max who states that the results are equivocal because of the inadequacy of the measuring instruments.³ Thus nothing exactly is known of articulation during thinking.

Bain and Stricker had maintained that inhibition of articulative movements would arrest the reading process. It was demonstrated in early studies that both silent reading and retention were decreased when the Ss were required to repeat numbers aloud, or the syllable 'la,' or to hold the breath, or repeat the alphabet, or to whistle, which supported this conclusion. Contradiction of it is found in research by Pintner who required 2 Ss to repeat "13, 14, 15, 16" during silent reading, in order to inhibit the presence of reading articulation, and compared 63 observations under these conditions with 20 observations of ordinary reading before and 20 after.⁴ He found that after practice such reading was as efficient in rate and retention

* Accepted for publication January 14, 1941.

¹ H. S. Curtis, Automatic movements of the larynx, this JOURNAL, 11, 1899, 237-240; H. C. Courten, Involuntary movements of the tongue, *Yale Psychol. Studies*, 10, 1902, 93-96; A. Wyczikowska, Theoretical and experimental studies in the mechanism of speech, *Psychol. Rev.*, 20, 1913, 448-458.

² H. B. Reed, The existence and function of inner speech in the thought processes, *J. Exper. Psychol.*, 1, 1916, 365-392; R. S. Clark, An experimental study of silent thinking, *Arch. Psychol.*, 7, 1922, (no. 48), 1-96; A. Thorson, The relation of tongue movements in inner speech, *J. Exper. Psychol.*, 8, 1925, 1-32.

³ L. W. Max, An experimental study of the motor theory of consciousness: I. Critique of earlier studies, *J. Gen. Psychol.*, 11, 1934, 112-125.

⁴ Rudolf Pintner, Inner speech during silent reading, *Psychol. Rev.*, 20, 1913, 129-153.

as ordinary reading and he concluded that reading without articulation can take place. This investigation and those indicating that articulated lip movements are detrimental to reading efficiency are the experimental basis of current ideas of instruction in reading in which articulation is regarded as a superfluous coördination. As in thinking, so also in silent reading, little exactly is known regarding the part played by the motor functions.

Related investigations into learning approached the problem from a different angle in a study of the efficacy of methods of stimulation of the senses, such as the visual, visual-motor, auditory, auditory-motor, visual-auditory, visual-auditory-motor presentations. Where the motor presentation was used the learner was asked to articulate each syllable or number to be learned. Results of numerous studies with this approach are not in accord and they are about equally divided between those reporting superiority for learning by methods of presentation that include articulation and those that do not.

Interest has changed in later investigations from the study of imagery types in learning to the pedagogical problem of how much is learned in various situations. More is learned with a certain number of recitations alternated with readings. Recitation involves articulation, of course. Forced articulation was found to be beneficial for children in learning to spell, but not for adults. Recently, Woody⁵ found that oral learning of poetry was superior for college students and Barlow⁶ reported critical ratios of 3.86 for school children and 3.64 for college students in favor of forced articulation in learning a 20-word list of 3-letter nonsense syllables.

There is but one piece of research reporting articulation in mental work where the process has reached a high degree of automaticity. Morgan observed that movements of the lips were present during noise where letters were being translated into a code.⁷ He believed that such articulations enabled the Ss to overcome an initial decrease in performance when noise was introduced and to effect a later increase over that of the normal quiet conditions. Assuming that articulations lengthen the breathing ratios, he shows that Ss whose breathing ratios did not lengthen did not increase performance during noise. Whether articulation always was present in the performance was not indicated by the investigation, but it appeared in certain individuals under noise stimulation.

A review of the literature indicates that our knowledge of the function of articulation is inadequate to the formulation of any theory or to the establishment of any practice for instruction. Evidently individuals carry on all forms of conscious activity with and without articulation and they differ greatly in their use of articulative coördinations. We need experimentation upon this problem throughout a wide area of mental tasks and there follows a series of minor experiments aimed at understanding one automatic mental function as influenced by articulation.

⁵C. Woody, The effectiveness of oral vs. silent reading in initial memorizing of poems, *J. Educ. Psychol.*, 13, 1927, 477-483.

⁶M. C. Barlow, The rôle of articulation in memorizing, *J. Exper. Psychol.*, 11, 1928, 306-312.

⁷J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, (no. 35), 1-84.

EXPERIMENT I. ARTICULATION INHIBITED

The object of the first experiment was to discover whether articulation could be inhibited by untrained Ss and, if so, whether its inhibitions affected the Ss' performance.

Method and procedure. The Ss were given Fryer's *Speed Addition Test*,⁸ which consists of 2400 problems in addition with totals of less than 10. The problems are distributed in chance order and are arranged in rows of 30 which are broken by double spacing between every two successive sets of 10. Performance in this test is considered to be automatic without immediate practice. In part of the additions the Ss were allowed to add in their usual manner and in part they were instructed to inhibit articulation.

Subjects and instructions. Nine graduate students were used as Ss. Their instructions were as follows:

Articulation in addition can be thought of as the formation of the answer with the lips, mouth, or throat. You are asked to add the problems placed before you as rapidly as possible in your usual way, until I whisper, "Now," after which you are to continue the adding without any articulation, *i.e.* you must write the answer without forming it verbally in your throat, mouth, or lips. In other words, you must not write any answer you articulate but write as many as you can without articulation (cross out those you write where you articulated). When I say "Now," draw a vertical line where you begin work without articulation. Start each test in your usual way and work as fast as you can.

A control period of 20 sec. was followed by an experimental period of 10 sec. in which articulation was prohibited. Six tests were administered.

Results. Results were computed as individual averages (*i.e.* means) for the last five tests in terms of the number of problems performed per minute. The average per minute for normal addition was 98.1 and for addition when articulation was prohibited it was 36.9. The critical ratio of the difference between these averages is 4.15.⁹ When articulation was prohibited, the average individual performance was 33.6% of the normal; or stating the result conversely, inhibition of articulation decreased performance on an average 66.4%. Of the 9 Ss, 6 showed decreases of more than 50%.

At the conclusion of every one of the six tests, the Ss were asked to answer the following question: "Could you continue writing the answers to the problems without articulation?" "No" was reported in 60% of the tests by all the Ss. Of the 9 Ss, 5 reported "no" in all of the tests. In explanation of the basis of their answers, one S stated that his additions "were conscious;" a second said that he had decided that he articulated because of feelings of movement in the mouth and throat. A third stated

⁸ Douglas Fryer, *op. cit.*, 1932. This test is published by, and may be obtained from, the Department of Psychology, New York University.

⁹ The CRs reported here and in later experiments were computed from σ_{d1s} , with σ_M by the formula; $\sigma_{d1s}/N^{1/2}$; and the σ_{d1r} by the formula: $[\sigma_{MA}^2 + \sigma_{MB}^2]^{1/2}$.

that he used pressure on roof of the mouth as the criterion in deciding that he articulated. The remaining two stated that they formulated the answers verbally. Three of the 9 Ss reported "yes" to the question, stating they could perform when articulation was prohibited. In explanation, one S reported that he contracted all vocal apparatus and he was not aware of any movement. The second stated that he held his mouth open rigidly when instructed not to articulate and that he was therefore sure that he had not done so. The third S, who reported "no" in 80% and "yes" in the remainder of the tests, explained his basis of the "yes" reports as the absence of tension in his speech mechanism.

The criteria by which the Ss judged whether articulation was used and was necessary vary, as we see, considerably. It seems probable, even with the exacting instructions and training in psychology of one or more graduate years, that these Ss were unable to decide with any exactness as to the inhibition of articulation. None of these Ss had any training in introspection and it is evident from these results that training is necessary for reports of this kind.

EXPERIMENT II. AWARENESS OF UNINTENDED ARTICULATION

The purpose of the second experiment was to discover whether S is aware of articulation when working normally, that is to say, when articulation is unintended.¹⁰ The author has searched through hundreds of descriptive introspections of awareness during written performance, particularly in simple addition, without finding an answer to this question. Articulation is infrequently but occasionally reported where there is no intention to articulate. If existing it is not in the focus of awareness.

Subjects and instructions. To test this question, highly motivated conditions were established and 4 Ss, F. C. Bartlett (*B*), G. C. Grindley (*G*), M. D. Vernon (*M*), and P. E. Vernon (*P*), highly trained in introspection, served without preliminary practice in the task. Introspections of a general descriptive nature were requested by instructions placed before the observer, which follow:

Report verbally following each test your awareness of speed, or movement, or push, or drive, or motive. Start with the last awareness as you finish the test and work backwards to the beginning of the test in recalling these awarenesses.

It will be noted that S's attention was not focused by these instructions upon the factor under study and he would not be expected always to report the presence of articulation if other conscious factors appeared more important while recalling the working process. He was uninformed of the purpose of the experiment.

The working instructions were varied from time to time during the tests to insure

¹⁰ This and the following experiment were performed in the Psychological Laboratory of the University of Cambridge, England.

a continued development to the highest possible motivation. *S* was asked to race against time in the preliminary instructions. Knowledge of his own results was later given in order to develop self-competition, and finally, the knowledge of the results of other *Ss* with higher records was given in order to develop group competition. All tests were timed at 1 min. with the exception of those with the preliminary instructions where *S* was required to perform 120 problems. The total number of tests varied for the different *Ss* from 29 to 76. These were given at the rate of 10 a day.

Results. Table I shows the results of Experiment II. In only 19% of the 210 introspections by the 4 *Ss* was articulation reported. The introspections suggest, however, that articulation may be frequently conscious during written performance. There were wide individual variations in the num-

TABLE I
NUMBER AND PERCENTAGE OF ARTICULATIONS IN INTROSPECTIONS

S	No. of introspections	Articulations	
		No.	%
B	40	29	73
M	76	2	3
P	29	9	31
G	65	0	00
Av.	52	10	19

ber of articulations reported: one *S* reported articulations in 73% of 40 tests and one reported no articulation in 65 tests.

Statements concerned with articulation of the problems of the task are abstracted from the introspective reports.

B reported articulations in 7 of the 15 tests performed while racing against time under the preliminary instructions. He says, regarding those cases: "I articulate sums before writing them; not figures to be added. There is just a little verbalization of the figures." In only one test did he report "no awareness of articulating." Under self-competition, he reported articulation in 8 out of 9 tests, as follows: "I am aware of articulation of the totals. Started with verbalization, 'I must go on,' between the articulation of the totals. Aware of articulation of sums throughout and no articulation of anything else." Under group-competition, he reported articulation in 14 of the 16 tests. He said: "I think that I do better when other awarenesses do not come in. Am articulating every sum." *B*'s reports show that articulation increases as motivation increases and, furthermore, that articulation is intentional.

M reported articulations in only 2 of 76 tests but says, "I was aware that I verbalized the answers as I wrote them." After one test she reported: "No articulation; additions seem to do themselves."

P reported that there were no articulations and that the process was automatic to the fourth test at which point he stated: "There is articulation now of each number as I put it down." Under self-competition he reported: "Articulation of sums is very much more laryngeal. Each number is almost whispered. I am urged

forward by verbalization of sums. I articulate all sums." Later, under group-competition, he reported, however, that "I am not very aware of articulation of sums."

G did not mention articulation in 65 tests. His reports were the least detailed, it should be remarked, of all the Ss.

To a fifth S,¹¹ 40 tests, every one consisting of 600 problems, were given without knowledge of the results. These were followed by 16 under self-competition. All other conditions were the same as those employed with the other 4 Ss. In 17 or 30% of the tests, articulation of the answers was reported as is indicated by the following typical introspection: "Am reading figures of next problem while articulating and writing answers. Focal attention on articulation. Developed motive to keep up fast articulation in advance of writing and felt that this was the method producing greatest speed."

Whereas instructions in this experiment did not focus intention upon reporting articulation such intention was adopted by the 3 Ss reporting it in any great amount. Changes in instructions to self- and to group-competition did not reduce the number of articulations reported by these Ss. Rating the reports for fullness and precision of detail places the same 3 Ss the highest. Considering this evidence, it would seem that articulation is frequently conscious in automatic mental work and, furthermore, that Ss reporting it intend to observe it.

EXPERIMENT III. FORCED AND INHIBITED ARTICULATION

In view of the results obtained in Experiment II it seemed worth while to investigate the effect that intent (1) to articulate and (2) to inhibit articulation has upon trained Ss. It was thought that the results would cast light upon the rôle played by articulation in the normal performance.

Subjects. Seven Ss, J. M. Blackburn (*Bl*), K. Edwards (*Ed*), D. H. Fryer (*F*), and B, G, M, and P who served in Experiment II, were used in this study. They were all trained in introspection and were all highly practiced in the observations required in this study.

Procedure. All the tests were of 1-min. duration. Immediately after the completion of a test, the Ss were directed to give their introspections in accordance with the instructions of Experiment II.

Preliminary to the main tests several preliminary tests were given the Ss under 'racing instructions' and the average performance of the Ss in these tests were later used as norms with which the results of the main tests could be compared. The Ss' norms averaged 130 problems per minute. The previous highest records of these Ss averaged 135 problems per minute. The difference of 5 problems per minute has a critical ratio of 0.53 which indicates that the norms are representative of our Ss' practiced performance.

Instructions. Instructions for the tests requiring articulation, which followed immediately after the preliminary tests were as follows: "Articulate rapidly and follow previous instructions to accomplish as many problems as possible." All the

¹¹ That is E prior to establishing the purpose of Experiment II.

Ss reported that they followed these instructions during the course of the tests and that they intended to articulate the answers to the problems.

Instructions in the tests prohibiting articulation, which followed the forced articulation-tests, were: "You must not articulate but work as rapidly as you can." During the last three tests, the following instructions were added: "Skip (do not write) all problems you articulate. Work, however, as rapidly as you can." The introspective reports show that the Ss followed these instructions.

Results: (a) With forced articulation. The average for 15 tests under forced articulation was 128.1 problems per minute, a difference from the normal score of -1.9 problems. The critical ratio of this difference is 0.18, which indicates that performance in the tests with forced articulation was not significantly different from normal performance. Average individual performance for tests given first, second, and third, were 95.9%, 99%, 104%, respectively, of normal performance. For all the tests together it was 98.9%, or a decrease from normal performance of 1.1%.

Intended articulation was judged by all of the Ss to be similar to that during normal performance. Statements abstracted from the introspections will make this point clear.

Ed reported that: "Above all, articulation was focal. Am articulating deliberately one ahead. With any irrelevant awareness there is a break, after which articulation is stronger. In background are verbalizations to get ahead."

Bl believed that the process was the same as normally but more "definite." He reported: "Normally I pronounce the sum as I write, but it is linked with auditory imagery."

B likewise, reported that normally he articulated all totals at the time of writing. He says: "That is evidently what I am doing, articulating totals at the moment of writing, but I think I look ahead normally."

M believed that articulation was normal in the task, but says: "Don't think I usually articulate each separately; they slur together. Having articulation at focus retards speed and at high speed am scarcely aware of any vocalizing, but strong kinesthesia in mouth."

P stated that the process was not greatly different to normal work but that conscious focus of articulations was much greater. He states: "Started with lip movements that continued throughout. In this intended articulation, awareness of task was not so far ahead as usual. Breathing was difficult. Had to slow down writing to articulation."

F reported difficulty with intended articulation in speeding up where possible before—e.g. where zeros exist—but otherwise the work was the same as normally.

G wondered if he usually articulated as he did here and came to the conclusion that conscious focus was greater than normally. He usually reported fewer awarenesses of the task than he reported here.

It is interesting to note (a) that *B*, *P*, and *F* had consistently reported articulation in normal performance in Experiment II; (b) that *M* and *G*, who had not, now came to a similar conclusion that articulation was present in normal performance,

and (c) that *Ed* and *Bl*, who did not participate in Experiment II, arrived at the same conclusion. *Ed*'s and *Bl*'s results stand in striking opposition to the fact that they had not mentioned articulation in 94 and 59 tests, respectively, that were made previously.

The results of the first part of Experiment III indicate: (1) that automatic mental work was not decreased greatly by forced articulation; and (2) that our *Ss*, who were trained in introspection, believed that articulation was normal to the task but that when forced, as it was here, it had a greater conscious focus inhibitory in its effect.

(b) *With inhibited articulation.* The average for 15 tests under the conditions of inhibited articulation was 67.6 problems per minute. The difference with the mean of normal scores is 66.8 problems per minute and the critical ratio of this difference is 4.86. Average individual performances for tests given first, second, and third, were 59.2%, 50.6% and 22.5%, respectively, of normal performances, and for all tests 50.8%, or a decrease from normal performance of 49.2%.

The objective results cannot be taken at their face value as is evident in the introspective reports.

Ed found out to his great surprise that he could not perform without articulating. He reported: "A tremendous effort to follow instructions and a tendency to disregard them. Tried to get a detached attitude and depend on movement of hand. When instructions came to 'skip all you articulate' it just dawned on me that I always articulate. I look at the problem and as I write the sum I verbalize it." He concludes that every one he performed he articulated.

Bl thought that he might be able to write the sums without articulating if he went rapidly.

B reported: "I tried all sorts of things. I tried to look real hard at the figures. I tried to go faster, thinking: If I go faster I won't articulate. It was no good. I do not think I did a sum without articulating."

M reported: "I started to recite the alphabet, then didn't know what happened. Adding went on without awareness of it. Don't know if I articulated in oscillation of attention. I would estimate I was verbalizing the sums throughout."

P thought it was possible to carry on adding without awareness of articulation and reported that: "The only way I felt I could do it was to think of other things, but that failed and I counted. When in difficulty I articulated but for easy ones there was no distinguishable articulation."

F stated that writing stops with inhibited articulation.

G failed to perform with instructions. He was given instruction: "Write 10 after which don't write any you articulate." Again he failed to perform after the first 10 problems.

If we are to accept the evidence of these introspective reports it is impossible to perform the task without articulation. But there is evidence indicating that the worker may become aware of other things to a degree

that he is not sufficiently aware of the articulation to report it. In this we may have an explanation of the usual situation in automatic performance. It is quite possible, also, that instructions to note the presence of articulations made them focal where the process was normal without awareness of them. It is implied here that previous experimenters inhibiting articulation by a pencil in the mouth or repetition of words and figures have not investigated the problem, for their Ss who usually were school children, were unable to indicate whether or not articulation was present in performing a task.

EXPERIMENT IV. EFFICIENCY WITH FORCED ARTICULATION

A group experiment was planned to measure the relative efficiency of normal work and work with forced articulation.

Conditions. Three 1.5-min. tests were administered to 6 groups of college students (see Table II) and instructions to articulate were included at the second test. These groups were psychology laboratory classes, and four were selected from one year (Groups I-IV) and two from another year (Groups V and VI) to check on any influence of social groupings. Also, the task was administered in 2-min. tests to a control sample consisting of 182 college applicants with the articulation instructions omitted to secure a validation of the method used in the treatment of the data of averaging scores in first and third tests as a norm for the second test.

Instructions. General instructions for all groups in all tests were to complete as many problems as possible in the time allowed. The following instructions were given at Test 2 to Groups II, III, IV, V, and VI called the "Inaudible Groups:" "Work as rapidly as possible—just as you have been doing. But this time you are to say the answers to yourself as you write them—not out loud, but in almost a whisper so that you know you are doing it. This is called articulation and you will be asked at the end of the test if you articulated all answers. Group I, called the "Audible group," received instructions in the second test "to speak the answers to each problem aloud." In this instance the answers were definitely spoken, not whispered. The records of all the Ss not following instructions according to their own statement were discarded.

Results. Table II presents the average number of problems performed by the six experimental groups. The column headed "articulative performance" includes the actual number at Test 2 and the column headed "normal performance" includes the expected normal performance at Test 2, computed from individual averages of the preceding and following tests (Tests 1 and 3). The significance of the differences is shown at the column to the right. Where the sign is plus it denotes that the difference is in favor of normal performance.

The difference in Test 2 between normal performance as thus defined

and actual performance with forced articulation is slightly in favor of normal performance, which is indicated in the combined results for Groups II to VI where the difference over the sigma of the difference is 0.54. Any importance attributed to this difference in the direction of normal performance is discounted because the experimental groups of the two years (Groups II—IV and V—VI) show opposite effects of articulation on performance. Its importance is emphasized by an artifact of method indicated in an analysis of the control sample where $D/\sigma_{diff.}$ between averages of Tests 1 and 3 and actual normal work in Test 2 is 1.2/2.8 or 0.44 in favor

TABLE II
AVERAGE PERFORMANCE AND DIFFERENCES WITH AND WITHOUT FORCED ARTICULATION
(Audible articulation used by Group I.)

Group	No. Ss	Normal performance	Articulative performance	$D/\sigma_{diff.}$	CR
I	23	145.6	149.0	3.4/5.9	.57
II	24	154.3	151.4	+2.9/5.5	+.52
III	22	147.3	150.9	3.6/5.8	.63
IV	23	140.0	141.0	1.0/6.5	.15
II-IV	69	146.9	148.1	1.2/2.9	.41
V	14	160.6	154.7	+5.9/6.2	+.95
VI	20	154.4	149.0	+5.4/7.7	+.70
V-VI	34	157.0	151.4	+5.6/5.2	+1.70
II-VI	103	150.5	148.9	+1.6/3.0	+.54

of Test 2. This suggests that the true critical ratio might be as high as 0.98, instead of 0.54, in favor of normal performance.

One may conclude from these results that there is likely to be a slight inhibition upon performance with forced articulation. But this inhibition may be an effect of greater conscious focus on articulation than exists in normal automatic work.

EXPERIMENT V. EFFICIENCY IN PRACTICED ARTICULATION

A group experiment was planned to trace the relative efficiency of normal performance and performance with forced articulation throughout an extended practice period. This should answer the question of whether or not forced articulation provided the same inhibition after practice that it did at the beginning of work. It should indicate if learning was greater in articulative or normal performance. It might indicate if forced articulation is an exaggerated or artificial condition of a normal situation.

Conditions. Twenty-seven ½-min. tests were administered in one experimental period to a group of 24 college students who were willing to cooperate. Instructions were read before the first three tests and every fifth test thereafter, as follows:

Instructions. Work as rapidly as possible. Attain the highest possible speed in each test, which will be half a minute duration. Also, attain the highest possible speed by the last tests of the day's work when your best record will be placed on the board so that you can see who has the highest record of all members of the class. Set a goal before starting each test to beat your last highest record. Try to feel you can beat it, and you usually can!

The 1st, 3rd, 5th, and other odd numbered tests, are regular work tests in which you are to do the work in your usual manner of adding without being concerned with how you do the adding. The 2nd, 4th, 6th, and other even numbered tests, are articulative tests, in which you are to add according to prescribed directions.

In the articulative tests you are to say the answers—not out loud, but with lip motions, in an inaudible whisper—so that you know you are doing it.

Every test will be introduced with the statement of either: "Regular method" or "Articulative method," followed by the statement "Set Goal" and then the bell. Begin with the bell and stop with the bell by lifting your pencil so—[demonstrated].

Following each test you will be asked to write "Yes" or "No" as to whether or not you followed instructions, and if you write "No," tell what you did. Number each test as directed.

If you find that the "Articulative method" is slowing you, you must continue it for all even numbered tests, but you will have an opportunity to make a record in the odd numbered tests when you can work with the regular method.

This last statement may have determined motivation in favor of normal work. Only six scores were discarded from the results with forced articulation where "No" indicated that *S* did not follow instructions.

Results. Results are shown in Table III comparing normal and articulative performance. The scores in Tests 1 and 2 were omitted from the results as a period in which experimental conditions were being established. All scores of following tests were computed as a percentage of Test 3 for every *S* to equate for individual differences in level of performance. Test 3 was a test of normal performance. The percentage scores in the odd-numbered test (normal performance) immediately preceding and following each even-numbered test (articulative performance) were averaged to indicate expected normal performance in the even-numbered tests. Normal performance, as thus defined, is shown in the second column of the table, articulative performance in the third column, and the significance of the differences between these two types of performance in the various tests is shown in the columns to the right.

The differences in all the comparisons in Table III are in favor of normal performance. Of all the *CR*s, however, only one is significant and only two others are close to significance. The significant *CR* (Test 4) may, furthermore, be due to the method of computing the differences in performance. Normal performance in Test 4 was the average of Tests 3 and 5 and the performance in Test 3 was arbitrarily taken as the 100% point. The difference in absolute scores was computed for this comparison which gave a $D/\sigma_{diff.}$ of $3.7/2.46$ which yields a *CR* of 1.50. Evidently then the one significant *CR* in Table III is an artifact of the method of computation. The same method of computation for Test 2 and the average of

Tests 1 and 3 gave a $D/\sigma_{diff.}$ of 3.4/2.46 which gives a CR of 1.4 in favor of normal performance. None of the CR s between the tests of the practice period was significant, but these results imply that normal performance is

TABLE III
AVERAGE PERCENTAGE NORMAL AND ARTICULATIVE PERFORMANCE OF 24 Ss AND THE SIGNIFICANCE OF THE DIFFERENCES BETWEEN THESE TWO TYPES OF PERFORMANCE IN THE VARIOUS TESTS

Tests	Normal performance	Articulative performance	$D/\sigma_{diff.}$	CR
4	99.5	93.4	6.1/1.3	4.66
6	100.6	98.9	1.8/2.1	0.86
8	101.3	100.1	1.2/2.1	0.56
10	101.7	97.0	4.7/2.3	2.01
12	101.6	96.7	4.9/2.5	1.96
14	101.5	99.4	2.1/2.2	0.97
16	104.5	99.9	4.6/2.5	1.87
18	106.1	102.8	3.3/2.9	1.16
20	104.9	102.8	2.1/2.4	0.89
22	104.8	101.1	3.7/2.5	1.47
24	106.9	100.6	6.3/2.8	2.29
26	106.8	105.2	1.6/2.7	0.60

likely to be superior to articulative performance at all times in automatic mental work.

Extent of inhibition. The extent of the probability that forced articulation will inhibit performance is indicated in Table IV. This comparison is of the individual S 's averages in Tests 5 to 27 inclusive, representing normal performance, and in Tests 6 to 26 inclusive, representing articulative performance. The figures are percentages of individual performance in Test 3.

TABLE IV
AVERAGE OF THE Ss NORMAL AND ARTICULATIVE PERFORMANCE SHOWING DIFFERENCES

	Normal	Articulation	Diff.	$\sigma_{diff.}$	CR
Av. of Av.	103.50	99.70	3.80	2.06	1.84
Av. of $\sigma_{dis.}$	5.60	5.05	0.55	0.49	1.12

Normal performance is likely to be greater as a whole to articulative performance and the degree that this can be expected is represented by a CR of 1.84. As would be expected, where average performance is greater individual variability over a period of performance is likely to be greater.

Inhibition on individuals. Individual CR s (computed for the 24 Ss) of the difference between average performance in articulative tests and an average of the standards of normal performance in those tests show that 20 Ss favor a normal performance and 4 Ss favor articulative performance. None of these 4 Ss have a CR above 1.00, whereas of the 20 Ss yielding results in favor of normal work, 17 have a CR above 1.00, 13 above 2.00, 12 above 2.50, 3 above 3.00. This illustrates the extent of

the tendency for forced articulation to inhibit performance in the individual. One-eighth of the Ss were always and half of them were very likely to be inhibited by forced articulation in efficiency of performance.

Inhibition at peak performance. Normal performance reached its highest average rate at Test 25 at which it was 108.4% of Test 3. Articulative performance reached its highest average rate at Test 26 at which it was 105.2% of Test 3. There is a difference between these two rates of 3.2, with a $\sigma_{diff.}$ of 5.2, and a CR of 0.61 in favor of normal performance. Thus forced articulation is likely to inhibit the peak of group performance.

The average of the highest individual normal performances at any test was 112.4% of Test 3. For articulative performance it was 107.9%. There is a difference between these two rates of 4.5, with a $\sigma_{diff.}$ of 2.38, and a CR of 1.89 in favor of normal performance. It is interesting that only one of the 24 Ss made his highest score in articulative work. Thus there is an even greater likelihood that individual peak performance will be inhibited by forced articulation than the peak of group performance.

Accumulated evidence conclusively indicates that forced articulation is likely to inhibit efficiency in automatic performance. The evidence of Experiment IV shows this to be true for an extended practice period as a whole and for individual tests in the practice period; also for peak performance of group or individuals and for separate individuals where a considerable proportion are always inhibited by the forced articulation.

Practiced articulation. The actual gain in efficiency throughout the series of tests was greater in articulative performance than in normal performance. From Test 4 (articulation) to Test 26 (articulation) there was gain of 11.8% whereas the gain from Test 3 (normal) to Test 25 (normal) was only 8.4%. This suggests that performance with forced articulation required practice to reach its highest efficiency. That its highest efficiency in the practice period was not equal to the highest efficiency of normal performance might be explained because the practice period was not sufficiently extended. The results of Experiment III support this conclusion.

Since efficiency in forced articulation approached efficiency in normal performance, the thought naturally arises that the former would eventually reach the latter if the series were prolonged—thus suggesting that forced articulation is an exaggerated condition of the normal situation. The comparison of normal and articulative performance by the tests in Table III shows, however, little if any decrease with extended practice of the CRs favoring normal performance, which contradicts that interpretation and suggests on the contrary that forced articulation is an artificial condition grafted on to the normal.

GENERAL CONCLUSIONS

The following general conclusion concerning all articulative activity seems warranted.

(1) No method has as yet been devised for the study of mental activity where articulation is completely inhibited and no conclusions, therefore, can be drawn as to what actually does take place without articulation. Attempts that have been made to devise methods of learning which inhibit articulation transcend the known facts. All the pedagogical methods in vogue at present to remove articulation in learning should be discarded.

Articulation may or may not exist in all mental work as far as any experiments to date have indicated. Where observable it does not have the same degree of conscious focus with different individuals and tasks. Defined as a function the worker must inhibit, it is evident that articulation is necessary to performance. Defined as a function the worker must exaggerate in performance, it is indicated that efficiency is likely to be influenced detrimentally. The following conclusions are supported in the experiments reported above as concerns the automatic task used:

(2) Forced articulation has little influence upon efficiency, but a slight inhibition is likely.

(3) Intended inhibition of articulation in any manner is detrimental to efficiency.

(4) Normal automatic work is performed by adults (undergraduate and graduate students in universities) with little or no awareness of articulation.

(5) To theorize from these experiments, one might believe that articulation to some degree at least is present in all mental activity. The concept of an articulative set is applicable. Articulation might be interpreted as mental set which has degrees of conscious focus in inverse proportion to the automaticity of the task and if the articulative set is disturbed by inhibition or exaggeration, performance will be inhibited according to the degree of the disturbance.

THE EFFECT OF AUDIOGENIC SEIZURES ON GENERAL ACTIVITY OF THE WHITE RAT

By FRANK W. FINGER and HAROLD SCHLOSBERG, Brown University

In a previous paper,¹ one of the writers reported that the punishment of rats' errors in a 'conflict' situation led to a decrease in the proportion of the day during which the animals were active. The effect, however, was small and lasted only for 24 hr. The experiments to be reported here extend the investigation to the effects of audiogenic seizures on general activity.

Since the original reports of Maier there has been considerable interest in the violent "running fits" that he described. It will be recalled that these seizures first occurred in a jumping-discrimination situation, in which a blast of air was used to force the response. The rats leaped from the apparatus, ran blindly around the floor for a time, and then typically exhibited violent tonic or tic-like movements of various muscles. They then frequently passed into a very relaxed condition in which they showed a condition analogous to "waxy plasticity."

Maier at first attributed this behavior to the conflict between alternate response patterns in the animal.² Morgan and Morgan promptly reported, however, that similar seizures could be elicited, by the presentation of a high-pitched sound, in the absence of any 'conflict.'³ Since that time the seizures have been obtained by numerous investigators, employing a variety of sound sources. Maier now admits the importance of the sound in producing the disturbances, but has adduced some evidence to show that 'conflict' may play at least a contributory part in certain instances.⁴ Morgan has argued against this view.⁵ At any rate it is certain that these seizures can be produced in many rats simply by subjecting the animals to a loud high-pitched sound, with no obvious 'conflict' in the situation. In this respect they differ from the traditional "neuroses" of Pavlov.⁶ Conflict, de-

* Accepted for publication June 7, 1941.

¹ F. W. Finger, Quantitative studies of 'conflict': II. The effect of 'conflict' upon the general activity of the white rat, *J. Comp. Psychol.*, (in press).

² N. R. F. Maier, *Studies of Abnormal Behavior in the Rat: I. The Neurotic Pattern and an Analysis of the Situation Which Produces It*, 1939, 1-81.

³ C. T. Morgan and J. D. Morgan, Auditory induction of an abnormal pattern of behavior in rats, *J. Comp. Psychol.*, 27, 1939, 505-508.

⁴ N. R. F. Maier and N. M. Glaser, Studies of abnormal behavior in the rat: II. A comparison of some convulsion-producing situations, *Comp. Psychol. Monog.*, 16, 1940, (no. 80), 1-30.

⁵ C. T. Morgan, Review of Maier's studies of abnormal behavior in the rat, *J. Gen. Psychol.*, 23, 1940, 227-233; C. T. Morgan and H. Waldman, "Conflict" and audiogenic seizures, *J. Comp. Psychol.*, 31, 1941, 1-12.

⁶ I. P. Pavlov, *Conditioned Reflexes*, trans. by G. V. Anrep, 1927, 1-430.

layed response, or difficult discrimination seem to the writers to be characteristic of all the usual situations in which experimental neuroses arise.⁷

The audiogenic seizures differ from experimental neuroses in a second respect. The typical experimental neurosis builds up slowly and lasts for a period of months or years, markedly disturbing the behavior pattern outside the experimental situation.⁸ The violent audiogenic seizure seems on the other hand to be an essentially episodic disturbance. A normally-appearing and completely untrained rat can, by means of one minute of intense auditory stimulation, be transformed into a violently 'abnormal' animal. After an interval of an hour, however, a casual observer is unable to distinguish the temporarily aberrant rat from his unstimulated litter mates. In this respect the audiogenic attacks are much more similar (at the human level) to epileptic seizures than to neuroses.

If one is to attempt to differentiate between experimental neurosis and audiogenic seizure on the basis of temporal course or generalized disturbance, one must have more evidence at his disposal than can be furnished by casual qualitative observation. There is a great deal of material at the descriptive level for both forms of disturbance. In the rat, for example, Cook observed that conflict-induced disorders left the rat more quiet and sleepy than normal.⁹ Similar observations have been made by Maier for the running attacks.¹⁰ There is also some quantitative evidence of the after-effects of conflict behavior. Liddell and his associates, for example, found in the sheep that experimental neurosis was accompanied by a more irregular cardiac cycle, increased general activity, and a disturbed diurnal rhythm.¹¹ Finger showed, as noted above, that punished errors in a conflict situation caused a temporary decrease in the general activity level of the white rat, but there is no comparable measurement of the after-effects of audiogenic seizures. If such effects exist, they should be measurable in terms of general activity. Hunt and Schlosberg have pointed out the advantage of this measure in a previous paper.¹²

⁷ Cf. S. W. Cook, Some theoretical considerations relating to "experimental neurosis," *Psychol. Bull.*, 36, 1939, 516; A survey of methods used to produce "experimental neurosis," *Amer. J. Psychiat.*, 95, 1939, 1259-1276.

⁸ H. S. Liddell, The experimental neurosis and the problem of mental disorder, *Amer. J. Psychiat.*, 94, 1938, 1035-1041.

⁹ Cook, The production of 'experimental neurosis' in the white rat, *Psychosom. Med.*, 1, 1939, 293-308.

¹⁰ Maier, *op. cit.*

¹¹ Liddell, *op. cit.*; O. D. Anderson, R. Parmenter, and H. S. Liddell, Some cardiovascular manifestations of the experimental neurosis in sheep, *Psychosom. Med.*, 1, 1939, 93-100.

¹² J. McV. Hunt and H. Schlosberg, General activity in the male white rat, *J. Comp. Psychol.*, 28, 1939, 23-38; The influence of illumination upon general activity in normal, blinded, and castrated male white rats, *ibid.*, 28, 1939, 285-298.

EXPERIMENTAL PROCEDURE

Apparatus. The method of recording general activity has already been reported in detail.¹³ Here it is sufficient to note that each rat lived continuously in an individual spring-suspended cage. Each cage was equipped with an inertia contactor which actuated a magnetic marker on a kymograph. The kymograph records were read to yield the number of 5-min. periods per hour during which movement occurred. Ten such cages were suspended in each of two quiet rooms. By means of a clock-controlled switch the rooms were kept lighted between 6 A.M. and 6 P.M., and were dark from 6 P.M. to 6 A.M. Water and 'dog chow' pellets were always available in the cages.

The sound source was a Galton whistle actuated by compressed air at about 20 lb. pressure. The whistle was set at 5.0, and produced a tone of 13,400 ~. The intensity of the sound was not determined, but it was sufficiently loud to be unpleasant to the human observer.

Method: (a) Group 1. Twenty male white rats were weighed and placed in individual activity cages. They were double first cousins, 110 days of age, and in good health. They had never been subjected to the loud sound, and represented a typical sample of our laboratory colony of remote Wistar stock. After they had been placed in the cages they were allowed 10 days to become accustomed to the new environments. Recording of activity was then started, and continued throughout the experiment. Animals were weighed and contactor calibrations checked at least once every two weeks.

Stimulation with the whistle was first introduced after the animals had been living in the cages for 40 days. The stimulation session occurred between 5 and 6 P.M., just before the beginning of the active 'dark' period. The whistle was sounded continuously for 2 min. in the center of each of the two experimental rooms, which placed it from 3 to 5 ft. away from the animals in their living cages. Several observers were present, and complete protocols were taken on an Ediphone. The kymograph was run at a faster speed during the session, to permit detailed analysis of the activity during that period. No seizure occurred, but we observed the usual restlessness and the peculiar "face-washing" behavior which is so frequently caused by loud high-pitched sounds. Three days later the same general procedure was repeated, with the exception that the whistle was sounded for 5 min. in each room. Again no seizure occurred. It was obvious that the stimulation was not intense enough to produce the desired effect. Since our interest was only in the effects of seizures on activity, it seemed desirable to abandon uniform stimulating conditions in favor of a program designed to obtain as many seizures as possible. Therefore, in each of sessions 3-12 the whistle was sounded within 3 in. of each cage for several 1- or 2-min. periods. Records were kept of the actual stimulating conditions, but they have not been included in our treatment of the results.

Sessions 3-7 were given at 2-day to 4-day intervals, and yielded 15 seizures from 7 of the 20 rats.¹⁴ Thirteen rats gave no seizure throughout the experiment.

¹³ Finger, *op. cit.*; cf. also Hunt and Schlosberg, *opp. cit.*

¹⁴ A seizure was considered to have occurred if the characteristic burst of violent and relatively undirected running was observed. Most seizures also showed the spastic and plastic phases. The Ediphone protocols were invaluable in evaluating a few marginal seizures.

Sessions 8, 9, and 10 followed session 7 at daily intervals, but produced only one seizure. We attributed this lack of responsiveness to adaptation¹⁵ so we waited 6 days before session 11. We obtained 3 seizures during this session, and one the following day during a special session. Stimulation sessions were then discontinued, and 'normal' activity records taken for a 10-day period before the experiment was terminated.

(b) *Group 2.* The 10 animals of this group had been selected on the basis of a preliminary test with the whistle. Each rat had developed a full seizure. It was hoped that this method would yield a higher incidence of seizures during the experiment. All 10 animals were males, 170 days old at the beginning of the experiment. Records were taken for two weeks before the first stimulation session. As in the later sessions of Group 1, every effort was made to obtain as many seizures as possible. Six stimulation sessions were distributed at equal intervals over 30 days. Four of the 10 rats had seizures at the first session, but the number fell off until the 6th session, which was completely unsuccessful. (Later evidence showed that this decrease was largely due to a piece of packing which had become lodged in the orifice of the whistle, markedly decreasing the intensity of the sound.) Four rats in this group had had a total of 11 seizures. The stimulation sessions were discontinued, and 2 weeks of 'normal' records obtained before the rats were removed from their cages and given a final weighing.

RESULTS

(1) *Short time effects.* As noted above, the kymograph records were read to yield the number of 5-min. active periods per hour in which each rat made some movement. These data were summarized in Fig. 1 and in Tables I and II.

In Fig. 1 we have plotted in solid dots the median number of active periods for each of the 24 hr. immediately following the seizures. The first 19 seizures of Group 1 furnished the data. Similar medians have been plotted in hollow circles for the (normal) days immediately preceding the seizures. It will be remembered that the stimulation sessions terminated at 6 P.M., just before the beginning of the dark 12 hr. Normally the onset of darkness results in almost continuous activity, as shown by the hollow circles. In the hours immediately after the seizure the rat is distinctly less active than usual. The difference between the medians of the pre- and post-seizure days continues for perhaps 9 hr. After that the pre- and post-seizure days are no longer clearly different, although there is a suggestion of a difference when the lights first come on again, 12 hr. after the seizure. Fig. 1 does not indicate the reliabilities of the differences, but suggests that there is a small, short-lived decrease in activity after the audiogenic seizures.

¹⁵ R. A. Patton and H. W. Karn, Abnormal behavior in rats subjected to repeated auditory stimulation, *J. Comp. Psychol.*, 31, 1941, 43-46.

To obtain some statistical index of the reliability of the differences, the data of Groups 1 and 2 were combined and subjected to the t-test.

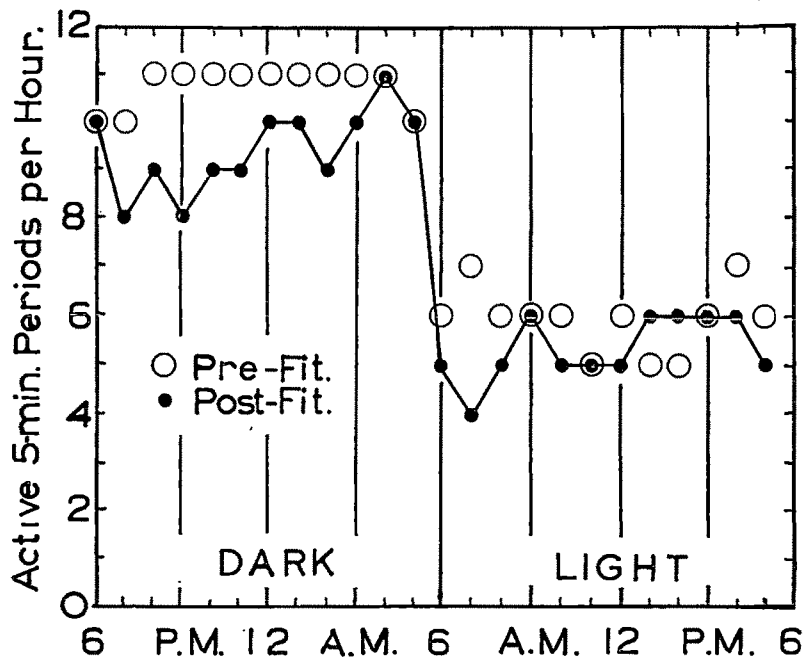


FIG. 1. ACTIVE 5-MIN. PERIODS PER HOUR

The data are medians from 19 seizures. Solid dots represent post-seizure days, while hollow circles indicate pre-seizure (normal) days.

Computations of Table I were based on 12-hr. light or dark periods, rather than on hourly figures. To simplify the treatment the actual number of active 5-min. periods was divided by the total possible number (usually

TABLE I

EFFECT OF 30 AUDIOGENIC SEIZURES ON GENERAL ACTIVITY DURING THE FOUR SUCCESSIVE 12-HR. PERIODS AFTER THE SEIZURES
(Decreases are calculated from the corresponding 12-hr. periods of the days before the seizures.)

	First day		Second day	
	night	day	night	day
Mean Decrease	9.5%	4.4%	0.8%	2.0%
t	3.9	1.7	0.3	0.6
P	<1%	9%	80%	58%

12 x 12, or 144) for the period concerned, so that the values in Table I are percentages. By combining Groups 1 and 2 we had available activity records for the periods before and after 31 seizures. The percentage of

activity for the 12-hr. period following each seizure was compared with the percentage value for the corresponding (dark) period of the preceding day, and the array of 31 differences was subjected to the nul hypothesis. In the first column of Table I it may be seen that there was an average decrease of 9.5% during the first 12-hr. period following the seizure. The *t*-value is 3.9, which indicates that the difference is significant considerably beyond the 1% level. Thus there is a small but reliable decrease in activity during the 12-hr. period immediately following the seizure.

Similar comparison of the activity percentages for the second 12-hr. periods after the seizures with the corresponding (light) periods of the

TABLE II

CUMULATIVE EFFECTS OF 31 AUDIOGENIC SEIZURES ON GENERAL ACTIVITY DURING PRE- AND POST-EXPERIMENTAL 10-DAY PERIODS AND ABSOLUTE AND RELATIVE INCREASES IN BODY-WEIGHT IN GRM. DURING THE EXPERIMENT

	Activity changes		Weight increases			
	control	exper.	absolute		relative	
			control	exper.	control	exper.
No. of rats	20	9*	20	10	20	10
Mean change	-1.3%	-1.4%	61.6 grm.	38.1 grm.	26.5%	16.6%
Difference		0.1%	23.5 grm.			9.9%
<i>t</i>		0.03	2.35			1.8
<i>P</i>		>90%	3%			8%

* The post-experimental activity records of one animal were spoiled.

preceding days shows a smaller decrease. The difference has fallen to 4.4%, with a *t*-value of 1.7. This difference is significant at the 9% level. The third and fourth 12-hr. periods after the seizures show a negligible and unreliable differences when compared with the pre-seizure percentages. Thus it may be seen that the decrease in general activity following audiogenic seizures barely lasts for 24 hr.

It may be convenient to have a single average figure for the activity decreases during the first 24 hr. after seizures. In raw percentage this loss is 7%, but this value is based on total possible number of 5-min. periods. Actually the normal baseline of activity in this experiment was about 66%. It may seem, therefore, that on the days immediately following the seizures the general activity drops by 10.5% of its normal value.¹⁶

To be certain that the decreases were the result of the seizures and not merely of the auditory stimulation itself, a similar analysis was made of activity records following unsuccessful stimulation sessions. It was possi-

¹⁶ A preliminary analysis of the first 19 seizures of Group I was reported at the 1941 meetings of the Eastern Psychological Association. The results were similar to those of the combined groups.

ble to match the first 19 seizures of Group 1 with an equal number of records from the same animals when the stimulation did not cause seizures.¹⁷ The auditory stimulation alone, when it did not induce a seizure, resulted in a slight and insignificant *increase* in activity during the following two 12-hr. periods. For the first 12-hr. period the increase was 1.5%, with a *t* of 0.42, and a *P*-value of 65%. For the second 12-hr. period the increase was 2.3%, with a *t* of 0.90 and a *P*-value of 40%. Thus the demonstrated decrease in activity was not the direct result of the auditory stimulation, but of the seizures induced by the stimulation.

(2) *Long time effects.* Even though the activity decreases seemed to be of short duration it was still possible that there was a cumulative effect on activity. To settle the question, activity percentages for the 10 days before and after the series of stimulation sessions were compared. Means for the changes from pre- to post-experimental periods were computed for 9 of the 10 animals (the 10th record was spoiled) that had exhibited seizures, and for the 20 animals that had not. An examination of Table II shows that the difference between experimental and control groups is only 0.1%, with a *t* of 0.03, and a *P*-value considerably above 90%. Thus there is no evidence in our data of a cumulative effect of seizures on activity.

In addition to activity records we also had available the weights of the rats before and after the series of stimulation sessions. When these are similarly treated, there is some evidence of a difference between seizure and no-seizure animals. All animals gained weight between the beginning and the end of the experiment; but Table II shows that those animals who had had seizures gained an average of only 38.1 gm., while the control rats gained an average of 61.6 gm. The difference in favor of the control animals is 23.5 gm., with a *t* of 2.35, and a *P*-value of 3%. If gains are computed in terms of percentage, of body weight, the difference becomes 9.9%, with a *t* of 1.8, and the difference is significant at the 8% level. There is thus apparently a tendency for repeated seizures to retard increases in body weight.

DISCUSSION

The decrease in general activity following audiogenic seizure is surprising slight and short-lived. The effect amounts to a drop of about 10.5% from the normal level during the first 24 hr., and is essentially over by the second day. Further, there is no evidence in our data of a cumulative

¹⁷ It seemed inadvisable to include Group 2 in this treatment, since the whistle was not uniformly satisfactory during this portion of the experiment.

effect of the series of seizures on the level of activity. This lack of long-range behavior change need not contradict Maier's observation that his animals became "retiring at all times."¹⁸ It is possible that, in the situation he was investigating, the effect of the actual conflict summated with the effect of the audiogenic attacks, producing a greater generalized disturbance than would result from the separate action of either factor. It is more likely, however, that the discrepancy is due to a difference in level of description and in the aspects of behavior concerned. There is little reason to expect that his type of observation would agree with our activity measure. While his qualitative description is undoubtedly of value, it would seem that our more precise and quantified function is of equal importance, since it allows us to compare with some exactness the effects of a variety of situations.

The temporary decreases found in the present study are of the same order of magnitude and duration as those reported by Finger to follow 'conflict.'¹⁹ He found that an average of 12 punished errors in a jumping-discrimination situation led to a decrease of about 9% in the general activity level during the succeeding 24 hr., and that most of the decrease occurred during the first 12 hr.²⁰ We may contrast the present results with those reported by Liddell in 'neurotic' sheep.²¹ He found that the sheep's activity *increased* (although the measure he used was somewhat different from ours), and that this effect persisted for much longer than 24 hr. If we may validly compare the behavior of the sheep with that of the rat, it may be stated that our failure to find large and cumulative activity changes constitutes another contribution to the growing mass of findings which differentiate audiogenic seizures from 'experimental neurosis.' In any instance, it is indicated that the adoption of such a quantitative technique in future studies of abnormal behavior may be valuable in detecting similarities and differences in the nature of the various behavior patterns.

We are inclined to explain the differences we did obtain largely on the basis of fatigue. The work of Skinner,²² among others, has shown that a rat will do a relatively fixed amount of running in a day, and that this amount can be widely distributed or compressed within a short time,

¹⁸ Maier and Glaser, Experimentally produced neurotic behavior in the rat, *Film*, 1938, 16 mm., 600 ft.

¹⁹ Finger, *op. cit.*

²⁰ Finger's figures have been converted from raw percentages to percentage of normal level for his animals.

²¹ Liddell, *op. cit.*

²² B. F. Skinner, The measurement of spontaneous activity, *J. Gen Psychol.*, 9, 1933, 3-23.

depending upon the length of time the rat has access to the running wheel. Anyone who has seen the violent running fits induced by sound would admit that the rat had had its full quota of activity for the day! The surprising thing is that the rat is active at all during the following 24 hr. It must be remembered, however, that our method of recording measures general restlessness rather than total amount of movement.²³ A rat may well move around the cage when he would not show sustained activity in a running wheel. The fact that our method of measuring general activity is thus relatively insensitive to fatigue effects seems to us to be one of its virtues, for it makes the results less dependent on the specific experimental situation, and therefore better measures of the possible disturbing effects of conflict. Our index, however, is probably affected sufficiently by fatigue to permit us to invoke that explanation for the activity decrease following audiogenic seizures.

In view of our failure to find cumulative effects of seizures on activity, we did not expect to find cumulative effects on body weight. The animals that had had seizures gained only about 62% as much weight as did the control rats. The results were particularly surprising, for the 10 experimental animals had had only 31 seizures, or an average of 3 per rat, distributed over 20-30 days. Page has reported roughly similar results after a severe series of 10-15 shock seizures (9-wk. old rats), but there was also pronounced general deterioration.²⁴ It is doubtful that the shock seizures are directly comparable to the audiogenic seizures. It must be remembered that our experimental animals did not actually lose weight during the experiment; they simply failed to gain as much as did the controls. This is perhaps a bit more understandable in light of the living conditions of the rats. Each rat was housed individually in a small round cage with a mesh floor. There was, therefore, little stimulation, social or otherwise, for activity, and little opportunity for running. On an unlimited food supply the control rats gained rapidly. It is possible that the additional activity involved in the seizures was much more effective in checking the normal weight gains that it would have been had the rats lived more active lives. A slight additional factor may have been the effects incidental

²³ In our method of recording activity, a rat will be scored as continuously active if he makes a single movement, e.g. scratching or eating, once every 5 min. The fact that this technique stresses the distribution of movements, or general restlessness, contrasted with running as measured by the activity wheel, has been discussed by Hunt and Schlosberg (*loc. cit.*). They found that castration had relatively little effect on this measure of general activity, in contrast to the findings of other investigators using the activity wheel.

²⁴ J. D. Page, Studies in electrically induced convulsions in animals, *J. Comp. Psychol.*, 31, 1941, 181-194.

to injury. Several of the rats cut themselves during the seizures, and at least one lost a substantial amount of blood. The irritation and physiological disturbance of such wounds might be expected to interfere with normal weight increases. There is still the possibility, however, that the audiogenic seizures had a more direct effect on weight than we have suggested above. Further experimentation will be necessary to verify and investigate this effect.

SUMMARY

(1) Twenty male albino rats were subjected to a series of 12 sessions of auditory stimulation by a Galton whistle. Another group of 10 animals was given 6 sessions. The rats lived continuously (including the stimulation periods) in single cages which recorded their general activity.

(2) A total of 31 audiogenic seizures was obtained from 10 of the rats.

(3) Audiogenic seizures caused a slight (10%) and transient (24-hr.) decrease in general activity. The effect was not cumulative.

(4) The animals that had had seizures gained significantly less weight during the experiment than did the control animals.

(5) The small and generally temporary changes induced by the audiogenic seizures are attributed primarily to the effect of fatigue.

(6) The results serve further to differentiate audiogenic seizures from traditional experimental neurosis.

OCULAR PATTERNS IN VISUAL LEARNING

By HERMAN F. BRANDT, Drake University

The purpose of this study is to investigate by means of ocular photography certain phases of the learning process. In an earlier study,¹ the author found three properties of eye movement when a symmetrical field is under observation. (1) The mean of the initial fixation is located at a point above and to the left of the center of the observed field, while the second and third fixations are located above and to the left of the first. (2) There are more fixations on the upper half of the field than on the lower, and more on the left half than on the right.² (3) Horizontal

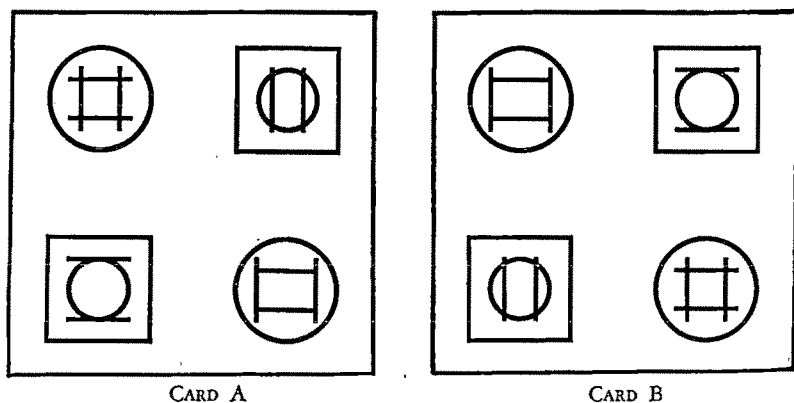


FIG. 1. EXPOSURE-CARDS

movements exceed both in frequency and in total excursion-distance. The purpose of the present experiment is to relate the eye's behavior during observation to subsequent recall. It will be seen that the analysis of ocular patterns when a subject learns provides a valuable technique for investigating problems of learning, attention, and other topics. This study answers specifically the following questions: (1) Is consistently more time devoted to material in certain positions than in others? (2) Is achievement in terms of recall significantly greater in one position than in another? (3) Is the character of the material responsible for a differ-

* Accepted for publication March 14, 1941.

¹ H. F. Brandt, Ocular patterns and their psychological implications, this JOURNAL, 53, 1940, 260-268.

² These results confirm, by a different method, Dallenbach's finding that positions to the left and above have an advantage for attention. K. M. Dallenbach, Position vs. intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-286.

ence in achievement? (4) Are ocular patterns of subjects of high achievement characteristically different from those of subjects of low achievement?

METHOD AND PROCEDURE

Subjects and observational material. A total of 90 Ss, 46 men and 44 women, were selected at random to serve in this investigation. Forty-five Ss, 21 men and 24 women, observed exposure-card A, shown in Fig. 1, while an additional 45, 25 men and 20 women, observed the same card

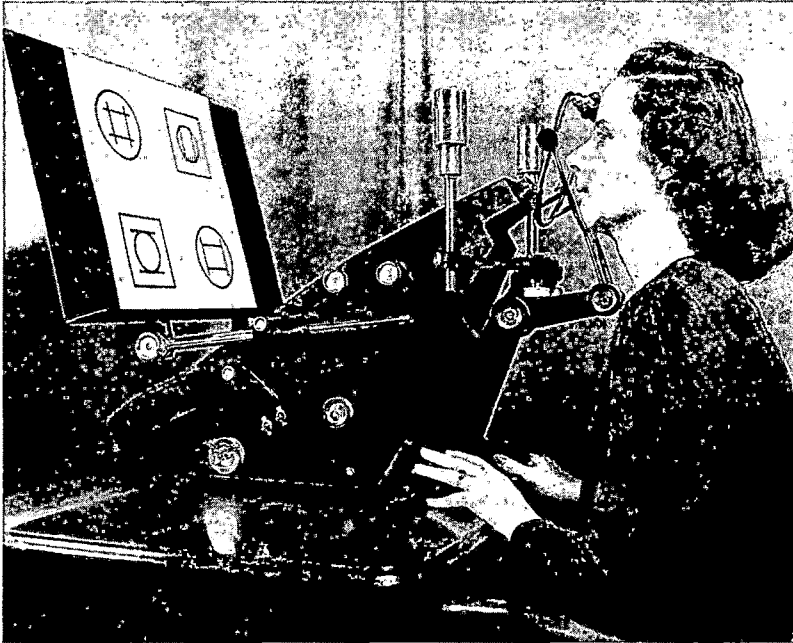
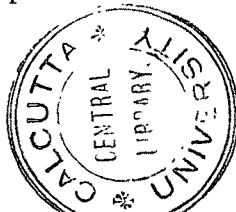


FIG. 2. PORTABLE EYE-MOVEMENT CAMERA AND EXPOSURE-CARD

in a reversed position exposure, card B. Every S was instructed to observe the card with the intention to reproduce the four designs on the card later. The card was exposed for a period of 10 sec., but no S was informed of this time limit. The size of the card was 14 in. sq., of each design 4 in. sq.

Scoring. The maximum score for each S was 12 units. If an S reproduced correctly the circle or square of a design, he received a credit of one unit; if, in addition, he reproduced correctly the figure within the square or circle, he was credited with two units; and if, in addition, the two parts of a design were reproduced in the correct location, the score was three units.



Eye records. A bidimensional mono-film eye movement camera (Fig. 2) was employed to record the ocular patterns of every S while he was studying the card.³

RESULTS

Eye records. We present the eye records first. These confirm our earlier findings. Fig. 3, which shows the distribution of fixation during a period of 10 sec., indicates that more time is spent on the left and upper half

CARD A		CARD B	
34.94 (1)	32.99 (3)	36.67 (4)	27.42 (2)
16.28 (2)	15.79 (4)	19.57 (3)	16.24 (1)

FIG. 3. PERCENTAGE OF FIXATION-TIME DEVOTED TO THE RESPECTIVE DESIGNS IN EXPOSURE-CARDS A AND B

(The numbers in parentheses are nominal numbers of the designs.)

of the field than in the lower and right half. It is apparent from Table I, which presents a comparison of extreme positions, that significantly more time is spent on Designs I and 4 when appearing in the upper left-hand

TABLE I
AVERAGE FIXATION-TIME (IN SEC.) SPENT IN UPPER LEFT AND LOWER
RIGHT-HAND POSITION FOR DESIGNS 1 AND 4

Design	Card	Position	Mean	SE	M _{diff.}	SE _{diff.}	CR
1	A	Upper left	3.49	.98	1.86	.18	10.33
1	B	Lower right	1.62	.71			
4	B	Upper left	3.67	.17	2.09	.21	9.95
4	A	Lower right	1.58	.12			

area than when appearing in the lower right-hand position of the field.

Of all the ocular excursions, 395 were horizontal, 204 vertical, and 112 diagonal. The frequency of horizontal and vertical movement is in the ratio of about 2:1, while vertical and diagonal stand in a similar ratio. Average horizontal and vertical frequencies are presented in Table II.

³ The Mono-film Eye-Movement Camera was invented by the author of this study.

The correlation co-efficient between excursion-frequency and total excursion-distance to fixation time is 0.95 and 0.92 respectively. This close relationship between three measures of ocular performance implies that,

TABLE II

Card	AVERAGE FREQUENCY OF HORIZONTAL AND VERTICAL EXCURSIONS					
	Excursion	Mean	SE	M _{diff.}	SE _{diff.}	CR
A	Horizontal	4.73	.30	2.69	.39	6.90
	Vertical	2.04	.26			
B	Horizontal	4.04	.26	1.55	.36	4.31
	Vertical	2.49	.20			

if a specified time is spent in an area, the frequency and distance of excursions will vary proportionally.

Fig. 4, a typical eye pattern, illustrates the main results, of preference for the upper left-hand position and for horizontal movement.

Reproduction scores. Fig. 5 indicates that achievement, measured in

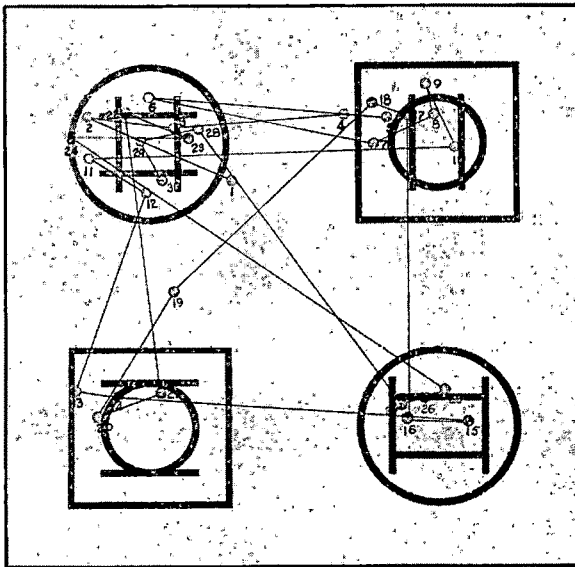


FIG. 4. OCULAR PATTERN OF S 46; ACHIEVEMENT SCORE: 7 UNITS

terms of reproduction-score, is greater in the upper left-hand area than in the lower right-hand position. Table III, which presents the average achievement in the extreme positions, supports the assumption that a longer time devoted to study in certain areas (other things being equal)

will yield a greater achievement. Both time and achievement are significantly greater for the designs appearing in the upper left-hand position. Design 4, with a critical ratio of 5.30, seems to profit more from the

TABLE III

RELATIVE ACHIEVEMENT IN THE UPPER LEFT-HAND AND LOWER RIGHT-HAND POSITION FOR DESIGNS 1 AND 4							
Design	Card	Position	Mean	SE	M _{diff.}	SE _{diff.}	CR
1	A	Upper left	2.38	.15	.87	.23	3.79
1	B	Lower right	1.51	.18			
4	B	Upper left	1.93	.13	1.06	.20	5.30
4	A	Lower right	.87	.15			

changed position than Design 1 with a critical ratio of 3.78. The attainment for the difficult Design 4, when appearing in the lower right, was 12.38% as compared to 30.11% when appearing in the upper left (see

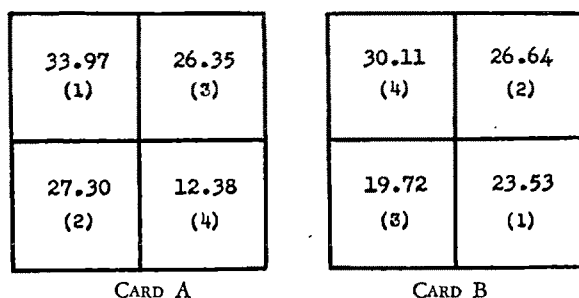


FIG. 5. PERCENTAGE OF TOTAL ACHIEVEMENT IN RESPECTIVE AREAS OF EXPOSURE-CARDS A AND B

Fig. 5). Achievement is higher in preferred positions, but the fixation-time per unit of achievement is lower for low-achievement areas.

Is the character of the designs responsible for a difference in achievement? Table IV indicates that Designs 1 and 4, when appearing in the upper left-hand position, yield an average achievement of 2.38 and 1.93

TABLE IV

RELATIVE TIME SPENT ON DESIGNS 1 AND 4 IN THE SAME POSITION							
Design	Card	Position	Mean	SE	M _{diff.}	SE _{diff.}	CR
1	A	Upper left	2.38	.15	.45	.20	2.25
4	B	Upper left	1.93	.14			
4	A	Lower right	.87	.15	.64	.23	2.78
1	B	Lower right	1.51	.18			

units respectively. When the two designs appear in the lower right-hand position, achievement drops to 1.51 and 0.87 units. In both positions Design 1 has an advantage; and, although the critical ratio is below three, it indicates that Design 1 is more readily reproduced than Design 4. Since

both designs appear in the same positions, the difference in achievement is entirely due to the character of the designs. The difference in achievement is more pronounced when the designs appear in the lower right or undesirable position of the field. This would imply that materials that are more difficult to acquire suffer more by a change from a preferred to a non-preferred position.

Are ocular patterns of subjects of high achievement characteristically different from those of low achievement? For both groups of subjects the difference is indicated in Table V. The table shows that Ss of low achievement devote nearly twice as much time, 2.05 sec., to acquire one

TABLE V
AVERAGE FIXATION-TIME PER UNIT OF ACHIEVEMENT FOR S OF
HIGH AND LOW ACHIEVEMENT

Card	Achievement	Designs				Total
		1	2	3	4	
A	High	1.19	.81	1.46	1.19	1.17
	Low	1.93	.93	2.46	4.06	1.93
B	High	.80	1.21	.92	1.74	1.17
	Low	1.64	2.07	3.91	2.08	2.18
A & B	High	1.02	.98	1.22	1.53	1.17
	Low	1.82	1.52	2.91	2.44	2.05

unit of subject-matter as do their superior competitors, who devote only 1.17 sec. to achieve a unit of the same type of subject-matter. The greatest margin of difference between those of high and low achievement is in Design 4 when appearing in the lower right-hand position; the high-achievement group spent an average of 1.74 sec. to acquire one unit, while the low-achievement group spent an average of 4.06 sec. for every unit correctly reproduced.

With difficult materials, position is a greater determiner of level of achievement for Ss of low than for those of high achievement. The low-achievement group spent an average of 4.06 sec. when Design 4 appeared in the lower right-hand position and only 2.08 sec. when the same design appeared in the upper left-hand area, while the high-achievement group had a ratio of 1.19:1.74. This difference is much smaller (1.93 to 1.82 sec.) when the more familiar Design 1 appears in the two positions. The ratio then is 1.19:80 and 1.93:1.64 for the high and low achievement group respectively.

SUMMARY AND CONCLUSIONS

This study is an evaluation by means of photography of certain phases of the ocular pattern when an S learns. The results of this study make it

apparent that both time spent and information gained are greater for materials presented in one position than in another. The difference in both time and achievement is significant for the two positions analyzed.

Although more time devoted is generally followed by a greater achievement, the time spent per unit of achievement is greater for areas of low achievement than for areas of higher accomplishments, and easy materials in favorable positions suffer diminishing returns for time invested per unit of achievement as compared to materials in less desirable positions. This seems like a contradiction to earlier statements, but it is only another way of saying that high achievement areas although much higher in units of accomplishment, yield lower ratios of achievement to time spent.

If the criterion of efficiency is determined by the ratio of time devoted to the achievement accomplished, then it is apparent from the results of this study that a positive relationship exists between the two factors. Time spent is followed by achievement, but the time spent per unit of achievement varies with the position and character of the material and with the individual subject.

Just what rôle ocular movements play in producing and reproducing visual forms is not known at this time, but it is likely that, as Breese put it, "motor activity is a necessary component of the learning process."⁴ Ocular photography, because of its unique approach to the analysis of learning, can throw new light on the problem. The fixation, location, frequency, and duration, together with the direction and distance of each excursion, constitute, when properly reproduced, a record of the ocular performance for a given task. Conditions which influence and determine the character of ocular patterns in learning-situations are the intellectual capacity of the individual, the character of the observed field, the nature of the problem, the past experience of the learner, and the purpose of the learner at the time of observation. The ocular patterns show the relations which exist for the learner among the materials presented. Organization in this sense of the term is no longer an abstract idea. It has meaning in terms of ocular performance which reveals the method and procedure employed by the learner in acquiring certain types of information: how he attacks the problem, how he organizes his data, and how he distributes his efforts.

Ocular photography may also serve as an indicator of the level of efficiency of the learner—records of earlier and later stages may be compared—and as a criterion in evaluating levels of achievement in a specified task.

⁴ B. B. Breese, *Psychology*, 1917, 416.

Ocular photography can also indicate remedial procedure. We need to inquire, under scientifically controlled conditions, what form of ocular performance we desire in order that the learner may gain a maximum of information with a minimum of effort. New types of presentation may thus be developed to aid the learner. From our records of frequency and direction of excursions it would seem that subject-matter, to be efficiently perceived, should be presented in such a way that the horizontal eye movements would have acquisitional advantages. To learn relations which are essential but which are in the vertical plane would on the basis of these findings complicate the learning process.

Moreover, if the time devoted to a certain area represents a comparable achievement in terms of information, retention, appreciation, interest, or reflection, then subject-matter could be evaluated by ocular photography to the advantage of educators, artists, advertisers, vocational counselors, and textbook writers alike.

The analysis of ocular performance by means of photography is thus a promising field of research in many respects.

CONFIGURAL ASPECTS OF HUMAN LEARNING ON THE ELECTRICAL MAZE

By CECIL W. MANN and WALTER O. JEWELL, JR., University of Denver

The purpose of the present study was the investigation of some of the configural aspects of the learning of an electric maze by human subjects. In addition to the usual methods of measuring learning—comparison of mean time and errors—an attempt was made to describe the patterns or configurations developed during the process of learning. The learning problem consisted of an electrical maze designed to avoid the use of blindfold or screens. Opportunities were presented for the Ss to indicate the patterns built up during the process of learning.

METHOD

Apparatus. The electrical maze developed for this experiment is a compact and portable instrument permitting the use of visual cues, yet preventing the true pathway from being seen. It has proved adaptable to various learning experiments from simple maze-performance to more complex experiments involving the use of shock under controlled conditions.

Haught, in 1921, described a maze consisting of bolt-heads through which was connected an unseen pathway.¹ Barker, reported in 1931, a maze which he called a 'stepping-stone' maze.² This consisted of a pathway electrically connected through a series of bolt-heads. A simple wiring device enabled the pattern to be altered very rapidly. Tolman, Hall, and Bretnall, in 1932, used a punch-board maze consisting of 30 holes.³ In 1934, Muenzinger repeated a similar experiment substituting bolt-heads for the punch-holes.⁴ Gurnee, in 1937, described a portable bolt-head maze of similar design.⁵ A summary of many of the studies of maze learning is given by Woodworth.⁶

The electrical maze used in the present experiment consisted of a panel of

* Accepted for publication February 27, 1941.

¹ B. F. Haught, Interrelation of higher learning processes, *Psychol. Monog.*, 30, 1921, (no. 139), 1-71.

² R. G. Barker, The stepping stone maze: A directly visible space problem apparatus, *J. Gen. Psychol.*, 5, 1931, 280-285.

³ E. C. Tolman, C. S. Hall, and E. B. Bretnall, A disproof of the law of effect and a substitution of the laws of emphasis, motivation and disruption, *J. Exper. Psychol.*, 15, 1932, 601-614.

⁴ K. F. Muenzinger, Motivation in learning: The function of electric shock for right and wrong responses in human subjects, *J. Exper. Psychol.*, 17, 1934, 439-447.

⁵ Herbert Gurnee, A portable bolt-head maze, this JOURNAL, 51, 1938, 405-406; The effect of electric shock for right responses on maze learning in human subjects, *J. Exper. Psychol.*, 22, 1938, 354-364.

⁶ R. S. Woodworth, *Experimental Psychology*, 1938, 124-155.

96 bolts, 9/16 in. flat-head, set 1 in. apart in 12 rows of eight in each row. The panel was of three-ply, 15 x 11 in. fastened to a frame of pine 4 in. in depth. Under the left-hand bolt in the first row was placed a washer to indicate the starting point, and the final position, the right-hand bolt in the top row, was indicated in a similar manner. Above the top row, and in the middle was placed an electric glow lamp to indicate right responses. This was wired in series with 36 bolt-heads in the panel and these constituted the true pathway. The remaining

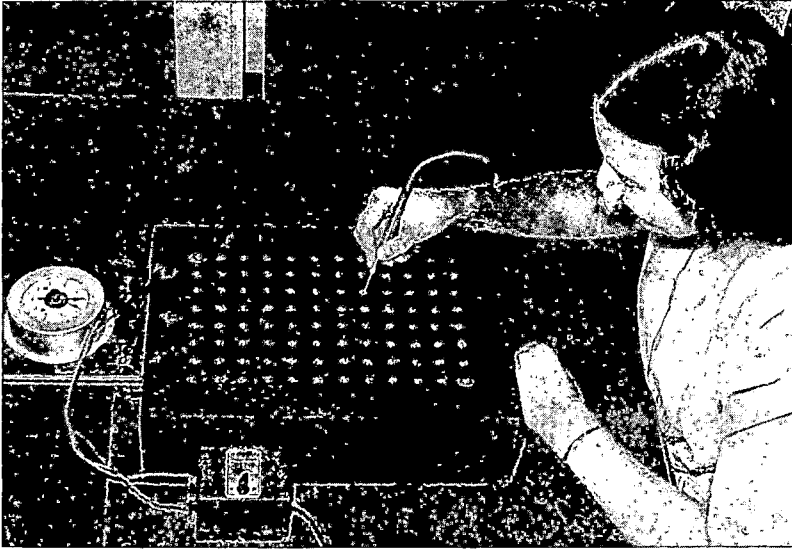


FIG. 1. ELECTRICAL BOLT-HEAD MAZE

bolt-heads were wired in series with an electric counter, which recorded errors as they were made. The three-ply panel was painted flat-black.

The reliability of the maze was determined by the method of split-halves using a group of 25 university students. The first trial was omitted and values of 0.74 for time and 0.82 for errors were found. Correction of these values by the Spearman-Brown formula yielded reliabilities of 0.84 for time and 0.90 for errors.

Procedure. In this experiment, two groups of Ss were run through the maze. Seventeen Ss, 6 men and 11 women students, were used as the control group. Every S was shown the starting point and the finish. He was then instructed to advance to the finish by moving the stylus not more than one position in any direction. If his choice were correct—as indicated by the light—he was to advance to another position. If, however, his choice were incorrect, he was instructed to return to his *last correct position* and from there make a new choice. He was advised to move as accurately and as rapidly as possible. Time of completing each trial and the number of errors were recorded. Three trials a day for five successive days were run, making a total of 15 trials.

In the experimental group there were 26 Ss, 8 men and 18 women students. The

instructions for the experimental group were the same as for the control group. In addition, however, the experimental group was required at the end of the trials each day to draw on a mimeographed facsimile of the maze a map of those parts of the maze which he considered he had learned. The instructions asked only that those parts known be included, and guessing was discouraged.

From every *S* in both groups, scores in terms of time and errors were obtained. In addition there were two measures obtained from the *Ss* in the experimental group. The maps were scored for correct position, *i.e.* the correct movement from one bolt-head to another; and for pattern, *i.e.* the shape of the pattern, regardless of position, with a pattern of three moves correct considered a pattern.

RESULTS

(1) *Learning curves.* From the measures of mean time and errors, conventional learning curves were drawn. It is not considered, however,

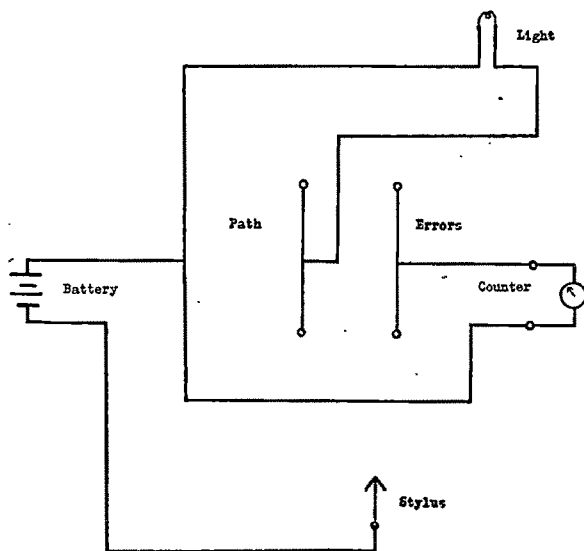
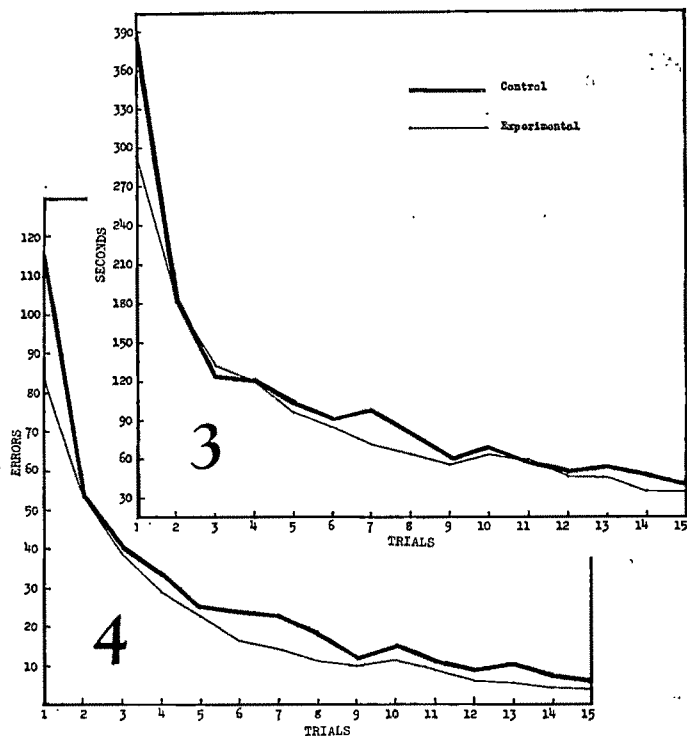


FIG. 2. DIAGRAM OF THE WIRING OF THE MAZE

that these indicate the whole process of learning; indeed, the curves may be regarded as very inadequate descriptions of learning. Critical ratios of 1.36 for time and 1.04 for errors were computed between the means of the control and experimental groups and one may conclude that there is no significant difference between the performance of the two groups.

To determine whether the drawings made at the end of each series of three trials by the experimental group had any effect on learning, two comparisons were made. (a) Comparisons were made between the control group on the fourteenth trial and the experimental group on the

eleventh trial. The eleventh trial was chosen because this included three maps, and thus there would be a possible total of 14 trials for each group, assuming that the map drawings have some effect on learning.



FIGS. 3 AND 4. GRAPHS OF MEAN TIME AND MEAN ERRORS PER TRIAL FOR THE CONTROL AND EXPERIMENTAL GROUPS

(Fig. 3 shows the graphs of mean time and Fig. 4 the graphs of mean errors.)

At this comparison, the CRs were 1.78 for time and 1.07 for errors, both indicating lack of significance.

(b) Comparisons were made between the control group on the four-

TABLE I

COMPARISON OF THE CONTROL GROUP ON TRIAL 14 WITH THE EXPERIMENTAL GROUP ON TRIAL 11

Group	N	Mean time				Mean errors			
		sec.	σ	σ_m	CR	av. no.	σ	σ_m	CR
Control	17	46.41	19.32	4.69	1.78	7.41	6.53	1.58	1.07
Experimental	26	57.62	22.06	4.33		9.46	5.51	1.08	

teenth trial and the experimental group on the fourteenth trial. The fourteenth trial of the experimental group would include 4 drawings. On this

comparison the *CRs* were 1.23 for time and 1.73 for errors, again indicating lack of significance.

It would appear from these comparisons that the interpolation of the map drawing procedure has little effect upon the learning. This may be

TABLE II
COMPARISON OF CONTROL GROUP ON 14TH TRIAL WITH THE EXPERIMENTAL
GROUP ON THE 14TH TRIAL

Group	N	Mean time				Mean errors			
		sec.	σ	σ_m	CR	av. no.	σ	σ_m	CR
Control	17	46.41	19.32	4.69	1.23	7.41	6.53	1.58	1.73
Experimental	26	39.73	14.87	2.92		4.42	3.62	0.71	

due either to the fact that the drawings gave little indication of what was right or what was wrong, or to the instructions in which the *Ss* were told to draw only what they knew to be correct. In any case, the drawings do not seem to have vitiated the learning process, and we are permitted to examine them for indications of the configural development of learning.

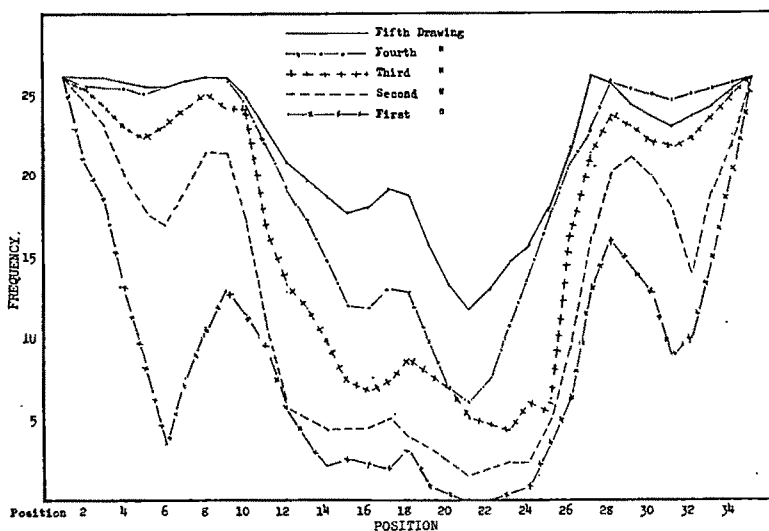


FIG. 5. GRAPH OF FREQUENCY OF CORRECT RESPONSES FOR EVERY POSITION

(2) *Positional learning.* The maps were first scored for correct position. This was done in such a way that correct movement from one point on the drawing to another, corresponding to similar contact points on the maze, constituted a correct score. It will be remembered that there were 36 bolt-heads in the total pattern, including the start and the finish. Position 1 is a move from the start to the first correct position. There are, therefore, a total of 35 correct positions.

From Table III and Fig. 5, there is some indication that primacy and recency play some part in determining the positions which are most readily learned. Reports of the Ss bear out this contention; all reported that the beginning and the end were much easier than the middle of the maze. It should be borne in mind, however, that at both the start and

TABLE III
SHOWING FREQUENCY OF CORRECT POSITION, FOR EACH DAY

Position	Day				
	first	second	third	fourth	fifth
1	25	26	26	25	26
2	20	25	26	25	26
3	17	23	24	26	26
4	19	21	23	25	26
5	3	16	22	25	25
6	3	16	22	25	25
7	5	19	25	26	26
8	14	22	25	26	26
9	12	23	25	26	26
10	13	19	22	26	26
11	9	8	15	20	22
12	6	5	13	19	20
13	2	4	12	18	20
14	3	6	10	15	19
15	2	3	7	11	17
16	3	4	5	10	17
17	2	6	8	14	20
18	1	5	9	9	16
19	1	3	9	9	16
20		2	5	5	11
21		1	6	6	12
22		2	4	6	12
23		3	4	9	15
24	1	2	5	16	16
25	1	2	9	17	16
26	9	11	17	20	22
27	13	15	23	25	26
28	17	22	25	26	26
29	18	23	23	26	25
30	8	18	21	24	22
31	10	19	22	25	24
32	9	17	22	25	23
33	11	16	23	25	24
34	25	24	26	26	26
35	25	25	26	26	26

the finish there were boundaries of the maze which may have helped learning at those areas. It is possible, too, that differentiation of the pattern at the boundaries is facilitated by the fact that at the boundaries there are fewer possible choices than in the middle part of the maze. It should be possible to correct this by the construction of a larger panel so arranged that while there are many more bolt-heads on the panel the pattern is

identical to that employed in this maze. By the use of such a device the boundaries would possibly offer no more help than the middle of the maze. This problem is now under way.

(3) *Configurations developed in learning.* In order to study the configurations formed, the maps were analyzed by patterns. Arbitrarily a pattern was assumed to be a group of not less than three correct joinings of bolt-heads, regardless of correct position. An analysis of the frequency

TABLE IV
FREQUENCY OF PATTERNS CHOSEN CORRECTLY REGARDLESS OF POSITION IN FIVE DRAWINGS

Size of pattern in number of correct places	Day				
	first	second	third	fourth	fifth
3	43	25	19	14	15
4	16	17	11	8	5
5	19	16	5	2	5
6	5	2	3	1	2
7	5	2	3	1	2
8		2		1	1
9	1	4	3	2	1
10	2	5	6	4	2
11	2	12	13	8	9
12	1	1	3	2	3
13	1	3	5	10	3
14		1		3	5
15			3	4	3
16			3	4	1
17					
18					
19				1	2
20				3	4
21			1	2	
22			1		1
36		1	2	4	7

of correct patterns of different numbers of bolt-heads as shown in the drawings for each of the five days is shown in Table IV.

From these results it seems that from the first drawing to the fifth patterns are being developed. Inspection of the table reveals that patterns of three are the most frequent, but that these are greatly reduced in two steps, from the first to the second drawing and from the third to the fourth. As with the 3-bolt patterns, 4 and 5-bolt patterns show a marked decrease while patterns of 11, 13 and 36 bolts show an increase. It is apparent that the 3-bolt patterns are built into larger configurations as the learning proceeds. Patterns of 11 and 13 appear to be intermediate between the small patterns of 3 bolts and the total pattern of 36. The explanation of the decrease of the number of smaller patterns would appear to lie in the increasing differentiation of the true path and the integration of the smaller into the larger patterns.

In order to illustrate the development of the patterns the successive drawings of 4 Ss (*A*, *B*, *C*, and *D*) are reproduced. Three of the series were made by Ss who had reached a criterion of mastery, the fourth, by *D*, is a series made by one of the poorer learners.

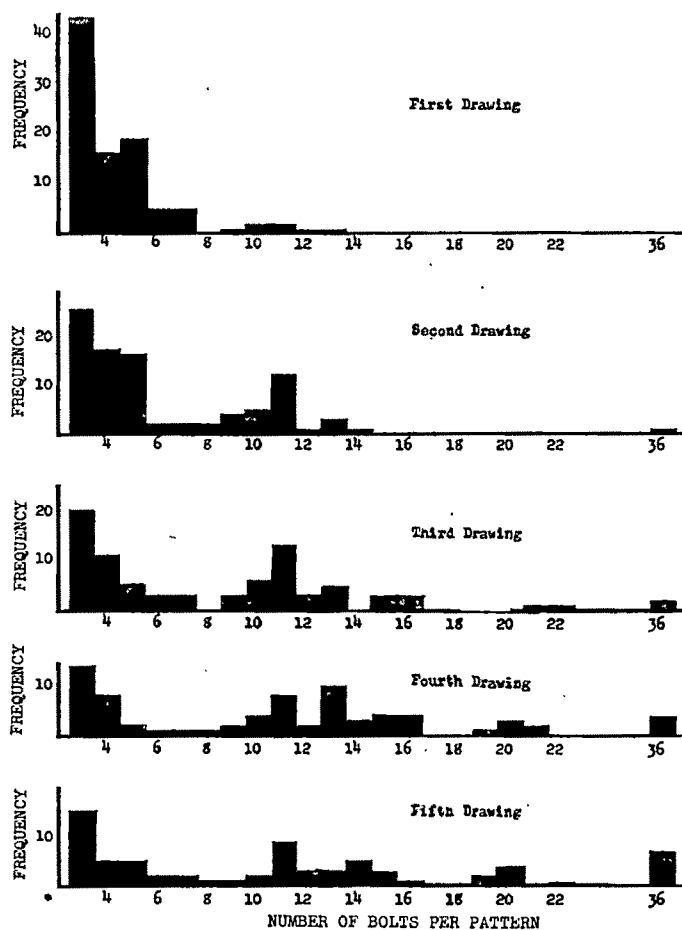


FIG. 6. GRAPHS OF FREQUENCY OF PATTERNS
(Correct places regardless of position.)

It will be seen that the patterns around the bolt-heads at the start and the finish are those first learned. The building up of the patterns commences from these areas and extends towards the middle of the maze. In each case, the drawings seem to indicate a differentiation of a pattern

against a ground, the development of minor organized groups or patterns and their integration into larger groups. This development does not appear

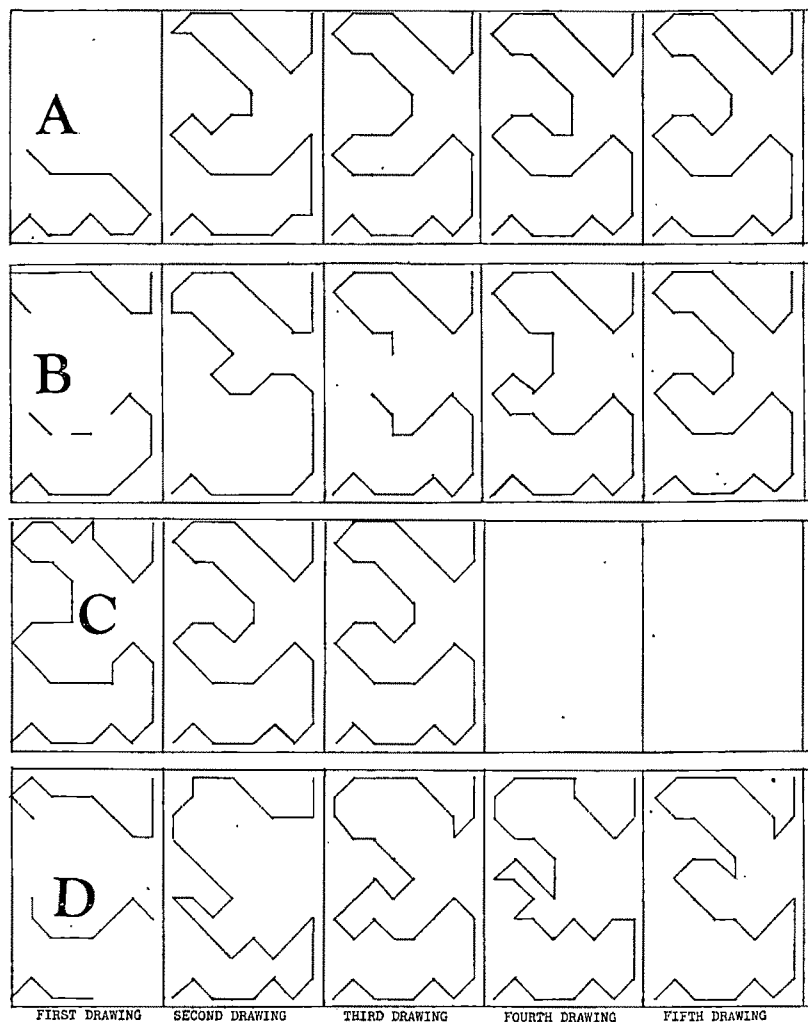


FIG. 7. REPRODUCTION OF FOUR OF THE S's MAZE DRAWINGS MADE ON SUCCESSIVE DAYS

to take place by mere addition of smaller patterns; what is happening is apparently the development of new wholes against a ground which is becoming progressively less homogeneous.

SUMMARY AND CONCLUSIONS

(1) An electric maze was developed for this experiment, its reliability when applied to 25 university students was found to be 0.84 for time and 0.90 for errors (split-halves method).

(2) A control group of 17 Ss and an experimental group of 26 Ss ran three trials a day for five successive days. At the end of each day, the experimental group was asked to draw a map of the pathway.

(3) Since there was no significant difference between the mean results of the control and the experimental groups it may be assumed that the map drawing had no measurable effect upon the learning of the experimental group.

(4) Those parts of the maze nearest to the starting and finishing points were learned first, either because of factors of primacy and recency or because of cues offered by the boundaries of the maze.

(5) Measures of the frequencies of patterns which included different numbers of bolt-heads were made. Patterns of three predominated, but patterns made up of greater numbers of bolt-heads were soon built up.

(6) Analysis of the maps indicated that patterns consisting of smaller wholes were first developed; these soon gave way to patterns consisting of larger wholes.

(7) It is suggested that the larger wholes consisted not of patterns made by the addition of smaller wholes, but of patterns formed of new wholes developed out of the total ground by the process of learning.

MNEMIC INHIBITION AS A FACTOR IN THE LIMITATION OF THE MEMORY SPAN

By CLARENCE W. YOUNG, Colgate University, and MICHAEL SUPA,
Cornell University

One of the best known phenomena in the field of memory experimentation is that of retroactive inhibition, which, of course, refers to the fact that the degree of retention is dependent upon the type of activity which intervenes between the time of learning and the time at which retention is tested. The most distinctive law of retroactive inhibition is that the inhibition increases with increase in the similarity of the interpolated activity to the task of learning and reproduction, up to the point where similarity becomes so great that practice or positive transfer appears.

This law also applies to the less well-known phenomenon of proactive inhibition; namely, the inhibition of both learning and retention as a function of the activity preceding the period of learning. Proactive inhibition also follows other laws characteristic of retroactive inhibition; and it seems obvious that they are essentially the same sort of thing and are presumably the results of the same set of causes. We propose, therefore, to apply to them the general term, "*mnemic inhibition*," which we define as the *functional relationship between mnemic decrement and the mental activities environing the mnemic process*. By "mnemic decrement" we mean any loss of efficiency in learning or any impairment of recall, relearning, recognition or other test of retention. By "mental activities environing the mnemic process," we mean activities preceding the act of learning or interpolated between learning and a test of retention, or occurring concomitantly with the act of learning or a test of retention. The mnemic inhibitory effect of concomitant activities would ordinarily be classed as "distraction."

Foucault was apparently the first to point out, in 1927, that in the learning of a series, such as the series of digits involved in the study of the memory span, the members of the series may be supposed to exert both retroactive and proactive inhibition upon one another.¹ The logic of this assumption is well stated by Woodworth, who writes: "Grant retroactive and proactive inhibition between two temporally adjacent lessons, and you can hardly deny its presence within a single lesson composed of a number of items. Work on the later items weakens the hold already

* Accepted for publication July 8, 1941.

¹M. Foucault, Les inhibitions internes de fixation, *Ann. Psychol.*, 29, 1928, 92-113.

gained on the earlier items; and the longer the list, the greater the amount of this mutual inhibition. Were there no such inhibitory factor at work, *O* could grasp the amount of one memory span, grasp another equal amount, and then recite the whole double span."²

Foucault, in fact, seems to have suggested all the possible forms of mnemonic inhibition; namely, "*inhibition regressive externe*" (ordinary retroactive inhibition), "*inhibition progressive externe*" (ordinary proactive inhibition), "*inhibition externe concomitant*" (distraction), and finally, "*inhibition interne*" (the mutual inhibitory effect of the members of a series). His theory of internal inhibition is stated in mentalistic terms. He points out that in reading a series of terms, forgetting begins as the series grows longer. This, he says, is because succeeding images drive preceding images below the threshold of consciousness and finally below the threshold of voluntary recall. Similarly, preceding images hinder perception and fixation of subsequent ones.

Foucault attempted to demonstrate internal inhibition through the study of the serial position curve in the immediate memory span for series of words. He found, as usual, that the terms at the beginning and end of the series were forgotten less frequently than the terms in the middle, and he explained this by assuming that the middle words, being subject to a greater concentration of both proactive and retroactive inhibition, were more strongly inhibited than the earlier and later terms of the series. It should be clear that in this demonstration Foucault did not actually show that an internal inhibition, similar in nature to external retroactive and proactive inhibition, actually takes place; he merely assumed internal mnemonic inhibition on logical grounds and employed the assumption to explain the familiar phenomenon of the advantage of primacy and recency. We are not attacking his assumption, since we believe it to be true. We merely point out that Foucault's experiment furnished no proof for it. The same may be said for other experiments, such as those of Gibson and Raffel³ and Philip,⁴ which investigate internal inhibition in terms of the serial position curve. Actually, as the discussions of these experiments make abundantly clear, the serial position curve is a function, not alone of mnemonic inhibition, but also of the method of learning employed. In the case of the memory span, for example, there is the tendency of the *S* to recite the first terms during the presentation of the latter ones, and, as the length of the series increases, there is a disposition to concentrate on the last few terms and let the middle ones go.

An empirical demonstration of the existence of internal mnemonic inhibition similar in nature to the well-known phenomenon of external retroactive inhibition could be provided by showing that the mnemonic decrement within a learning task is positively related to the similarity between the parts of the task. Such a demonstration would reveal that the law of the positive relationship between degree of inhibition and degree of similarity between the material learned and the environing material, holds among the parts of a single task of learning as well as for the learning material and interpolated material in a conventional retroaction experiment. So far as we

² R. S. Woodworth, *Experimental Psychology*, 1938, 231.

³ J. J. Gibson and Gertrude Raffel, A technique for investigating retroactive and other inhibitory effects in immediate memory, *J. Gen. Psychol.*, 15, 1936, 107-116.

⁴ B. R. Philip, Proactive and retroactive effects in the recognition of form, *J. Exper. Psychol.*, 26, 1940, 502-513.

know, Von Restorff is the only experimenter who has ever provided such a demonstration.⁵ We quote from Koffka's description of her experiment, "On three different days, the 15 Ss were shown once one of three series of 10 elements. After the presentation of the series, the Ss had to learn a meaningful text for 10 min., whereupon they were asked to write down as many items of the series as they could remember (method of retained members), a time of 30 sec. being allowed for this performance. On the first day they learned series (1), consisting of 10 different elements, viz. a number, a syllable, a colour, a letter, a word, a small photograph, a symbol, a button, a punctuation mark, and a name of a chemical compound. On the other two days either of the two series (2) and (3) were presented; (2) consisting of one number and nine syllables, (3) of one syllable and nine numbers."⁶

The "critical elements" in this experiment were the nonsense syllables and numbers. In series one they were presented in a "mixed relationship," with a number of environing items which were different from them and also different among themselves. In series two the syllables were presented in a "repetitive relationship," with a number of environing items which were similar to them, while the numbers were presented in an "isolated relationship," with a number of environing items different from them but similar to one another. In series three, the numbers were in the "repetitive relationship" and the syllables in the "isolated relationship." The total percentages of recall for both numbers and syllables in the three relationships were as follows: repetitive relationship 22%; mixed relationship 40%; isolated relationship 70%.

The comparison between the mixed relationship and the repetitive relationship demonstrates one of the fundamental laws of mnemonic inhibition; namely, the functional relationship between mnemonic decrement and the similarity of environing activities. Thus it indicates clearly that there is a process of internal inhibition identical in nature with external retroactive and proactive inhibition. The comparison between the mixed and isolated relationships suggests a law which, as far as we know, has never been demonstrated in any of the conventional experiments on retroactive inhibition; namely, that mnemonic decrement is a negative function of the similarity of the environing activities to one another.

THE EXPERIMENT

The present experiment was undertaken to determine whether a similar demonstration of internal inhibition could be made for the immediate memory span. It was planned to resemble as closely as possible the form of the conventional retroaction experiment.

Method and procedure. Our method was simply to determine whether the span for a series of entirely similar elements would be smaller than the span for series in which the last three elements were different in kind from the members with which the series began.

Subjects. The Ss were 34 Colgate University freshmen—all men.

⁵ H. von Restorff, Über die Wirkung Bereichsbildung im Spurenfeld, *Psychol. Forsch.*, 18, 1933, 299-342.

⁶ Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 486.

Procedure. For the first half of the experiment, the memory span of every *S* was determined for series of digits (*D*-series) and for series of digits and words combined (*DW*-series). The words composed the last three elements of each *DW*-series, irrespective of the total length of the series. In the second half of the experiment the span was tested for series of words (*W*-series) and for series of words and digits combined (*WD*-series), in which digits always constituted the last three elements of the series.

The digits used were 1, 2, 3, 4, 5, 6, 8, 9, and 0. Zero was pronounced as "Oh" and 7 was omitted, so that all elements in a series would be monosyllables. Nine monosyllabic names of common domestic animals were employed for the words. They were: hen, horse, goose, dog, cat, cow, pig, sheep, and ox. The list of words was restricted in this fashion in order to make the word series less difficult. It was desirable to find some type of material that would be approximately equal in difficulty to the digits, and although the memory span for words was almost a unit lower than that for digits, the words were the most nearly equal material that we could find.

The following are samples of series of eight units each in length:

D-series: 4 9 5 1 3 0 8 6
DW-series: 2 4 1 9 3 hen cow pig
W-series: goose cat ox sheep horse dog cow pig
WD-series: horse ox cat goose dog 6 0 4

The two halves of the experiment were performed on separate days with every *S*. Beginning at the level of six units per series, there was a presentation of three series of the uncombined type and three of the combined type, alternated to avoid giving special practice advantage to either type. For the first half of the experiment the order of presentation was as follows.

Length of series	Order of presentation of types of series					
6 units	D	DW	DW	D	D	DW
7 units	DW	D	D	DW	DW	D
8 units	D	DW	DW	D	D	DW
9 units	DW	D	D	DW	DW	D

This scheme of presentation was continued until *S* failed on all series at a given level. The same scheme was employed for the second half of the experiment. The first series presented at the level of 6 units was a *W*-series, the next two were *WD*-series, and so on.

The series were presented orally at the rate of one element per 0.75 sec. *E* timed his reading by the tick of a metronome, placed within his range of hearing, but sufficiently removed so that the *Ss* either did not notice it or else supposed it to be a clock and paid no attention to it.

Instructions. The *Ss* were tested one at a time in a room where they were alone with *E* and free of distractions. For the first half of the experiment the following instructions were given.

I am going to test your memory span. I will read to you lists of digits and lists of digits and words combined. When I finish reading you are to repeat each list as accurately as possible. For instance, I am now going to read a list of four digits and when I have finished you are to repeat them after me. Now, listen. [The list was read off and the subject allowed to repeat it.]

Now I am going to test you with a list of three digits and three words. The words will be names of common domestic animals, chosen from the following. [The list of nine words was read.]

Now listen carefully, remember there will be three digits followed by three words, and you are to repeat them all in the order in which I read them. [The test was made.] Now do you understand completely the sort of thing I want you to do? [Further informal instructions were given if necessary.]

I am going to read lists of digits or lists of digits followed by words. The digits will be chosen from 1, 2, 3, 4, 5, 6, 8, 9, or 0. The digit 7 will never appear. The words will be chosen from hen, horse, goose, dog, cat, cow, pig, sheep, or ox. I am going to begin with a series of six digits. Now listen. [This was the first series of the experimental sequence.]

[After S had recited, E said:] Now I am going to read you a list of three numbers and three words.

In this manner S was informed prior to every series how many digits or how many digits and words the series would contain. A "ready" signal was always given 0.75 sec. before the reading of each series.

The instructions for the second half of the experiment were similar, being modified only to account for the fact that the Ss were already familiar with the testing of the memory span. There were two practice series, as before, and the lists of digits and words from which the elements of the series were to be chosen were again presented.

Scoring. In scoring, the last span at which S reproduced all three series presented was taken as the base and one-third point was added for each successful reproduction above this level. In this manner comparable scores were secured for all four types of series.

This method of scoring was not always followed, however, for the series presented at the six-unit level. It was found, both for the first and second halves of the experiment, that the first series presented at this level was failed more frequently than might have been expected on the basis of the achievement on later series. This was true to a slighter degree for the second series presented. The effect was apparently due to failure to "settle down" to the experiment until after the first or the first and second presentations. Since the first series, which was an uncombined series in both halves of the experiment, was the one which suffered most, the counting of these failures would have tended to produce an error in the direction of a spurious confirmation of the hypothesis we were testing. Whenever the first series of either the combined or uncombined type was missed, therefore, the miss was not counted and 0.5 credit was given for each of the two remaining series of that type in the six-unit sequence.

Results. If the hypothesis that mnemonic inhibition is, in part at least, responsible for limitation of the immediate memory span is to be verified, the spans for the uncombined series should be shorter than those for the comparable combined series, since the last three units in the combined series should exert a lower degree of retroactive inhibition than the last three in the uncombined series; and they should also be subject to a lower degree of proactive inhibition. In other words, the D-series span should be lower than that of the DW-series, and the W-series span lower than that of the WD-series. It need not be true, however, that the WD-series

have a higher span than the D-series, since the former were composed predominantly of words, which are more difficult than digits. Similarly, the DW-series and the W-series are not significantly comparable.

The following are the means and standard deviations for the four types of series:

Series type	Mean	SD
D	7.19	1.235
DW	8.02	1.210
W	6.40	.942
WD	6.79	1.011

The following are the important differences derived from the analysis of the above data:

	Diff.	SE	CR
DW-mean minus D-mean (8.02—7.19)	.83	.154	5.38
WD-mean minus W-mean (6.79—6.40)	.39	.115	3.37
Superiority of DW to D minus superiority of WD to W (0.83—0.39)	.44	.192	2.30

The standard errors for the differences between the means are corrected for correlation. The product-moment coefficient of correlation between D and DW was 0.73; between W and WD, 0.76.

It will be seen that for both halves of the experiment the mean span for the combined series was greater than the mean span for the uncombined series. Our evidence is, therefore, entirely favorable to the hypothesis that mnemonic inhibition plays a part in the limitation of the memory span.

Since words are more difficult than digits, it might be supposed that the combination of words and digits would show a greater superiority to words alone than the combination of digits and words would show to digits alone. Actually, however, the advantage in relative superiority is in the opposite direction and the critical ratio for the difference between these differences is 2.30, which make the probability approximately 100 to 1 that this difference in superiority is not a result of chance.⁷ We believe that this apparently paradoxical result may be explained on the supposition that the difficulty of any type of material is more a function of its susceptibility to mnemonic inhibition than of its potency in causing inhibition. Thus, in the WD-series, the digits would exert proportionally greater retroactive effects on the words than the words would on the digits in the DW-series. Another way of viewing it is that there is an inverse rela-

⁷ The difference in superiority is still marked when the calculation is made in terms of percentages. The gain of the DW-series over the D-series is 12%; of the WD-series over the W-series, 6%.

tionship between degree of learning and the amount of retroactive inhibition. This assumption is in accord with the results of several studies of retroaction.⁸ Since the words, being difficult, would be less well learned, the digits would exert more retroaction on the words than the words on the digits.

Although we secured no introspective reports from our Ss these explanations accord with our own experience in trying out the material used in the experiment. When a combined series is presented, it is seldom necessary to expend much effort on the last three members. One listens to them "out of the corner of one's ear" while focussing attention on the review of the preceding units. It is much more difficult, however, to keep the words in mind in the WD-series than to keep the digits in mind in the DW-series. The words fade away rapidly, whereas the digits remain relatively well-fixed.

It is worth noting that the greater superiority of the DW-span over the D-span relative to the superiority of the WD-span over the W-span appears to be anomalous in terms of the combined difficulty of the individual members but that it can be credibly explained in terms of internal mnemonic inhibition. Thus this peculiar phenomenon provides additional, if somewhat indirect, evidence for the existence of internal mnemonic inhibition among members of a memory span series.

SUMMARY

(1) We have introduced the term "mnemonic inhibition" to signify the functional relationship between mnemonic decrement and the mental activities environing the mnemonic process.

(2) The best known form of mnemonic inhibition is retroactive inhibition. Proactive inhibition has also been demonstrated.

(3) On logical grounds it has been assumed that there is an internal mnemonic inhibition which the parts of any complex learning activity may exert upon one another, and the serial position curve for immediate memory span has been explained on the basis of this assumption.

(4) The existence of internal mnemonic inhibition, similar in nature to external retroactive inhibition, can be demonstrated empirically by showing that the mnemonic decrement within a learning task is positively related to the similarity between the parts of the task.

(5) Von Restorff has successfully demonstrated such a relationship for a ten-member series tested by the method of delayed recall.

(6) The present experiment shows that the memory span for series containing two dissimilar types of material is longer than the memory span for series containing material all of one kind, hence providing empirical evidence that the limitation of the memory span is, at least in part, a function of internal mnemonic inhibition.

⁸S. H. Britt, Retroactive inhibition: A review of the literature, *Psychol. Bull.*, 32, 1935, 381-400.

THE RECIPROCITY OF VISUAL CLEARNESS AND THE SPAN OF APPREHENSION

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Chapman and Brown¹ have described some experiments undertaken to verify with visual stimuli the principle of reciprocity of clearness and range of attention (apprehension span) formulated by Külpe² and Henning.³ As stated by Külpe, this principle is as follows: "The greater the range of attention, the lower is the degree of consciousness attaching to any individual content; while, vice versa, the number of objects grasped by attention decreases as concentration upon any one of them increases."⁴

Chapman and Brown rightly suggest that such a law should be capable of experimental proof. Their method of approach consisted in presenting to an *S* a series of cards on which were mounted printed capital letters about $\frac{1}{4}$ in. in height. Each card was exposed by means of a tachistoscope for 0.06 sec. Half the cards constituted a control series; the other half an experimental series. In the former all letters were of the same size, color, and relative position, while in the latter one letter (of eight) on each card differed in one of these respects. Three experiments were conducted; in one the difference between the cards of the control and the experimental series lay in the size of one letter; in a second, the difference was one of color; and in a third, it was one of the position of a letter. Throughout, the cards of the control and experimental series were merged and presented in a random order. The *Ss* were not informed of the purpose of the experiment, but were required simply to report immediately the letters they saw.

In analyzing their results, Chapman and Brown show that the single outstanding letter⁵ of the experimental series (that is, the letter on each card which differed in size, color, or relative position from the rest) were correctly reported by the *Ss* from 4% to 220% more often than the average letter of the control series. They determined for every *S* in the three experiments the mean number of letters correctly reported for the cards of the control series, and similarly for the experimental series. Nearly every *S* reported more letters correctly in the control series, although the differences at times were not great. Pooling the data from the three experiments without regard to differences in the mode of presentation, the rank correlation was determined between (1) the differences between the mean number of letters reported in the control and the experimental series, and (2) the "coefficients of clearness," which was a measure of the greater frequency with which the critical (large, red, or differently placed) letter was reported relative to the undifferentiated letters of the control series. For all 20 *Ss*, this correlation amounted to $+0.20 \pm .15$.

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¹ D. W. Chapman and H. E. Brown, The reciprocity of clearness and range of attention, *J. Gen. Psychol.*, 13, 1935, 357-366.

² O. Külpe, The problem of attention, *Monist*, 13, 1902, 36-68.

³ H. Henning, *Die Aufmerksamkeit*, 1925, 57.

⁴ Külpe, *op. cit.*, 57.

Considering only the 15 Ss in the first two of the three experiments, the correlation was $-0.09 \pm .18$.

These findings impelled Chapman and Brown to the following conclusions: "(1) All three types of change in the critical letters (size, color, position) were effective in enhancing the clearness of the letters. (2) This enhancement of clearness was accompanied by a reliable decrease in the range of attention, as would be predicted from the law of reciprocity. (3) Inconsistently with the law of reciprocity, no reliable correlation between the magnitude of the increase in clearness and the magnitude of the decrease in range was found under the conditions of this experiment."⁵

They point out, in regard to the third conclusion, that the explanation of the inconsistency may lie in the unsuitability of the materials or methods employed to demonstrate the law. They tend to reject, as repugnant to scientific thought, the alternative that the reciprocal relationship is not sufficiently quantitative and linear to yield any significant rank correlation. A further and somewhat different approach to the problem is, therefore, in order.

PROBLEM

The present investigation was conducted in an endeavor to demonstrate more effectively the linear form of the reciprocal relation between clearness and the span of apprehension, using materials and methods rather similar to those of Chapman and Brown. In revising the procedure, the principal criticism which it was felt might be directed against the original study concerned the attempt to exploit individual differences amongst Ss as a basis of correlation. This attempt implies acceptance of the proposition that those Ss for whom the critical letters stood out, relatively, more clearly should suffer greater depression of their span of apprehension. While this proposition may be true enough, it seems to involve a round-about approach to what was originally postulated as an individual law. In the present investigation, a direct approach, rather than an indirect procedure utilizing individual differences, was employed.

Two experiments were carried out, the second of these growing out of the experience resulting from the first.

EXPERIMENT 1

Methods and materials. Sixty cards, 6 in. wide and 3 in. high, were prepared for use with a tachistoscope having an exposure-time of 0.25 sec. On each card were pasted eight letters in Cheltenham type, arranged in the form of a circle having a diameter of 2 in. In terms of the positions of the hour hand on a clock-face, the letters were placed at 1:30, 3, 4:30, 6, 7:30, 9, 10:30 and 12. Certain letters, such as vowels, and those which might easily be confused with others, were avoided (e.g. N was used but M was not). This followed the procedure of Chapman and Brown in their first and second experiments. Certain changes, however, were made. The cards were divided into five groups of 12 cards each. In the first group of 12, all letters were of the same size, viz. 20 pt. (about 3/16 in. in height). In the second group, one of the eight letters was set in 24-pt. type;

⁵ *Op. cit.*, 364.

in the third, in 32-pt.; in the fourth, in 40-pt.; and in the fifth, in 54-pt. type. It was assumed that the larger the letter, the clearer would be its appearance by contrast with other smaller letters. In the second to fifth groups, the large letters were distributed at random amongst the eight possible positions on the card. In these groups, the seven remaining letters on each card were set in the same 20-pt. type, that was used for all letters in the first group of cards. The 60 cards were shuffled thoroughly, and were presented in this fixed, random order to all the Ss. The Ss were not informed of the purpose of the experiment, but were merely instructed to report whatever letters they saw. They were, however, shown six typical practice cards in advance of the regular series.

Subjects. In all, 20 Ss, men sophomore students in the introductory course in psychology, were used.

Results. Experiment 1 yielded, for every S, in accordance with the 5 groups of cards described above, 5 distributions of the number of letters correctly reported. Every distribution contained 12 cases, and had a possible range of from 0 to 8 letters. In the relevant instances, the critical letter itself, when reported correctly, was counted in determining the S's span. Following the method of product-moment correlation adapted to the case of an ordered qualitative variable and a quantitative variable described by Holzinger,⁶ the linear correlation was computed for every S between the number of letters reported and the size of the critical letter. There resulted a distribution of correlation coefficients having a range of $+0.10$ to -0.37 and an arithmetic mean of -0.21 . While this evidently suggested a tendency toward inverse relationship between the size of the critical letter and the number of letters reported, the tendency was so slight as to be barely meaningful.

In searching for reasons by which to explain the small magnitude of this mean correlation a detailed examination was made of the individual records. One finding which this enquiry brought to light was the tendency of the Ss to report particularly the letters in the upper half of the circle, and (with some inversions) to follow the ordinary reading habit of noting the left-hand letters first.⁷ As a result, in the four sets of cards containing the critical letters, the critical letter itself was often not noticed at all if it happened to be in the bottom half of the circle. A separation of the cards in which the critical letter was placed in the upper half of the circle suggested that it was principally these cards which provided the basis for the slight negative correlation previously discovered. Furthermore, a comparison of the results obtained for the first 20 cards taken in their order of presentation, indicated that at first the critical letters had enjoyed a novelty-value which gradually declined until toward the end they appeared to occupy no more of S's attention than the letters of normal size, *i.e.* 20 pt.

⁶ K. J. Holzinger, *Statistical Methods for Students in Education*, 1928, 260-262.

⁷ These findings corroborate the results first obtained by Dallenbach, and since verified by several investigators, that positions above and to the left have attentional advantage. (Cf. K. M. Dallenbach, Position *vs.* intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-286; Ruth Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left handed observers, *ibid.*, 35, 1924, 267-268; H. F. Brandt, Ocular patterns and their psychological implications, *ibid.*, 53, 1940, 266 f.; Ocular patterns in visual learning, *ibid.*, 54, 1941, 528.)

EXPERIMENT 2

As a result of the evident shortcomings in the materials used in the Experiment 1, it was decided to run a further series of trials in which allowance could be made for the position of the critical letter and its novelty-value.

Methods and materials. Sixty cards were used as before. The 12 cards of the first group, containing nothing but letters in 20-pt. size arranged around a 2-in. circle, were the same cards that were used in Experiment 1. The remaining 48 cards were divided into four groups of 12 cards each as before. The critical letters of varying size employed in the first experiment were replaced by letter, picture, and number and geometric forms, each one different from the next. Some of these critical factors were in color, others in black and white. They were graduated in four sizes, viz. $\frac{1}{4}$ -in., $\frac{3}{8}$ -in., $\frac{1}{2}$ -in., and 1 in. sq. The same assumption was made as before with respect to the increasing clearness-value of these factors with greater size.⁸ Twelve cards were provided with each size of critical factor. The remaining seven positions on each of these 48 cards were occupied as before by letters of 20-pt. size. The critical factors were located in three positions only on the circle: in terms of the hour hand on the clock face, at the 10:30, 12, and 1:30 positions. These changes, it was hoped, would ensure that the novelty-value of the critical factor would be sustained throughout the 60 cards (as the Ss would never be certain what kind of form might next be shown) and that the critical factor (when it appeared) would always be in a position to be noticed. The 60 cards thus prepared were shuffled thoroughly, and presented in a fixed, random order with a tachistoscopic exposure of 0.25 sec. The Ss were not informed of the purpose of the experiment, but were shown seven cards in advance of the regular series in order to accommodate themselves to the task of observation. The point of observation was from 10 to 24 in. in front of the tachistoscope, the distance differing according to S's preference. In determining the number of letters reported for each card, the critical factor whenever present and reported was counted as a letter, provided that S's description of it was sufficiently clear to leave no doubt that he had perceived it.

Subjects. In all 40 Ss, 13 women and 27 men, sophomore students in the introductory class in psychology, were used. Their motivation throughout the experiment was judged to be high.

Results. As in Experiment 1, the 60 reports for every S were divisible into five sets depending upon the size of the critical factor or its total absence. Using the method of correlation previously employed, the degree of linear relationship between the size of the critical factor and the number of letters correctly reported was determined in each case. This treatment of the raw data provided 40 correlation coefficients (one for every S). The distribution of these coefficients of correlation is shown in Fig. 1. Their negative trend is apparent, the range being from 0.00 to -0.61, with a mean value of -0.35. In Table I are shown examples of high, medium, and low correlational scatters.

⁸ As the results turned out, this assumption was necessitated by the fact that the critical factor was nearly always reported correctly whenever it appeared.

One feature of the method of treatment of the data employed is that it facilitates analysis of the causes of individual differences amongst Ss in the degree to which the law of reciprocity of clearness and apprehension-span tends to hold. Such analysis may be made by correlating the coefficients obtained (or some function of these

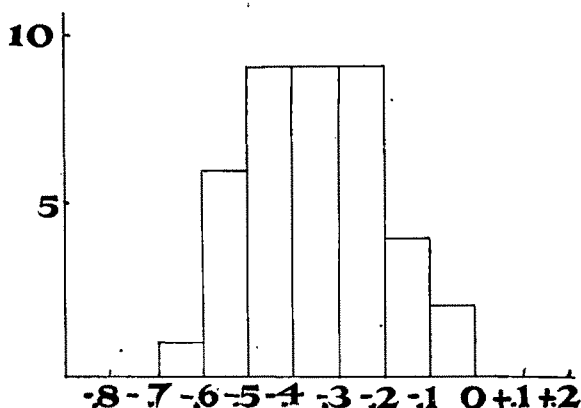


FIG. 1. THE DISTRIBUTION OF THE COEFFICIENTS OF CORRELATION

coefficients) with other characteristics in which the Ss differed. In this study, however, few additional data concerning the Ss were available. Age, and raw scores upon the Otis Self-Administering Test of Mental Ability (Higher, Form D) were found to be unrelated to the size of the correlation coefficients. Other perceptual factors,

TABLE I

EXAMPLES OF HIGH, MEDIUM, AND LOW CORRELATIONAL SCATTERS

Showing for 3 Ss the number of letters correctly reported for various stimulus-cards.

No. of letters correctly reported	Subjects															
	R.A.					A.B.					B.L.					
	Norm.		Size of critical factor			Norm.		Size of critical factor			Norm.		Size of critical factor			
	1/4	3/8	1/2	1	1/4	3/8	1/2	1	1/4	3/8	1/2	1				
5	4	1														
4	4	6			4	2	1	1			3	6	5	4	5	
3	4	4	2	6	5		6	5	4	3	2	8	5	4	8	5
2		1	4	2	3		3	5	5	6	2	1	1	2	2	2
1			6		2		2	2	2	3	8		1			
			r = -0.61					r = -0.35					r = -0.01			

as well as factors of attitude, might, however, be shown to have some bearing upon the outcome.

Although the trend of relationship between the size of the critical factor and the span of apprehension was inverse for all the Ss, the fact that the coefficients of correlation were considerably less than unity may be accounted for in several ways.

One reason may be the simple unreliability of the sample of 60 observations taken for every *S*. This explanation could easily be tested by repeating the procedure with the same *Ss* and a similar set of exposure-materials, or by a split-half analysis of the results obtained. A second reason why the coefficients were small may be due to differences in the attention-value of the critical factors. Though the factors used were regularly graduated in size, one of them might have engaged *S*'s attention to such an extent that he was able to report nothing else while another factor of the same size might be perceived more readily leaving time for the observation of the other exposure materials. A third reason may be the sequence in which the stimulus-cards were exposed. That the sequence affected the *Ss*' reports there is no doubt. For example: if a card having a large critical factor was immediately followed by one having none, the *Ss* usually reported fewer items for the latter than if the first card had contained a critical factor of moderate size. The reason for this appears to be a matter of expectancy. When a large critical factor appears upon one card, *S* expects it to appear on the one following; if it does not he wastes time searching for it and is consequently unable to observe and to report much else. When the preceding card contains a relatively small critical factor, *S* is less expectant of the appearance of the large factor, he wastes no time in searching and consequently observes and reports more. An increase in the number of stimulus-cards, a reduction in the variation of the attention-value of the critical factors, and the control of the sequence of the stimulus-cards might increase the size of the coefficients. If not, then it may well be that the coefficients are small because there is, as Dallenbach maintains,⁹ only a slight correlation between visual clearness and the span of apprehension.

SUMMARY

By a modification of the method used by Chapman and Brown, there has been demonstrated a tendency toward an inverse linear relationship between clearness (as represented by graduated sizes of visual attention-distracting factors) and the visual span of apprehension, thus fulfilling in some degree the theoretical formulation of such a relationship by Külpe and Henning. Various conditions are suggested which might, if controlled, increase the demonstrable strength of this tendency.

⁹ Dallenbach, *Attributive vs. cognitive clearness*, *J. Exper. Psychol.*, 3, 1920, 228 f. Cf. also A. D. Glanville and Dallenbach, *The range of attention*, this JOURNAL, 41, 1929, 207-236, esp. 236.

EFFECT OF VISUAL ADAPTATION UPON INTENSITY OF LIGHT PREFERRED FOR READING

By MILES A. TINKER, University of Minnesota

Is the preference of the reader an adequate means of determining the intensity of light needed for easy and comfortable reading? In certain instances, the light intensity preferred for reading has been employed for two purposes: (1) As supplementary data in prescribing foot-candle standards of illumination for reading; and (2) to help persuade consumers that a certain level of light intensity is necessary.

Analysis of the data on light preferences raises certain important questions. Luckiesh and Moss had each of 82 Ss select a light intensity he considered ideal for reading good sized black print on white paper for long periods.¹ Choice was solely in terms of comfort afforded by the lighting. Ten to 1000 foot-candles were available. Unfortunately the authors make no statement concerning control of visual adaptation and whether general or local lighting was employed. The distribution of preferences was: 11% at 10 foot-candles, 18% at 20, 32% at 50, 20% at 100, 17% at 200, 1% at 500, and 1% at 1000. The average is slightly less than 100 foot-candles and the median (more representative for this skewed distribution) is 50 foot-candles. By reference to the average of about 100 foot-candles chosen, the authors argue that this experimental evidence emphasizes the conservative character of the foot-candles recommended by them.² In another report, Luckiesh states that a large number of Ss preferred on the average 370 foot-candles of light for comfortable reading.³ The Ss, with no control of visual adaptation, viewed the illuminated page of a telephone directory (very illegible material) through a small window in a black box.

Results from earlier experiments should be cited. Luckiesh, Taylor, and Sinden set up an experiment under favorable conditions of light distribution and with general illumination of the room.⁴ Up to 30 foot-candles of light were available. They found that on the average readers preferred 5.3 foot-candles for reading large type (11 and 12 pt.) and 10.6 to 16.1 for medium sized type (9 pt.). With a low degree of brightness contrast between 9 pt. type and the paper, the readers chose

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¹ M. Luckiesh and F. K. Moss, *The New Science of Lighting*, 1934, 1-36.

² These recommendations are without valid experimental foundation. See M. A. Tinker, Cautions concerning illumination intensities for reading, *Amer. J. Optom.*, 12, 1935, 43-51.

³ M. Luckiesh, The eyes should have it (An interview by C. Brooks), *Good Housekeeping*, Nov., 1934, 88-89; 201-202.

⁴ M. Luckiesh, A. H. Taylor, and R. H. Sinden, Data pertaining to visual discrimination and desired illumination intensities, *J. Franklin Inst.*, 192, 1921, 757-772.

17.4 foot-candles. In another study Luckiesh and Taylor obtained preferences for light intensities when reading 9 pt. type under general, well distributed illumination.⁵ With values up to 8 foot-candles available, the readers chose 4.2; with 30 available, 10.6; with 45, 16.1; with 65, 23.2; and with 100 foot-candles available they chose 35.8. Why do the readers choose such a variety of intensities for comfortable reading? It is well known that the eye readily adapts itself to easy and efficient seeing at various intensities of illumination above a certain minimum.⁶ Apparently in none of these experiments was there any attempt to control visual adaptation. Probably the wide variety of intensities preferred may be explained in terms of visual adaptation.

PROBLEM

The purpose of this experiment is to measure the degree to which the illumination intensity chosen for comfortable reading is determined by the illumination level to which the eye is adapted.

PROCEDURE

The room in which the experiment was conducted is 15 ft. long, 9 ft. wide and 11½ ft. high. The light reflection factor of the ceiling was 75%, and of the walls, 50%. Indirect illumination was derived from 5 Pittsburgh Permaflexor luminaires suspended 27 in. from the ceiling. The range of illumination intensities available at the working surface was from approximately 1 to 100 foot-candles. The reading table was near the center of the room. During observation, one-half of the Ss faced the door of the room, the other half faced the opposite wall.

There were 144 university students who served as Ss. Every S served for two sessions of 50 min. each.

The experiment was designed to discover what intensity of light the S would choose for ease and comfort in reading after his eyes were adapted to a certain standard level of illumination. At one sitting this level was 8 foot-candles, at the other sitting, 52 foot-candles. Half of the Ss began with the 8 foot-candles, the other half with the 52 foot-candles.

The Ss were tested one at a time. When an S arrived at the laboratory, he spent 15 min. adapting to the standard illumination (8 or 52 foot-candles). When 10 of these 15 min. were over, he was directed to do some sample reading of the printed material which was to be used in the test. This text (*Hovious, Flying the Printways*) was printed in a clear 11-pt. type with 2-pt. leading on a good quality of mat surfaced paper. At the end of the adaptation period the following directions were read to S.

"I am going to ask you to make comparisons between different degrees of lighting. You are to choose which of two lights you prefer for ease and comfort in reading. Read 3 or 4 lines under each light, and note how the type looks and then make your choice. You will be shown each light twice before the choice is required."

⁵ M. Luckiesh and A. H. Taylor, Illumination intensities chosen for reading, *Trans. I.E.S.*, 17, 1922, 269-272.

⁶ M. A. Tinker, Illumination standards for effective and comfortable vision, *J. Consult. Psychol.*, 3, 1939, 11-20.

Ten seconds after notifying *S* that the light now on was one of the lights to be compared, the standard was switched off and simultaneously the comparison light was switched on. Ten seconds later the whole procedure was repeated and then a choice asked for. *S*'s eyes were re-adapted to the standard intensity for 4 min. before the next comparison was made. The 8 foot-candle light was compared with 1, 2, 3, 5, 12, 18, 26, and 41 foot-candles taken in a random order. For every alternate *S* this random order was different. The 52 foot-candle standard was compared with 18, 30, 41, 46, 59, 62, 71, and 100 foot-candles. Controlled randomized presentation was followed. Thus every *S* made eight choices under each of the two conditions of adaptation. The results indicate that the range of illumination was adequate in each case.

The illumination intensities were checked with a recently calibrated General Electric Foot-Candle Meter every day to be sure that the intensities employed were accurate as planned.

RESULTS

The frequency with which every intensity was chosen as best for easy and comfortable reading was computed. Since the standard intensity was compared with each

TABLE I
EFFECT OF VISUAL ADAPTATION UPON INTENSITY OF LIGHT PREFERRED FOR READING

	Adapted to 8 foot-candles								
Foot-candles compared	1	2	3	5	8	12	18	26	41
Choice frequency	0	5	6	18	107	98	70	57	48
	Adapted to 52 foot-candles								
Foot-candles compared	18	30	41	46	52	59	62	71	100
Choice frequency	25	37	53	65	98	54	55	42	35

of the eight other intensities, it appeared eight times as often as each of the comparison lights. It was necessary, therefore, to divide the frequency of choice for the 8 foot-candles and for the 52 foot-candles by 8 to make these figures comparable with the others. The results, showing these adjusted data, are given in Table I. In the first row of each part of the table are listed the intensities of illumination employed. In the second line are given the frequencies with which each intensity was chosen as best for ease and comfort in reading the 11 pt. type. These data are shown to better advantage in Figs. 1 and 2.

It is obvious that the intensity of illumination to which the *Ss* are adapted plays a dominant rôle in the preferences. When adapted to 8 foot-candles (Fig. 1), the 8 foot-candle intensity is chosen most frequently. Note that a few choices are as low as 2 foot-candles but that the majority range from 8 to 41. The three most popular intensities are 8, 12, and 18 foot-candles. The median choice is 12 foot-candles.

The trend is similar but even more striking when the *Ss* were adapted to 52 foot-candles. Again there were more choices for the intensity to which the eye was adapted than for any other. The preferences ranged from 18 to the 100 foot-candle

intensities with a concentration at 41, 46, 52, 59 and 62. The median choice was at 52 foot-candles, *i.e.* the intensity to which the Ss were adapted.

Apparently visual adaptation at the moment determines to a large degree the illumination intensity preferred for reading ordinary print (11 pt. type). Results from experiments in which visual adaptation was not controlled can have little meaning. Apparently, with proper control of adaptation, one can obtain preference for almost any desired intensity of light. In the earlier experiments, cited above, the

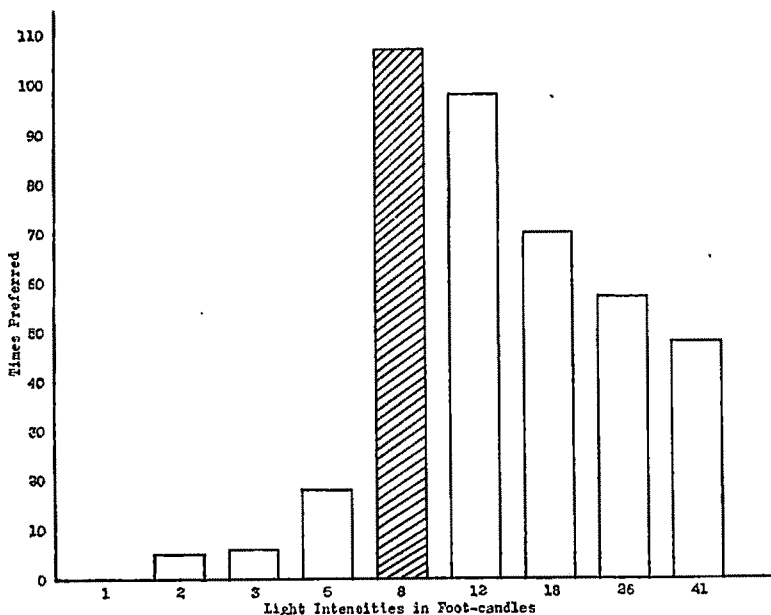


FIG. 1. EFFECT OF VISUAL ADAPTATION UPON INTENSITY OF LIGHT PREFERRED FOR READING
(Eyes adapted to 8 foot-candles.)

intensity of light chosen varied considerably from one situation to another when the range of light intensities to which the Ss were exposed was changed. This variation in preferred intensity was probably due to variation in visual adaptation of the Ss. It is not surprising, therefore, that preferred light intensity frequently fails to coincide with results from experiments to determine the illumination intensity needed for efficient visual work.

The results obtained in this experiment indicate that preference for light intensity cannot be employed as a satisfactory measure of effective illumination. Such data, therefore, should not form the basis for recommended standards of illumination. Furthermore, preferred intensity of light should not be used to persuade a consumer that one intensity is better than another for ease and efficiency in reading.

SUMMARY

- (1) The effect of visual adaptation upon intensity of light preferred for reading was determined.
- (2) One hundred and forty-four university students were used as Ss.
- (3) The Ss were adapted to 8 foot-candles of light at one sitting and to 52 foot-

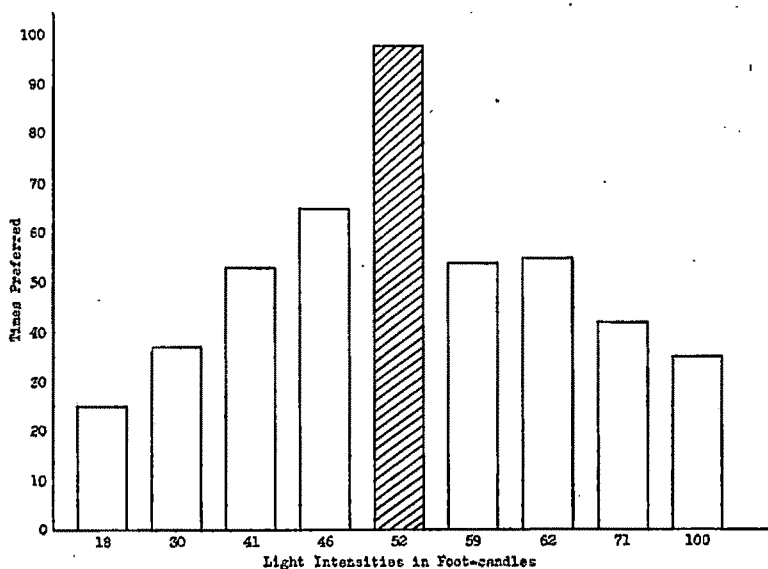


FIG. 2. EFFECT OF VISUAL ADAPTATION UPON INTENSITY OF LIGHT PREFERRED FOR READING
(Eyes adapted to 52 foot-candles.)

candles at another sitting. After adaptation, a paired comparison technique was employed to obtain preferences for intensities considered best for ease and comfort in reading 11 pt. type.

(4) The data indicate that status of visual adaptation at the moment determines to a large degree the intensity preferred for reading.

(5) Preference for illumination intensity, therefore, is not a satisfactory method for determining the intensity of light needed for efficient visual work.

APPARATUS

A CHAIR-STABILOMETER

By ALAN D. GRINSTED, Louisiana State University

The apparatus described here is similar in purpose to the stabilometers of Szymansky¹ and Renshaw.² It is more complicated than their devices and it has the advantage of recording *O*'s movements, when he is sitting in the chair, without his being aware of that fact. The movements of the different segments of the body are recorded, as well as those of the whole body.

The chair is, to all appearances, simply a comfortable morris chair with a foot rest. There is nothing about it to give *O* any reason to suspect that his movements are being recorded; although actually the back, seat, footrest, and two arms of the chair are separately responsive to every change of pressure upon them (see Fig. 1). All parts of the chair rest against the coil springs (*S*); and all, except the seat, are hinged at one of their ends (*H*)—the arms and footrest at the rear, and the back of the chair at the point where it is attached. The springs are heavy enough to prevent their being collapsed entirely by *O*'s weight but are still light enough so that any pressure will compress them to some degree. Thus, as *O* sits in the chair with his feet on the foot-rest and his arms on the chair-arms, any movement he makes (whether he changes bodily position, leans forward or backward, or moves his feet or arms or even his hands) causes one of the movable parts of the chair to change the pressure being exerted on the springs of that part.

From each of these parts of the chair a line (*L*) runs by means of pulleys (*P*) to the recording box, which is concealed behind the chair and is attached to the back of the seat. The attachment of each of these lines is such that the direction of its pull is opposite to the pressure exerted by the springs in the chair part to which it is attached, and any change in position of the part varies the tension upon the attached line.

The lines are made of wire (No. 24, copper), except where they run through the pulleys, where twisted Irish Cuttyhunk linen fishline (No. 12) is used in order that there may be free movement through the pulleys. Wire was used where possible for two reasons: first, because it is practically invisible where construction makes complete concealment difficult; and secondly, because it reduces the possibility of stretching under increases in strain. Since stretching would have a damping effect on the recording of the movements, the elimination or reduction of stretching was highly to be desired. The use of wire and cord as described reduces the stretching to an insignificant minimum. Under actual test, a 6-ft. line, made up of 4 ft. 6 in. of wire and 18 in. of fishline showed a stretch of only 3.25 mm. when a strain of 200 grm. was increased to 400 grm. These strains approximate closely those

¹ H. M. Johnson and T. H. Swan, Sleep, *Psychol. Bull.*, 27, 1930, 19.

² S. Renshaw and A. P. Weiss, Apparatus for measuring changes in bodily posture, this JOURNAL, 37, 1926, 261-267.

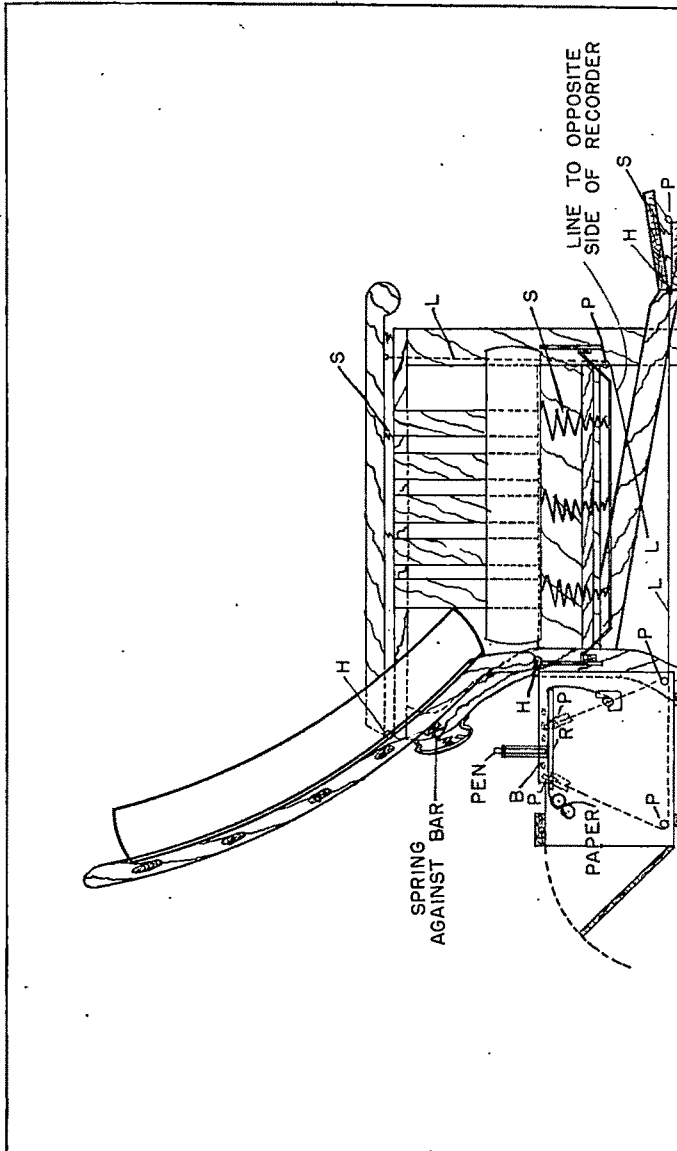


FIG. 1. CROSS SECTION, SIDE VIEW OF THE STABILOMETER

actually imposed on the lines used in the chair under working conditions, and in no case does any of the lines used in the chair exceed 6 ft. in length. In other words the stretch of the lines as used in the chair under working conditions was so slight that it had a negligible effect upon the recordings.

The lines, at the recording apparatus, attach directly to sliding bars (B) that move the recording pens (see Fig. 2). These bars are made of $\frac{1}{4}$ in. steel, and slide smoothly in fitted slots in a $10\frac{1}{2} \times 7\frac{1}{2}$ in. rectangular frame of $\frac{3}{8}$ in. steel, which is set in such a position as to bridge the recording platform (K) on which moving paper receives the impression of the pens which rest on it. Each sliding bar has attached to it, on the opposite end from that at which the line is fastened, a rubber band which is strong enough to pull constantly in opposition to the pull of the lines so that the lines are always kept under tension. Any relaxation on the pull of a line is immediately taken up by the pull of its opposing rubber band, and the bar between changes position accordingly. Each bar bears a thin metal arm or tongue with a $\frac{1}{16}$ -in. hole, and these arms vary in length as necessary to place the holes of the several arms in line with one another. In each hole rests the writing end of a No. 11 Inkograph pen, the writing point of which protrudes through the hole and rests against the moving sheet of paper. The upper part of each pen rests loosely in a hole in a supporting rack, which serves to hold it upright without obstructing its free movement. Thus, when increase or decrease in the tension of one of the lines as it pulls against its opposing rubber band causes movement of the intervening sliding bar, the arm which is attached moves the pen and records the movement on the moving paper.

The frame carries six sliding bars in all: five attached to the lines from the five moving parts of the chair, and the sixth attached similarly to the clapper arm of a bell magnet, which is wired to a concealed telegraph key for use in recording any signals the experimenter may wish to have on his record.

The paper on which the record is made unrolls from within a box on which the recording device is mounted, passes up through a slot and across the recording platform beneath the pens, and then between two rubber rollers and back into the box at the opposite end from where it emerged. It is pulled through by the two rollers, one of which is driven at a constant speed of 1 r.p.m. by a telecron electric motor. The length of a 1-min. record on the paper is, of course, equal to the diameter of the driven roller.

In experiments so far conducted with this apparatus, no *O* has been aware of the fact that his movements were being recorded or that there was anything especially peculiar about the chair. The motor, almost noiseless under normal conditions, is inclosed in a housing to reduce any hum that might be present. The paper is always within the box except where it passes beneath the pens, so that any noise caused by its occasional crumpling cannot be heard by the *O*. The pens used record without scratching, hence there is no distracting sound from that source; and the recording frame, when properly lubricated, is also noiseless.

The lines from the back and seat of the chair are, of course, easily concealed. The one from the footrest runs at one side from beneath that part, under the chair, to the recorder; and the few inches of wire exposed, running close to an oak colored brace, are practically unnoticeable. Similarly, the lines running from under the arms to small pulleys which guide them under the chair are close to the wooden uprights

that support the chair arm and are practically invisible to one not sitting in the chair, while they are completely concealed from the *O* who is in the chair.

Actual experiment with the chair-stabilometer has shown that it is possible to obtain with it satisfactory records of the bodily movements of anyone sitting in it without his becoming aware that records were being made and without his becoming suspicious that the chair was anything other than a very comfortable place in which to sit.

AN AUTOMATIC DEVICE FOR PROVIDING MOTIVATION AND RE- ENFORCEMENT IN OPERANT CONDITIONING

By JOHN VOLKMANN and FRED S. KELLER, Columbia University

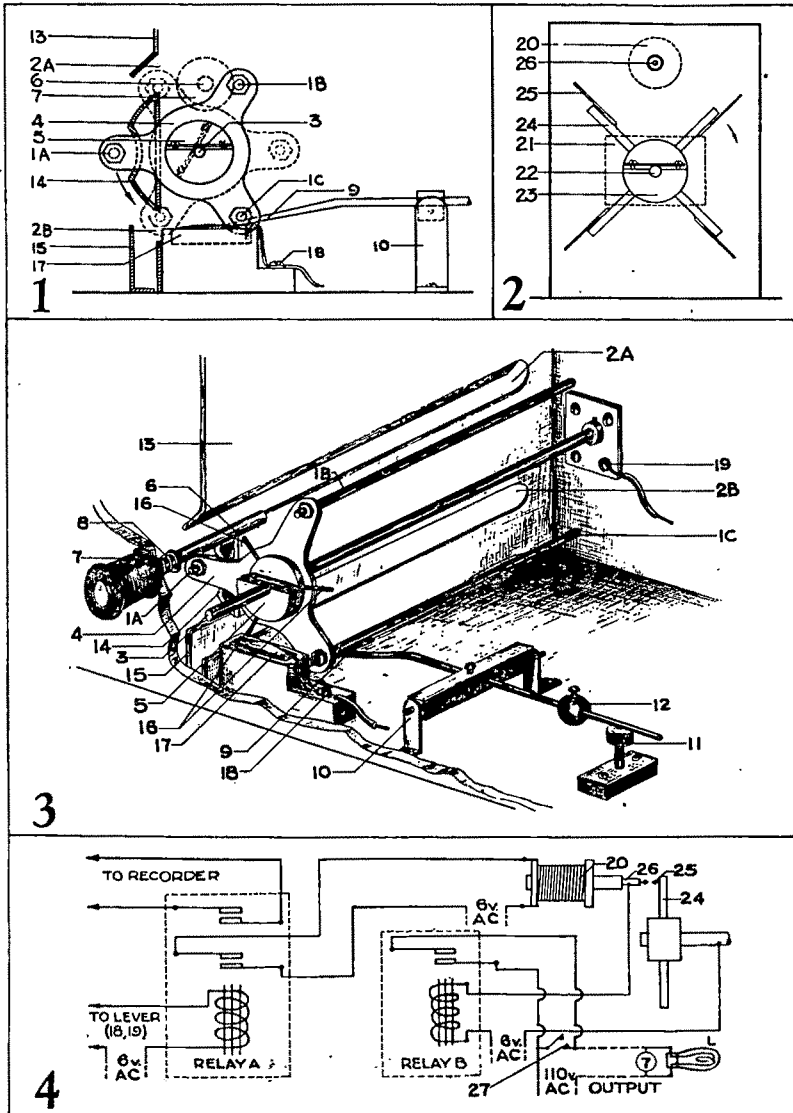
Although aversion drives have not been extensively used in the study of operant conditioning, they offer certain advantages which other drives, *e.g.* hunger and thirst, do not. They permit the rapid institution and removal of the drive-state and the intensive and temporal control of the motivating source. In addition reinforcement can follow immediately the emission of the operant; under hunger motivation several responses may intervene between the operant that is measured and its subsequent reinforcement. Lever-pressing, for example, is followed by the approach to a tray and the seizure of food. Moreover, the motivating source of an aversion-drive serves also as a discriminative stimulus, thus permitting the measurement of the 'latency,' 'force,' and the other properties of operants which resemble the properties of respondents.

An apparatus of proper design should provide for the intensive control of the motivational source, its rapid removal when the *S* responds, and its automatic return at the end of a definite period of reinforcement. Keller¹ has shown that the strength of the light-aversion drive can be regulated by varying the intensity of the light: when his rats responded by pressing a lever, he turned out the light for a period of one minute. Keller's study, however, revealed certain additional requirements. His rats not only pressed the lever, but they held it down or pressed it repeatedly during the period of reinforcement. Although this behavior raises its own interesting problems, it seems advisable, in order to secure regular functional relations, to restrict the response to a single abrupt lever-depression which produces the full period of reinforcement. Such a restriction permits the measurement of the 'latency' and the rate of responding without the possible interference of prolonged or repeated responding.

The apparatus constructed to meet these requirements consists of a box similar to that described by Skinner,² a special lever-mechanism, an automatic reinforcing device, and a lamp over the animal's chamber. The reinforcer and recording equipment are located in an adjoining room. During the training of the animal, this apparatus operates as follows: The reinforcer turns on the light over the animal and one of a set of three levers rotates into the animal's chamber; when the animal presses this lever, the light goes off. The lever rotates out of the chamber; hence, the animal cannot hold it down. All three levers are kept out of the animal's

¹ F. S. Keller, Light-aversion in the white rat, *Psychol. Rec.*, 4, 1941, 235-250.

² B. F. Skinner, *The Behavior of Organisms*, 1938, esp. 49.



FIGS. 1-4. AUTOMATIC DEVICE FOR OPERANT CONDITIONING (Fig. 1, side view of the lever-mechanism; Fig. 2, front view of the reënforcer-panel; Fig. 3, isometric drawing of the side and rear of the lever-mechanism; Fig. 4, wiring diagram of the reënforcer.)

chamber during reinforcement in order to prevent repeated pressing. At the end of the 1-min. period of reinforcement the light comes on again and a lever appears. During experimental extinction the light remains on, and after every depression a lever comes into place immediately.

Fig. 1 shows a side view of the lever-mechanism, and Fig. 3 an isometric drawing of the side and rear. The space at the left of each figure lies in the animal's chamber. Each lever is a $\frac{1}{8}$ in. round rod 5 in. long; the end of one is shown (1A) with the lever in position for pressing. The second and third levers (1B) and (1C) are in the other chamber. The animal's chamber is separated from the lever-mechanism by a partition; the levers rotate through slots (2A) and (2B). They are turned by a shaft (3), which is driven continuously by a motor at a speed of approximately 20 r.p.m. To a collar (5) is fastened a small flat spring which bears upon a shaft (3) and provides a light friction coupling with this shaft. The collar supports a three-cornered plate (4) and the attached levers (1A), (1B), and (1C). Thus when the plate (4) is not held by a positive stop or a greater friction, the shaft turns it and the three levers in the direction shown by the arrow in Fig. 1. It was found that this motion was sometimes impeded by the animal's nose or tail; accordingly, metal guards were placed at (14) within the radius of the levers, at (15) on the floor of the chamber, and over slot (2A).

During reinforcement all three levers are kept out of the animal's chamber by a positive stop (6), which is normally held in an inserted position by a spring (8). This position of the levers is shown by the dotted outline of the plate (4) in Fig. 1. At the end of the period of reinforcement, when the light goes on, a solenoid (7) pulls out the stop, and allows the plate to rotate into its operating position (solid outline, Fig. 1). Here it is held by a friction-stop (9), which interrupts the passage of the lever in position 1C. The friction-stop is mounted in a substantial bearing (10), and the amount of friction which it offers to the passage of the lever is regulated by a height-adjustment (11) and a counterweight (12). This regulation is critical; the amount of friction must safely exceed the friction of collar (5) and the rotatory inertia of the plate and levers, so that the levers will not ride past the stop. On the other hand, the friction must not be too great, lest the animal be unable to overcome it by depressing the lever. The apparatus now requires the animal to exert a force of approximately 17 grm.; this figure might be somewhat reduced by decreasing the rotatory inertia of the plate and levers. The speed of rotation should not be decreased, because it is desirable in experimental extinction to have the next lever appear promptly. The three levers should be set at equal radial distances from the shaft (3).

When the animal depresses it, the lever rotates downward out of his chamber. During this phase of the rotation, one of the contact-rods (16) passes through the mercury contact (17), thus making and then breaking a circuit through the leads (18) and (19). Fig. 4 shows these leads as they enter the reënförer through a connecting cable. Each time the animal depresses a lever, relay A closes and then opens. One pair of contacts on relay A, normally open, actuates a recorder; as previously described,³ this recorder draws a curve of the cumulative frequency of responding. A second pair of contacts, normally open, actuates a solenoid (20) mounted on the panel of the instrument (Figs. 2 and 4).

The panel supports a synchronous clock-motor (not shown), and a gear-box

³ Keller, *op. cit.*, 236.

(21) shown in outline only; the final shaft (22) from the gear-box runs at either 1 r.p.m. or $\frac{1}{4}$ r.p.m., according to the arrangement of the gears. On the shaft is a friction-collar (23), similar to the one already described. The collar holds 1, 2, or 4 radial rods (24); each rod has at its outer end a spring contact (25). This contact meets another contact (26) which is carried by the plunger of solenoid (20). Shaft (22) rotates continuously, and drives the friction-collar and the contact rods when they are not being held stationary by the contact (26), which serves as a positive stop. The period of reënförment, when the animal is in darkness, is set by the period of free rotation of the collar and contact rods. By arranging the gears and the number of contact rods, the following periods of reënförment may be obtained: 15 sec., 30 sec., 1 min., 2 min., and 4 min.

During the period of deprivation or non-reënförment, contact (26) acts as a stop, and it completes with contact (25) the operating circuit of relay B, a heavy-contact relay. The two contacts of this relay close the circuit which lights the lamp (L) over the animal's chamber; the same circuit operates the solenoid (7) which withdraws the stop (6) and allows the lever to rotate into position for pressing. Now, when the animal presses the lever and relay A closes and opens, the solenoid (20) is actuated; it releases the contact rod and friction collar, and these rotate; contacts (25) and (26) are thus opened, and relay B is released; the lamp (L) goes out and solenoid (7) releases, so that the stop (6) holds the rotating levers outside the animal's chamber. At the end of the period of reënförment, the friction-collar and rods have rotated until contacts (25) and (26) are again closed; relay B closes, and the apparatus is thus ready for the next depression of the lever. The 'latency' of the animal's response is the time between the automatic closing of the lamp-circuit and the next lever-depression; this time may be recorded on a moving tape from a second pair of contacts (not shown) on relay B.

To bring about experimental extinction the E closes a switch (27), which keeps lamp (L) on and the solenoid (7) actuated. Relay A still operates the recorder; relay B, solenoid (20) and the clock motor may be turned off. During both extinction and conditioning the operation of the apparatus is entirely automatic.

The design has certain limitations: it does not provide for automatic periodic reënförment, or for non-uniform periods of reënförment; it likewise does not provide for the measurement of the force of lever-depression or of the time during which an animal would hold the lever down. On the other hand, the design is flexible in certain respects. If it is desired to permit repeated pressing during reënförment, solenoid (7) may be fastened in the retracted position; the next lever then appears immediately after each pressing. In addition, the apparatus may be used to control the sources of other aversion-drives, and in this way may facilitate the study of an important type of motivation.

THE DEPAUW LABORATORY FOR RESEARCH ON THE PSYCHOLOGICAL PROBLEMS OF RADIO

By PAUL J. FAY and WARREN C. MIDDLETON, DePauw University

The laboratory for research on the psychological problems of radio is an integral unit of the psychological laboratory of DePauw University which was occupied at the

beginning of the academic year 1940-1941. Various units of the Psychological Laboratory are located on the ground and third floors of the John Harrison Hall of Natural Sciences. The radio rooms are on the third floor, which is serviced by an elevator. The floor plan, given in Fig. 1, shows only the rooms used in work with the radio. It omits the departmental waiting room, offices, clinical and statistical rooms, animal laboratories, shops and photographic dark room. It also omits, as does this paper, reference to power panels and equipment incident to research in experimental psychology.

The control room (Room J), which is completely suspended and insulated at all points of suspension, is sound-resistant and sound-absorbent. The walls and ceiling are lined with Celotex acoustical blocks over acoustical plaster. The floor covering is cork. Illumination is furnished by a light recessed in the ceiling and by an adjustable lamp for the recorder. The heavy refrigerator-type door of the control-room opens into a small hallway, the walls of which are also covered with acoustical blocks. The outer door of this hallway is inlaid with cork.

All equipment in the Radio Research Laboratory is standard RCA broadcasting and recording equipment. The heart of the system is a custom-designed RCA studio control-console which is located in front of the window separating the control-room from the studio (see Fig. 2). The experimenter can observe the speakers in the studio and, at the same time, can conveniently operate the recorder and reproducer. A high gain amplifier is incorporated in the pedestal. Two microphone pre-amplifiers are mounted on the main amplifier.

On the control panel are 20 pairs of input switches with pilot lights. Fifteen of the inputs are for microphones; one each are for the recorder, the reproducer turntables, and the radio tuner. The dual channel hook-up permits the simultaneous use of any two inputs. Sixteen speaker control-switches enable the experimenter to send a program to any or all of the loud-speakers. Since there is a hook-up with the public telephone system, a program may be transmitted to a radio station for broadcasting. A listening position on each switch makes possible the use of the loud-speakers as part of a two-way communication-system. This communication-system also includes a talk-back communication switch and a microphone jack on the control-panel. There are independent volume-controls for the two input channels, a master volume-control, a volume-control for the monitor-speaker, and a volume-indicator meter. Headphones or a loud-speaker over the studio window may be used for monitoring. Dialogue-equalizer controls make it possible to eliminate most of the frequencies above or below 1000~.

High fidelity recordings are made on the RCA Recording and Instantaneous Playback Equipment. The cutting-head, mounted on a calibrated cutting-arm to indicate recording-time, operates inside-out. The 16-in. turntable is operated by a synchronous motor at 78 or 33 $\frac{1}{3}$ r.p.m., so that records may be produced with a playing time up to 15.1 min. Programs may be recorded from any of the input channels of the console, including direct recording from the radio-tuner or from the reproducer turntables. Since the recorder has its own 12-in. loud-speaker, as well as independent tone-control, volume-control, volume-indicator meter, and monitor-jack, it may be used independently of the public address system.

In the rear of the control-room are located an RCA two-turntable reproducer and

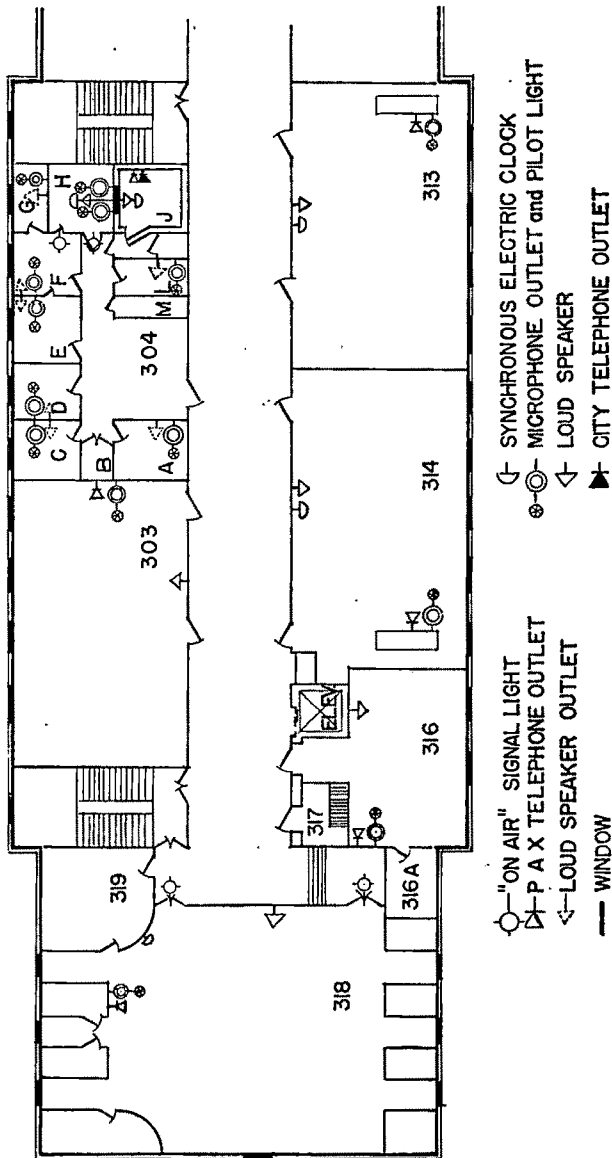


FIG. 1. FLOOR PLAN OF RADIO LABORATORY

an all-wave superheterodyne RCA radio. A synchronous electric clock over the window is synchronized with a similar clock in the studio. A PAX telephone system enables the experimenter to communicate with the audition-director in any of the audition-rooms. A graphoscope is used for analysis of voices. The oscillograms are recorded by a motor-driven cathode ray oscillographic camera.

The experimental programs usually originate in Studio H, although they may

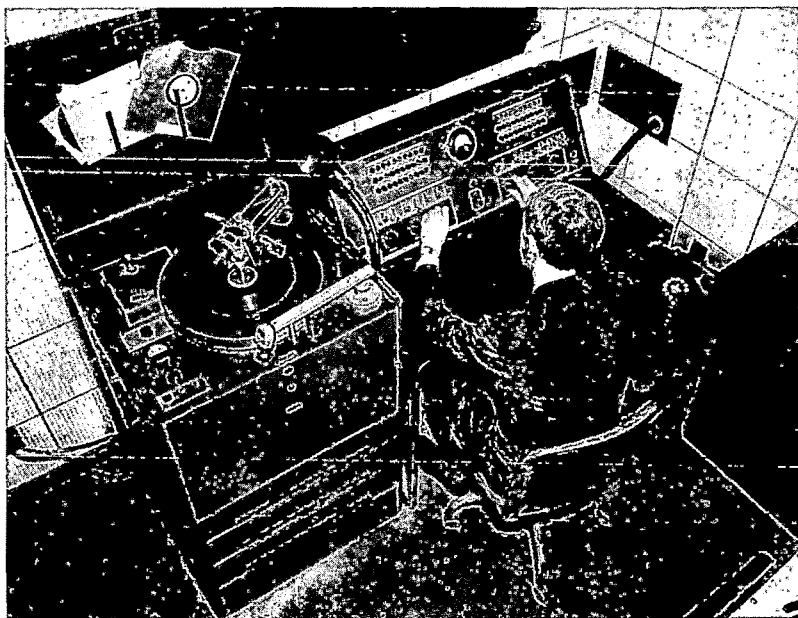


FIG. 2. FRONT OF CONTROL ROOM

originate in the control-room, or in any of the 13 rooms which are supplied with microphone-receptacles. Two microphones may be used simultaneously in the studio. When any microphone-circuit is opened, a pilot lamp on the same panel as the corresponding microphone-jack is lighted. One of the microphones currently used in the laboratory is an RCA pressure-operated microphone. If used in a horizontal position, the pick-up pattern is semi-directional; if it is vertical, it has a non-directional pick-up characteristic. This microphone has been found to be most satisfactory for recording individual voices. The other microphone which is used more for group work is an RCA three-way velocity microphone. It may be adjusted so as to have either a uni-directional, a bi-directional, or a non-directional pick-up characteristic. The experimenter may monitor programs either with the loud-speaker located over the window of the control room or with headphones plugged into the panel jack.

The walls and ceiling of Studio H are covered with acoustical blocks and a thick rug is available for the floor. A cork-board top is provided for the studio table. A double plate-glass window has been built into the wall which separates the studio from the control-room. An "On-Air" signal-light is located over each of the studio doors; both signals are automatically lighted when either of the studio microphone-channels is open.

Radio Station WIRE of Indianapolis has fitted up Room 318 as a remote studio. From here radio programs produced by students and faculty members of DePauw University are regularly broadcast over Station WIRE. An "On-Air" signal light is located over each of the exit doors. The microphones and control-panel used in broadcasting are furnished by the radio station. Through the panel receptacle at the front of Room 318 the microphone-circuit is connected with the console in the control-room (Room J). Here it may be switched to the outside telephone line and to the recorder, so that the program may be simultaneously broadcast and recorded.

In addition to the two primary studios there are 11 reserve studios (Rooms A, C, D, E, F, G, L, 303, 313, 314, and 316). A panel in each of these rooms contains a microphone receptacle and a pilot light. The ceilings in all rooms are of acoustical plaster.

Five rooms of varying size may be used for audition purposes—a lecture room (318), two class-rooms (303 and 316), and two general laboratories (313, 314). The regular seating capacity of these rooms is, respectively, 175, 70, 35, 32, and 32. It is, therefore, possible to have approximately 350 listeners participate in any one experiment.¹ An RCA dynamic loud-speaker is mounted in each of the audition-rooms. The wall-panel in each of the small rooms (Rooms A, C, D, E, F, G, and L) contains a jack for plugging in a loud-speaker.²

¹ Many laboratory experiments on the radio have suffered from a paucity of auditors. In some cases the chief difficulty consists of insufficient seating facilities. A few experiments have been performed in the laboratory of DePauw University in which the entire seating capacity of the audition rooms was utilized.

² The following studies, all by the authors of this note, have come from the radio unit of the DePauw Psychological Laboratory: Judgment of occupation from the voice as transmitted over a public address system and over a radio, *J. Appl. Psychol.*, 23, 1939, 586-601; Judgment of Spranger personality types from the voice as transmitted over a public address system, *Char. & Pers.*, 8, 1939, 144-155; Judgment of intelligence from the voice as transmitted over a public address system, *Sociometry*, 3, 1940, 186-191; Judgment of Kretchmerian body types from the voice as transmitted over a public address system, *J. Soc. Psychol.*, 12, 1940, 151-162; The ability to judge the rested or tired condition of a speaker from his voice as transmitted over a public address system, *J. Appl. Psychol.*, 24, 1940, 645-650; The ability to tell truth-telling, or lying, from the voice as transmitted over a public address system, *J. Gen. Psychol.*, 24, 1941, 211-215; Rating a speaker's natural voice when heard over a public address system, *Quart. J. Speech*, 27, 1941, 120-124; The ability to judge sociability from the voice as transmitted over a public address system, *J. Soc. Psychol.*; (in press); and The ability to judge self-confidence from the voice as transmitted over a public address system, *Quart. J. Speech* (in press).

STATIC ATAXIAMETER FOR HEAD AND HIPS

By A. S. EDWARDS, University of Georgia

The development of an inexpensive ataxiameter has continued at the University of Georgia for some time. A heavy metal model was described in 1939.¹ Modifications have been made in a number of ways. Most of the parts are now made of wood instead of metal. The apparatus makes it possible to measure body sway in millimeters in four directions—front, back, right and left—at both head and hips. The apparatus for the head measurements is raised and lowered by a platform

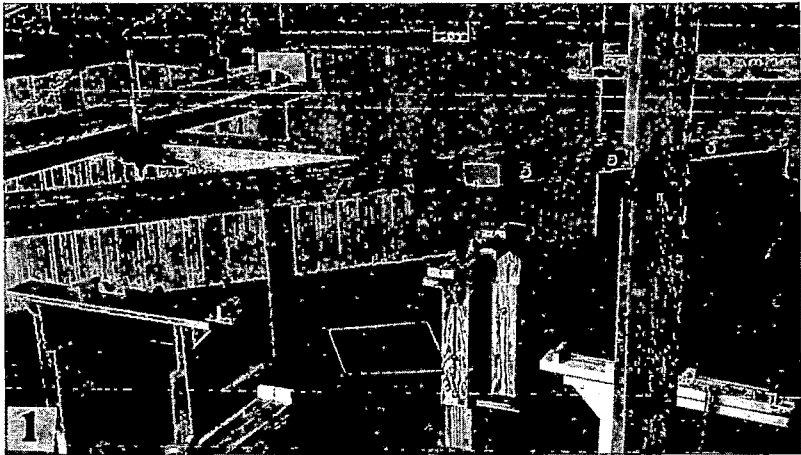


FIG. 1. STATIC ATAXIAMETER

supported by a pulley with a counter-weight (see Fig. 1). The counter-weight is a pail filled with sand. Measurement at the hips is made by apparatus supported on standards. These standards can be raised and lowered.

The heart of the apparatus lies in the rider and pawl shown in Fig. 2. Non-stretch cords passing from the head or from the waist pass through the bottom part of the rider. The pawl permits the rider to move in only one direction when the string is pulled back and forth by the sway of S. Many sizes of riders have been tried and many types of pawls. The most satisfactory at present is the rider shown in Fig. 2, $\frac{3}{8}$ in. thick by $3\frac{1}{4}$ in. long by $1\text{-}9/16$ in. high. The pawl is made of copper or heavy tin, properly weighted with solder, and a piece of rubber is placed at the tip end.

Non-stretch cords extend from the center to the outer ends of the runways hanging over metal pulleys and weighted with small metal washers enough to keep the cords taut.

In order to make the pull as slight as possible and to insure that the rider will never travel backwards, a cord weighted with washers pulls the rider continuously, so that only about 5% of its weight is pulled by S.

A light strap cap holds a piece of wood in which is a lever. The aluminum centerpiece is released by squeezing the lever and wood together with the forefinger and thumb. At the waist the individual is released by squeezing a paper-clip. The aluminum piece is so light that the counter-weights hold the top riders locked. The

¹ A. S. Edwards, New apparatus for the measurement of bodily movement, *J. Exper. Psychol.*, 25, 1939, 125 f.

string at the waist is so light that the counter-weights pull it away from the S's body and lock the lower riders.

All measurements are in millimeters and are read directly, since scales are beside the riders, as shown in the picture.

The pictures show both long and short runways. The long runways accommodate a meter-scale which permits readings up to about 930 mm. The short runways use a scale which permits readings up to 303 mm. The short runways are adequate for nearly all Ss, but long runways were necessary for testing patients in a mental hospital.

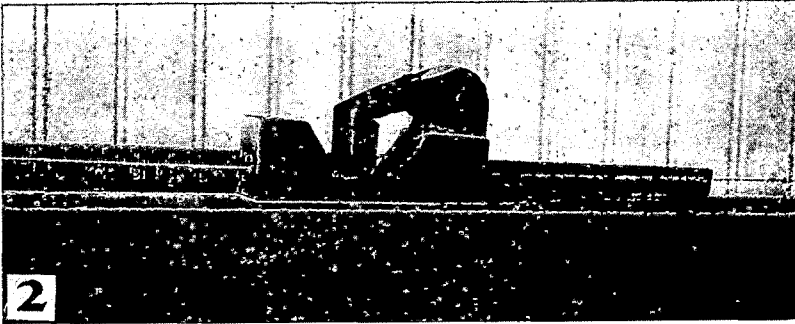


FIG. 2. RIDER AND PAWL

This is the only apparatus known to the writer which permits measurement of static ataxia at the head and waist simultaneously directly in millimeters with four readings each.

Two checks have been made on the accuracy of the apparatus. (1) By carefully pulling the string a distance of 10 mm. ten consecutive times and taking readings. These readings, when the apparatus is carefully adjusted, are accurate to 1%. (2) By inserting a pencil at the aluminum cap and moving it along lines of known length. Records taken on the four scales were of practically the same accuracy.

One point of standardization is especially essential. The points from which the cords emerge from the forward ends of the runways are exactly 15 in. from the center. This distance was arbitrarily chosen, so that, when the sway forward or backward also pulls the cords at the side, the amounts of pull on all riders will be of the same proportional amount. The present apparatus in the writer's laboratory is steadied from both the floor and the ceiling. A movable apparatus has been made that can be knocked down and carried in an automobile. It can be set up, adjusted and checked within an hour, and does not have a counter-weight to help raise and lower the platforms.

Either apparatus can be made inexpensively.

AN ELECTRO-DYNAMIC OSCILLOGRAPH: A TONE-WRITER

By H. T. DIEHL, University of Southern California

The electro-dynamic oscillograph or tone-writer, illustrated in the accompanying drawing, is now being used in the psychological laboratory of the University of Southern California for making kymographic records of bird songs that had previously been recorded phonographically.¹ It replaces a crystal oscillograph formerly

¹ This work is being done under the direction of Dr. Milton Metfessel.

in use. Since it is both efficient and relatively easy to build, it is described here for the consideration of those who might be interested in the construction of similar instruments. It should prove useful to any investigator working in the fields of music

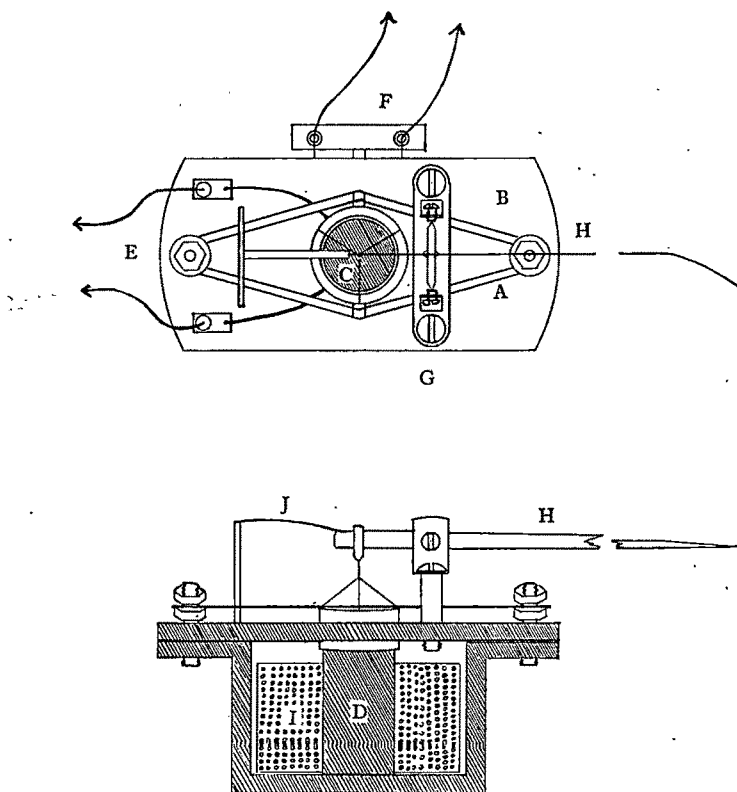


FIG. 1. DIAGRAMS SHOWING CONSTRUCTION OF THE ELECTRO-DYNAMIC OSCILLOGRAPH

- | | |
|--|------------------------------------|
| A = Rhombohedral support | F = Connections to B+ power supply |
| B = Upper housing plate | G = Recording arm assembly |
| C = Voice coil | H = Recording arm |
| D = Central pole | I = Field coil |
| E = Connections to output of amplifier | J = Balance spring |

or speech whose research problems involve the production of kymograms giving a visible presentation of sound patterns.

The oscillograph was constructed by making a few simple changes in a 6-in. Jensen electro-dynamic speaker. First, the speaker cone and assembly were removed. This operation was performed with great care so that the sensitive voice coil might not be injured. It was then necessary to build a framework to support the voice

coil. This was done by cutting a rhombohedral perimeter (A) from a sheet of thin fiber board. The rhombus outline was, of course, measured to fit the upper plate of the field housing (B). It was fastened to the plate's two supporting bolts. Two fiber strips were glued to the corners of the framework and so fixed to the voice coil (C) that it could be suspended about the central electro-magnetic pole (D). After the voice coil was in position, it was made certain to be horizontally stable while free to move vertically. The tinsel wires originally used in the speaker were soldered to the voice coil leads and tied to its suspension frame. These wires were then connected to terminal posts supported by a backing of fiber board placed across the handle end of the upper plate of the housing. Connections (E) run from the terminal posts so that a union may be made with the output of a resistance coupled amplifier using one 6C6 tube, one 6B5 tube, and one 80 rectifier tube. The lead wires (F) from the field coil (I) go to the B plus power supply of the amplifier.

The units of the recording arm assembly (G) were fashioned next. This assembly consists of several parts: the axial shaft, the bridge, the adjusting screws, the supporting bolts, the bolt sleeves, and the recording arm (H). The axial shaft and the adjusting screws were taken from the balance wheel of a Westclox clock. The recording arm was cut from a piece of 16 gage aluminum. It is $5\frac{5}{16}$ in. long, tapering from $\frac{3}{8}$ in. at its receiving end to $\frac{1}{4}$ in. at its writing end. A stylus of spring steel was glued to the recording arm to bring it to an over-all length of $6\frac{5}{16}$ in. The bridge supporting the axial shaft is a piece of brass $1\frac{9}{16}$ in. long and $\frac{1}{4}$ in. wide. Holes were sunk in the upper plate of the housing and two bolts tapped in to extend $1\frac{1}{8}$ in. above the plate. After the bolts were sleeved, the bridge to which the axial shaft and adjustment screws were previously soldered was set in place. The recording arm swings on the axial shaft so that its receiving end projects over the magnet. A bearing was next fitted to the recording arm and connected by means of a pin to the point of intersection of the three brass strips leading diagonally upright from the rim of the voice coil. The motive force is delivered in this wise to the recording arm. A balance spring (J) $\frac{7}{8}$ in. long was soldered to the projecting end of the recording arm and fastened to the upper plate of the housing by means of a curved piece of paper clip wire 3 in. in length. This was done to counterbalance the weight of the aluminum shaft and hold the voice coil in a neutral position. A metal handle 7 in. long and $\frac{3}{8}$ in. in diameter was bolted to a strip of brass wrapped around the rear upright of the housing. This serves as a support by which the tone-writer can be clamped to a stand.

The instrument is activated in the same way as a radio loud-speaker. The phonograph record is placed on a turn-table and played back at about $\frac{1}{6}$ of the recording speed. A "pick-up" delivers the electrical impulses to the amplifier where the waveform is maintained but greatly enlarged. The voice coil of the tone-writer is connected to the output of the amplifier so that the pulsating electrical currents passing through the coil produce a similarly pulsating magnetic field. Since the voice coil is placed in a constant magnetic field with which its own field interacts, it moves in accordance with the exciting electrical current. This movement is transmitted to the recording arm by means of the metal strip. When the stylus is placed against a moving belt of smoked kymographic paper, it makes a kymogram of the impulses coming to it from the record.

APPARATUS NOTES

A COMMUNICABLE METHOD OF RECORDING AREAS IN THE RORSCHACH TEST

The problem of recording the location of an individual's response to the ink-blot in the Rorschach Test has been solved in various ways. Beck blocks off each blot into areas, and assigns a number to each area.¹ These areas, however, are relatively large. They are used in "delimiting the portions of the figures most likely to be encountered."² Thus, unusual interpretations and details cannot be recorded easily with Beck's system. Hertz uses a mimeographed diagram for each blot.³ She too

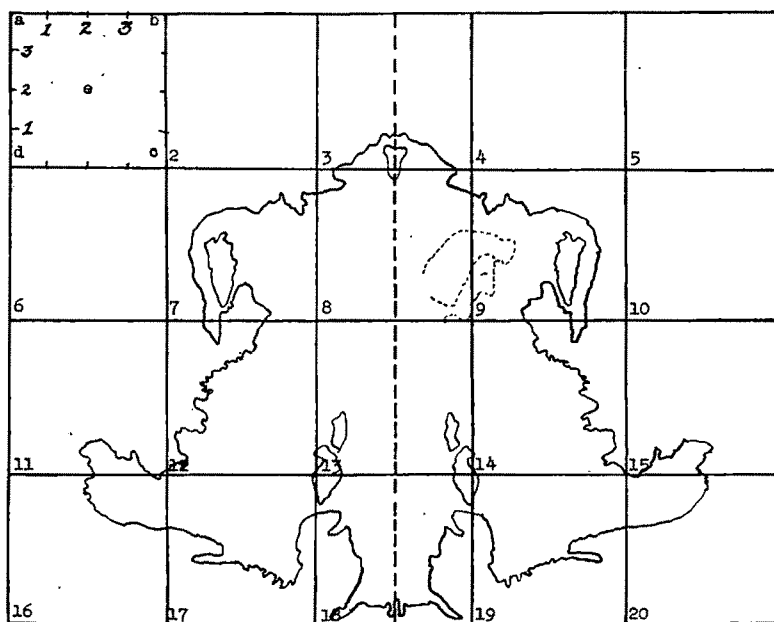


FIG. 1. CELLULOID CHART OF RORSCHACH INK-BLOT IV
(S's response: "A head, bowed, pensive." Localization: "Chin at 8c; nose at 9d-1, top of hair at 9d-2.")

assigns symbols or numbers to the areas most commonly employed in responses, but she records the uncommon responses by tracing them roughly on the diagram. Rickers-Ovsiankina also employs mimeographed diagrams which she uses only to record the more uncommon interpretations.⁴

¹ S. J. Beck, Introduction to the Rorschach method, *Amer. Orthopsychiat. Assn.*, 1937, 205-214.

² *Op. cit.*, 205.

³ M. R. Hertz, The method of administration of the Rorschach ink-blot test, *Child Develop.*, 7, 1936, 237-254.

⁴ M. Rickers-Ovsiankina, *Rorschach Scoring Samples*, 1938.

All these methods are adequate for common responses which scarcely require recording by the experienced Rorschacher. None of them, however, provides a communicable record of the location of rare interpretations. Drawing an outline of the area interpreted serves to preserve its location for the experimenter until he gets ready to classify the responses, but it has certain disadvantages. The outlines cannot be tabulated in a report. Moreover, outlining requires a separate diagram of each blot and perhaps several in the case of a prolific subject.

In order to obviate some of these difficulties for a study in which the subjects were authors and cartoon-animators who promised to be both prolific and productive of rare responses, the writers have devised a new method. The apparatus consists only in a piece of celluloid the size of the cards upon which the blots are mounted. This sheet of celluloid is divided into 20 squares of 1.5 in. by lines drawn with India ink. This division results in 4 horizontal rows of 5 squares each of which are numbered consecutively through the 4 rows from the upper left-hand corner: 1 to 5, 6 to 10, 11 to 15, and 16 to 20. Furthermore, the celluloid piece is bisected with a dotted line which passes vertically through the middle column of squares with number 3, 8, 13, and 18 (see Fig. 1).

When *S* has finished with his interpretations of the last blot, and *E* turns to recording the location of the areas interpreted, he may place this celluloid sheet over each card in turn. The placement of the sheet is rendered precise by adjusting the top of the sheet so that it coincides with the top of the card, and then adjusting it laterally so that the dotted line coincides with the mid-line of the bilaterally symmetrical blot. When the celluloid has been clipped in place, *S* points out the area of the blot involved in each of his various interpretations, and *E* records these areas in code.

The code employed in our method of recording derives principally from the 20 numbered squares which give the first approximation of location. A second degree of refinement is obtained by representing the corners and center of each square with letters, starting with the upper left-hand corner as 'a' and proceeding clockwise around the square through 'b,' 'c,' and 'd,' and the center as 'e.' This is all the refinement necessary to locate most interpretations. Occasionally, however, it is necessary to be very precise about some cardinal detail. A third degree of refinement may be added for such instances by dividing the distances between the consecutive corners of the squares in thirds and representing the points of divisions with the numbers 1, 2, and 3. Thus, *e.g.* 2 would bisect the distance between corners 'a' and 'b,' or any other pair, and 1 would bisect the distance between 2 and 'a' just as 3 would bisect the distance between 2 and 'b' (see Fig. 1). With this third degree of refinement even the location of the smallest and vaguest details may be recorded quite adequately. For instance, an interpretation of the shadows in Blot IV which had been coded by one of the writers (Fleischer) was located readily by the other.

Fig. 1 will serve to illustrate the use of our method of recording the location of responses when they are to be scored. In this instance the response was: "One head bowed, pensive." It was coded by Fleischer as follows, "Chin at 8c. Nose at 9d-1. Top of hair at 9d-2." The other writer placed the celluloid sheet over Blot IV as indicated in Fig. 1, then, using the code, found the constellation within a few seconds. Any one wishing to score a set of Rorschach responses recorded by our method could relocate them easily in this fashion.

The writers are well aware that each Rorschacher likes his own method of recording the location of interpreted parts. We submit this one only as another possibility, which, it seems to us, has certain advantages. It does away with the necessity for a number of mimeographed diagrams of each blot. It serves to locate even the smallest and most obscure area interpreted. It provides, furthermore, a communicable record of the location, a feature which should make it valuable for tabular representation when the work of different investigators is to be compared.

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NOTES AND DISCUSSIONS

RECENT ADVANCES IN STATISTICAL THEORY AND APPLICATIONS

The amount of statistical material published during the last five decades on theory, methodology, and applications of statistics has steadily increased, and the end is no where in sight. Twenty years ago almost all articles on statistics utilized in education and psychology could be found in either the *Journal of Educational Research*, the *Journal of Educational Psychology*, or the *British Journal of Psychology*; but today one must search through many journals in these fields, and one journal (*Psychometrika*) devotes itself exclusively to such articles.

Reviews. This tremendous increase in the literature has made necessary the existence of reviews such as this. There is some evidence that these reviews in the future will have to be limited to applications in a special field of investigation, or even to particular branches of statistics itself, as for example, factor analysis. Two years ago the writer presented a similar review¹ before the Eastern Psychological Association, and although the material was selected from the point of view of the audience, over a hundred articles were cited. Cureton and Dunlap the same year devoted sixteen pages to the developments in statistical methods related to test construction alone.² Every year Swineford and Holzinger prepare an annotated bibliography of selected references on statistics, the theory of test construction, and factor analysis; this bibliography is very valuable for students interested in these fields.³

A similar service has been performed for British psychologists and educators by Burt.⁴ Today, workers in education and psychology have a much clearer realization of the limitations of statistical method. Few investigators believe statistics reveal causation, or that statistics describe or identify the underlying causes, or that having computed extensive and elaborate measures from their data their task is finished. Statistics give concise description, accurate predictions, precise tests of significance, and exact measures of relationship; but the interpretation must rest with the investigator, and thus most data will always be subject to more than one interpretation. Examination of the literature over a period of time reveals that the present trend is to give more consideration to tests of significance including the old but little used method of chi square, the analysis of variance, methods of treating small samples, matrix methods including factor analysis, and finally, consideration of problems of experimental design.

Degrees of freedom. One of the more confusing concepts to the student of elementary statistics is the matter of degrees of freedom. Basically, the concept is simple and the idea is not at all novel to students of formal logic. It is, however, funda-

¹J. W. Dunlap, Recent advances in statistical theory and applications, this JOURNAL, 51, 1938, 558-571.

²E. E. Cureton and J. W. Dunlap, Developments in statistical methods related to test construction, *Rev. Educ. Res.*, 8, 1938, 307-322.

³F. Swineford and K. J. Holzinger, Selected references on statistics, the theory of test construction, and factor analysis, *Sch. Rev.*, 48, 1940, 460-466.

⁴C. Burt, Recent developments of statistical methods in psychology, *Occup. Psychol., Lond.*, 12, 1938, 169-177.

mental to statistics and of basic importance in the interpretation of data. Walker has given an unusually clear and succinct explanation of the idea and many illustrations as to how the number of degrees of freedom may be determined for various situations.⁵

Analysis of variance. Baxter, studying the influence of sensory modality, used only six cases, but set the experiment up in a factorial design.⁶ He demonstrated by the application of analysis of variance that his experimental design was as efficient as the usual single variable experiment using eight times as many subjects. Rubin-Robson working with piano students also used the methods of analysis of variance and factorial design and demonstrated with a small number of cases real and significant differences.⁷ This does not mean that investigators should work with a small number of cases, but only that when by nature of the materials being studied the practical situation limits one to a few observations, methods have been developed for treating the data. Brandt has shown, in experiments where reversal trials occur, *i.e.* where two groups are treated simultaneously but with the order of presentation in one group being the reverse of that in the other, that the method of analysis of variance gives a highly efficient analysis of the data.⁸ This article should be of particular interest to individuals studying problems of learning under different conditions. Dunlap has pointed out a number of applications of the method to problems in education.⁹ One of the most exhaustive treatments of the subject, with particular emphasis on problems of education is to be found in Lindquist's new text.¹⁰ The problem of testing the difference between variance ratios has been solved by Jellinek in terms of analysis of variance and the intra-class correlation coefficient.¹¹

Experimental design. In the foregoing comments the general problem of experimental design has been implied, and this topic has been specifically studied recently by several workers. As most investigators know, this branch of statistics has received most attention by workers in agriculture and biology as the result of the early and stimulating work of R. A. Fisher. Shen recently gave a very pertinent discussion of various methods of control, indirect, direct, statistical and experimental, and pointed out various experimental designs and appropriate statistics for each.¹² He considers particularly applications to problems of education, and has pointed out certain pitfalls for the unwary.

⁵ H. M. Walker, Degrees of freedom, *J. Educ. Psychol.*, 31, 1940, 253-269.

⁶ B. Baxter, An application of factorial design to a psychological problem, *Psychol. Bull.* 37, 1940, 421.

⁷ Grace Rubin-Rabson, Studies in the psychology of memorizing piano music, *J. Educ. Psychol.*, 30, 1939, 321-345.

⁸ A. E. Brandt, Tests of significance in reversal or switchback trials, *Res. Bull. Ia. Agric. Exper. Sta.*, 1938, (no. 234), 60-87.

⁹ J. W. Dunlap, Applications of analysis of variance to educational problems, *J. Educ. Res.*, 33, 1940, 434-442.

¹⁰ E. F. Lindquist, *Statistical Analysis in Educational Research*, 1940, 1-266.

¹¹ E. M. Jellinek, On the use of the intra-class correlation coefficient in the testing of the difference of certain variance ratios, *J. Educ. Psychol.*, 31, 1940, 60-63.

¹² E. Shen, Experimental design and statistical treatment in educational research, *J. Exper. Educ.*, 8, 1940, 346-353.

In a review of the arguments concerning the relative efficacy of arrangements generated by the randomizing process as compared to systematic arrangements in designing experiments, Yates concludes that, although systematic arrangements may in certain cases give decidedly greater accuracy than methods of randomization, nevertheless, the evidence favors the latter, since these methods are likely to give similar gains in accuracy and are more satisfactory from a statistical point of view.¹³ Jeffreys, a seismologist, points out that in certain cases randomness might obscure the true value one is seeking, and cautions workers against allowing statistical theorizing to supplant systematic design in experimentation.¹⁴

The traditional principle of experimental design has been to allow only one independent variable, but the work of Fisher has made it possible to study simultaneously more than one independent variable. Crutchfield and Tolman point out the value of multiple-variable designs in studying such complex interactions as exist in data concerning behavior.¹⁵ Such designs in their opinion not only are more economical, but provide a broader inductive basis for generalization of the results.

One of the difficulties in using quasi-factorial designs such as randomized blocks, balanced incomplete blocks, etc., is the fact that data are often missing from one or more of the blocks. Cornish has provided formulas for estimating the missing values and also tests of significance for these values.¹⁶ All who are interested in efficient experimental designs should see Fisher's "The Design of Experiments."¹⁷

Practical problems in sampling. For years it has been the practice for authors of elementary texts in statistics to point out the necessity for a random sample, if the conclusions from the observed data are to be applied generally. Unfortunately, however, they usually let the matter rest with that warning! A few writers in periodical literature have treated the question of how to secure an adequate sample, but it is only recently that one can observe general interest in the problem. This attention to the practical aspects of the problem is probably due in part to the efforts of the Gallup Poll, the Institute of Public Opinion, and the magazine *Fortune* to secure adequate evaluations of public opinion. Snedecor treats with the design of sampling experiments in the social sciences, indicating the more promising types of sampling, and gives a brief history of the various methods.¹⁸

Two empirical studies as to the effect of follow-up returns on questionnaires have recently been reported. Lindman in a study of follow-up returns for a group of high school graduates found that errors of sampling due to partial returns did not markedly distort the results.¹⁹ These results are contrary to the findings of Shuttle-

¹³ F. Yates, The comparative advantages of systematic and randomized arrangements in the design of agricultural and biological experiments, *Biometrika*, 30, 1939, 440-466.

¹⁴ H. Jeffreys, Random and systematic arrangements, *Biometrika*, 31, 1939, 1-8.

¹⁵ R. S. Crutchfield and E. C. Tolman, Multiple-variance design for experiments involving interaction of behavior, *Psychol. Rev.*, 47, 1940, 38-42.

¹⁶ E. A. Cornish, The estimation of missing values in quasi-factorial designs, *Ann. Eugen. Camb.*, 10, 1940, 59-68.

¹⁷ R. A. Fisher, *The Design of Experiments*, 1935, 1-251.

¹⁸ G. W. Snedecor, Design of sampling experiments in the social sciences, *J. Farm Econ.*, 21, 1939, 846-855.

¹⁹ E. L. Lindman, The adequacy of follow-up samplings, *Occupations*, 19, 1940, 33-35.

worth and other earlier investigators. Shuttleworth found that only 0.5% of the first 184 replies from alumnae were from unemployed individuals, whereas 5.8% of the next 121 individuals replying were unemployed.²⁰ This substantiates the hypothesis that individuals replying to questionnaires may well exhibit bias. It is just such a factor as this that probably accounts for the shocking failure of the *Literary Digest* election polls. Stephan has also analyzed different types of selective sampling procedures and discusses errors that should be avoided.²¹ Certain of these methods should be of especial value in making surveys of large city or state school systems. The problem of determining what may be accepted as critical differences between different percentages in such polls has been treated by Wilks in an article dealing with theoretical considerations, which also provides methods of calculation.²²

When samples are being drawn from masses of data already available such as census records, school records, and the like, bias may occur by chance. Vickery has developed a method in which punched cards and tabulating equipment are used to correct such bias.²³

Theoretical problems in sampling. Theoretical problems of sampling of populations have not been ignored by mathematical statisticians, as evidenced by the work of Neyman and others. Neyman suggests that in those cases where it is very difficult or expensive to obtain direct measurements on some trait, measurements be first secured on some correlated trait which is much less expensive per unit to secure.²⁴ This large sample could then be sorted into strata on the basis of the desired trait, and a small sample of measurements obtained for each stratum. He has developed the necessary formulas for this work and points out the conditions under which the method will yield a maximum amount of information at a minimum of total expense. Muench has dealt with the problem of analyzing a frequency distribution arising from the combination of two or more binomial or Poisson distributions and provides methods of resolving the distribution into its component parts.²⁵ This technique should have considerable value in analyzing items of personality blanks, interest inventories, etc., in determining if all the items are from the same basic distribution. In a sense it might be used as a check on the current methods of factor analysis in such problems. Boissevain has also dealt with the problem of the distribution of abilities depending upon two or more independent factors, and shows how his method might be used to interpret, say, distributions of income or of contributions to scientific journals.²⁶ Craig, in a highly technical paper, has enumerated many of

²⁰ F. K. Shuttleworth, Sampling errors involved in incomplete returns to mail questionnaires, *Psychol. Bull.*, 37, 1940, 437.

²¹ F. S. Stephan, Representative sampling in large-scale surveys, *J. Amer. Statist. Assoc.*, 34, 1939, 343-352.

²² S. S. Wilks, Confidence limits and critical differences between percentages, *Publ. Opin. Quarterly*, 4, 1940, 332-338.

²³ C. W. Vickery, Punched card technique for the correction of bias in sampling, *J. Amer. Statist. Assoc.*, 33, 1938, 552-556.

²⁴ J. Neyman, Contribution to the theory of sampling human populations, *J. Amer. Statist. Assoc.*, 33, 1938, 101-116.

²⁵ H. Muench, Discrete frequency distributions arising from mixtures of several single probability values, *J. Amer. Statist. Assoc.*, 33, 1938, 390-398.

²⁶ C. H. Boissevain, Distribution of abilities depending upon two or more independent factors, *Metron*, 13, 1939, (no. 4), 49-58.

the theorems and methods of mathematical statistics which serve useful purposes in the estimation of stratified random sampling.²⁷ Another matter, closely related to this, that has received a great deal of attention on the part of the mathematical statistician is the problem of tests of significance.

Tests of significance. Smith has prepared a monograph on various tests of significance and what they mean.²⁸ This work should prove useful to the non-mathematician.

One of the most useful tests which is only now coming into widespread recognition is Pearson's chi square, and considerable attention is being given to its advantages and limitations. Berkson argues that chi square cannot be used routinely and uncritically as have many of our other tests.²⁹ He cites certain hypothetical instances and also certain real instances in which he believes the blind acceptance of chi square would lead to completely erroneous conclusions. Camp questions certain of Berkson's conclusions, and states that there are always a number of criteria for the acceptance or rejection of an hypothesis.³⁰

Closely related to chi square is Neyman's psi square, which was developed for situations in which all the deviations from the hypothetical distribution are of the same sign. David demonstrates that there is no good rule for determining when psi square should be applied instead of chi square.³¹ This lack opens up the need for more theoretical work and also for empirical investigations of the point.

Deming comments on the fact that chi square is useful only when the points are subject to error and when the nature of the true curve is not known.³² Fry in an elementary discussion derives chi square and emphasizes the basic questions which can and cannot be answered by the chi square test.³³ Greenwood has also given a detailed proof of the chi square test of goodness of fit in a monograph of some seventy pages.³⁴

The use of chi square involves an error due to certain approximations in its derivation. Hoel has studied the magnitude of this error and concludes that for most work the present methods of evaluating chi square are entirely satisfactory.³⁵

²⁷ A. T. Craig, On the mathematics of the representative method of sampling, *Ann. Math. Statist.*, 10, 1939, 26-34.

²⁸ J. H. Smith, Tests of significance, what they mean and how to use them, *Stud. Bus. Adm., Univ. Chicago*, 10, 1939, (no. 1), 1-99.

²⁹ J. Berkson, Some difficulties of interpretation encountered in the application of the Chi-square test, *J. Amer. Statist. Assoc.*, 33, 1938, 526-536; A note on the Chi-square test, the Poisson and the Binomial, *J. Amer. Statist. Assoc.*, 35, 1940, 362-367.

³⁰ B. H. Camp, Further interpretations of the Chi-square test, *J. Amer. Statist. Assoc.*, 33, 1938, 537-542; Further comments on Berkson's problem, *J. Amer. Statist. Assoc.*, 35, 1940, 368-376.

³¹ F. N. David, On Neyman's "smooth" test for goodness of fit, *Biometrika*, 31, 1939, 191-199.

³² W. E. Deming, Some thoughts on curve fitting and the Chi-test, *J. Amer. Statist. Assoc.*, 33, 1938, 543-551.

³³ T. C. Fry, The χ^2 test of significance, *J. Amer. Statist. Assoc.*, 33, 1938, 513-525.

³⁴ E. R. Greenwood, Jr., *Detailed Proof of the Chi-square Test of Goodness of Fit*, 1940, 1-73.

³⁵ P. G. Hoel, On the Chi-square distribution of small samples, *Ann. Math. Statist.*, 9, 1938, 158-165.

The use of chi square for testing non-normal material has been investigated by Welch.³⁶ He proposes that the squared correlation ratio be used in such situations.

David has demonstrated that the distribution of the means of samples drawn without replacement from a finite population tends to be normal.³⁷ Newman has shown that if a number of samples are available, it is possible to obtain from the several ranges an estimate of the standard deviation which is only slightly less accurate than that obtained from the sums of squares.³⁸ This result is due to the fact that a high correlation exists between the range and the standard deviation.

Finney in an earlier paper has discussed the distribution of the ratio of estimates of the two variances and has shown how a test for significance could be applied when the population correlation is known. Pitman³⁹ and Morgan⁴⁰ have investigated the same problem by different means and have determined exact tests of significance. Chapman has shown that the "z" test is applicable to skewed as well as to normal distributions.⁴¹

The customary procedure when a number of mean values have been obtained in experimental work, as for example when several methods of teaching are being compared, has been to calculate independently the standard error of each mean and thus to obtain a different standard error for each comparison. Fisher assumes the null hypothesis, *i.e.* that all methods are equivalent, and consequently that pooling the errors of estimate is permissible, to determine one overall test of significance.⁴² He also treats the case of unequal variances, as has Starkey.⁴³

Considerable work has been done recently on the problem of transforming the form of a statistic so as to make it more amenable for developing a test of significance. Those interested in this problem might well examine articles by Pearson,⁴⁴ Hotelling and Frankel,⁴⁵ and Kendall.⁴⁶

Probability. Students interested in the theoretical, logical, philosophical, and mathematical implications of probability might well examine articles by Goodstein,⁴⁷

³⁶ B. L. Welch, On tests for homogeneity, *Biometrika*, 30, 1938, 149-158.

³⁷ F. N. David, Limiting distributions connected with certain methods of sampling human populations, *Statist. Res. Mem.*, 2, 1938, 69-90.

³⁸ D. Newman, The distribution of range in samples from a normal population expressed in terms of an independent estimate of standard deviation, *Biometrika*, 31, 1939, 20-30.

³⁹ E. J. G. Pitman, A note on normal correlation, *Biometrika*, 31, 1939, 9-12.

⁴⁰ W. A. Morgan, A test for the significance of the difference between the two variances in a sample from a normal bivariate population, *Biometrika*, 31, 1939, 13-19.

⁴¹ R. A. Chapman, Applicability of the z test to a Poisson distribution, *Biometrika*, 30, 1939, 188-190.

⁴² R. A. Fisher, *The Design of Experiments*, 1935, 1-251.

⁴³ D. M. Starkey, A test of the significance of the difference between means of samples from 2 normal populations without assuming equal variances, *Ann. Math. Statist.*, 9, 1938, 201-213.

⁴⁴ E. S. Pearson, The probability integral transformation for testing goodness of fit and combining independent tests of significance, *Biometrika*, 30, 1938, 134-148.

⁴⁵ H. Hotelling and L. R. Frankel, The transformation of statistics to simplify their distribution, *Ann. Math. Statist.*, 9, 1938, 87-96.

⁴⁶ M. G. Kendall, Proof of Fisher's rules for ascertaining the sampling semi-invariants of k-statistics, *Ann. Eugen. Camb.*, 10, 1940, 215-222.

⁴⁷ R. L. Goodstein, On Von Mises' theory of probability, *Mind*, 49, 1940, 58-62.

Nagel,⁴⁸ White,⁴⁹ and Wright.⁵⁰ Also of interest will be Jeffrey's⁵¹ and Mises'⁵² books on probability.

Causation. The problem of causation seems to be losing its charm as only one published article has dealt with the subject during the past two years. This article was by Mendershausen.⁵³ He confines himself to the analysis of causal systems underlying the variation of a single dependent variate. It is assumed that this dependent variate can be completely accounted for by a set of postulated uncorrelated "basis variates." The basis variates cannot be measured directly. The independent variates are linear combinations of these basis variates and also of other factors not correlated with the dependent variates. Here is a stimulating study which deserves some consideration in connection with current methods of factor analysis.

Correlation. Thorndike in a recent article has pointed out that if the correlation between two traits has a certain value, this value cannot necessarily be imputed to the subgroups.⁵⁴ This is a pitfall into which the unwary occasionally fall. Thorndike demonstrates the principle with 12 samples and also by graphic means.

Another simple graphical method is offered by Peatman for illustrating the effectiveness for prediction of correlations of various magnitudes.⁵⁵ Teachers who have difficulty with this point might well try this method.

The effect of homogeneity in one variable has been investigated by Casanova and he has derived formulas for estimating the correlation that would have obtained for a total group if both variables had been heterogeneous.⁵⁶ This is closely related to Kelley's formula for estimating the correlation in one range when the correlation is known in another range.

Thouless in a recent article emphasizes the fact that correction for attenuation is necessary only when one wants to know the absolute value of the correlation coefficient between two tested functions; and that the use of such a correction is wrong if what is desired is the correlation between the test scores.⁵⁷ Workers might well read this article before correcting coefficients for attenuation.

The problem of significance of a covariance has been solved by Greenwood⁵⁸ and

⁴⁸ E. Nagel, Probability and the theory of knowledge, *Philos. Sci.*, 6, 1939, 212-253.

⁴⁹ M. G. White, Probability and confirmation, *J. Philos.*, 36, 1939, 323-328.

⁵⁰ G. H. v. Wright, On probability, *Mind*, 49, 1940, 265-283.

⁵¹ H. Jeffreys, *Theory of Probability*, 1940, 1-380.

⁵² R. von Mises, *Probability, Statistics, and Truth*, 1939, 1-323.

⁵³ H. Mendershausen, Clearing variates in confluence analysis, *J. Amer. Statist. Assoc.*, 34, 1939, 93-105.

⁵⁴ E. L. Thorndike, On the fallacy of imputing the correlations found for groups to the individuals or smaller groups composing them, this JOURNAL, 52, 1939, 122-124.

⁵⁵ J. G. Peatman, On the predictive meaning of correlation, *J. Gen. Psychol.*, 22, 1940, 17-23.

⁵⁶ T. Casanova, Corrections to correlation coefficients on account of homogeneity in one variable, *J. Exper. Educ.*, 8, 1940, 341-345.

⁵⁷ R. H. Thouless, The effects of errors of measurement on correlation coefficients, *Brit. J. Psychol.*, 29, 1939, 383-403.

⁵⁸ J. A. Greenwood, A covariation statistic, *J. Parapsychol.*, 3, 1939, 163-166.

workers who prefer this statistic to the correlation coefficient are indebted to him for this solution.

The problem of multiple correlation has had the consideration of three workers. Dwyer⁵⁹ has developed a method by which one can determine r variables out of a pool of n , so that each of the remaining $n-r$ variables may be predicted almost as well from the r variables as it might be from all the $n-1$ variables. Wherry⁶⁰ has provided an approximation method for obtaining a maximized multiple criterion based on the formula developed by Edgerton and Holbe. Wherry compares his method with that of Horst and with that of Hotelling. He concludes that his method is simpler and equally effective. Fisher,⁶¹ in a theoretical and technical paper, has summarized the relationships between three independent lines of research on the treatment of multiple measurements.

The problem of measuring the relationship of ranked data has also been examined. Wallis has proposed a measure analogous to the correlation ratio, based on an extension of Friedman's early work on the method of analysis of variance applied to ranked data.⁶² Wallis's method, however, is identical with earlier work of Kendall and Smith. Kendall has derived a new measure of rank correlation together with its exact sampling error.⁶³ Basically this method is the psychophysical method of paired comparisons and seems to merit consideration. Kendall, Kendall, and Smith⁶⁴ have determined the sampling distribution of Spearman's coefficient, ρ . A comparison of Spearman's ρ and Kendall's coefficient seems to indicate that the latter is to be preferred.

The matter of reliability of chance halves has been investigated experimentally by Read who found substantial agreement between the various methods of selecting the halves.⁶⁵ Richardson and Kuder have proposed a new method for determining test reliability based on the method of rational equivalence.⁶⁶ The reliability is a function of the standard deviation of the distribution of the scores on the test and of 'p,' the proportion of correct answers to a test item. The advantage of this formula lies in certain theoretical considerations and in the speed with which it can be solved.

Thomson has developed a method for estimating the reliability of an entire test

⁵⁹ P. S. Dwyer, The contribution of an orthogonal multiple-factor solution to multiple correlation, *Psychometrika*, 4, 1939, 163-171.

⁶⁰ R. J. Wherry, An approximation method for obtaining a maximized multiple criterion, *Psychometrika*, 5, 1940, 109-115.

⁶¹ R. A. Fisher, The statistical utilization of multiple measurements, *Ann. Eugen. Camb.*, 8, 1938, 376-386.

⁶² W. A. Wallis, The correlation ratio for ranked data, *J. Amer. Statist. Assoc.*, 34, 1939, 533-538.

⁶³ M. G. Kendall, A new measure of rank correlation, *Biometrika*, 30, 1938, 81-93.

⁶⁴ M. G. Kendall, S. Kendall, and B. B. Smith, The distribution of Spearman's coefficient of rank correlation in a universe in which all rankings occur an equal number of times, *Biometrika*, 30, 1938, 251-273.

⁶⁵ C. B. Read, A note on reliability by the chance halves method, *J. Educ. Psychol.*, 30, 1939, 703-704.

⁶⁶ M. W. Richardson and G. F. Kuder, The calculation of test reliability coefficients based on the method of rational equivalence, *J. Educ. Psychol.*, 30, 1939, 681-687.

from the reliabilities of the single tests and their weights.⁶⁷ He shows that the prediction weights and reliability weights sometimes conflict, so that one may be forced to make a choice between validity and reliability. Those constructing tests will be well advised to examine these last two studies.

Prediction. Closely related to the problem of multiple correlation is that of multiple regression. Eisenhart presents a very clear exposition of certain concepts involved in prediction.⁶⁸ He makes it quite apparent why it is necessary in some instances when estimating X from a knowledge of Y, that one should first use Y as the dependent variable and then use the inverse of the relation found. He also treats the question of the effect of range on the accuracy of prediction. This article is worth studying, particularly by those working with data such as mental growth curves, where the slope is not constant over the entire range. Guttman has developed formulas for determining multiple and partial correlations in terms of factors isolated from a group of tests.⁶⁹ This should be of value in studies in which prediction as well as resolution into basic components is a problem.

Factor analysis. The history of statistics seems to be one of fads. A new technique is developed and usually lies unnoticed for a time. Then, suddenly it seems to catch the attention of workers, and there follows a flood of material dealing with applications, modifications, simplifications, and extensions of the technique. This is the position at present of factor analysis and while the method undoubtedly has great value, it is questionable whether its worth is commensurate with the current volume of literature on the subject. A number of writers have attempted to appraise the value of factor analysis. Burt has compared the sampling theory with the two-factor theory, and has also examined the question of reciprocity between tests and persons.⁷⁰ He concluded that so long as the factors of Spearman and Thomson are regarded as descriptive and not causal elements, there is no serious conflict between their theories. Essentially, factor analysis is a method of classification. The principle of reciprocity referred to above states that non-general factors obtained by correlating persons are the same as those obtained by correlating tests.

McCloy, Metheny, and Knott,⁷¹ in a comparison of the Thurstone and Hotelling methods, conclude that both methods require rotation, and that Thurstone's method is simpler and requires less time. Hedman⁷² compared Thurstone's method with that of Spearman and came to the conclusion that the basic principles are identical. A criticism is leveled against the practice of taking residuals and treating them as correlations, but this holds against both methods.

⁶⁷ G. H. Thomson, Weighting for battery reliability and prediction, *Brit. J. Psychol.*, 30, 1940, 357-365.

⁶⁸ C. Eisenhart, The interpretation of certain regression methods and their use in biological and industrial research, *Ann. Math. Statist.*, 10, 1939, 162-186.

⁶⁹ L. Guttman, Multiple rectilinear prediction and the resolution into components, *Psychometrika*, 5, 1940, 75-99.

⁷⁰ C. Burt, The factorial analysis of ability: III. Lines of possible reconciliation, *Brit. J. Psychol.*, 30, 1939, 84-93.

⁷¹ C. H. McCloy, E. Metheny, and V. Knott, A comparison of the Thurstone method of multiple factors with the Hotelling method of principal components, *Psychometrika*, 3, 1938, 61-67.

⁷² H. B. Hedman, A critical comparison between the solutions of the factor problem offered by Spearman and Thurstone, *J. Educ. Psychol.*, 29, 1938, 671-685.

Thomson, in a series of articles, has considered recent developments in statistical method with particular emphasis on factor analysis.⁷³ He maintains that Thurstone's method (and by implication Hotelling's method) is beautiful from the mathematical standpoint, but has little to commend it in reality. In the second article Thomson sets out the main principles of the several factor systems, and indicates where they are incompatible. Various principles have been set up for selecting one method as the best. Among the principles are the following: (1) the factors must represent the experimental facts; (2) the maximum amount of the variance should be reproduced; (3) the correlations should be reproduced by a minimum number of factors; (4) there must be a single general factor 'g'; (5) the factors should be rotated until they become psychologically significant; (6) the factors should exhibit simple structure; (7) there should be an invariance of analysis of the same test in different batteries, when used on equivalent samples of persons; and (8) factor loadings should be used which are reciprocal for persons and tests. The statistician is interested essentially in reproducing the observed variance, while the psychologist's primary concern is to find meaningful entities.

Spearman's and Holzinger's methods basically are the same, although the latter's method is the more powerful. The essential difference between their techniques and that of Thurstone lies in the fact that Thurstone's simple structure is marked by the absence of negative saturations and has a maximum of zero saturations.

Spearman,⁷⁴ in two articles, compares his method with that of Thurstone and points out that Thurstone's method has eliminated the general factor. Both methods create hypothetical entities but Spearman believes that his technique gives a better fit to the observed correlations than does Thurstone's. Spearman claims, further, that his method is simpler statistically, and psychologically is more meaningful. He states that Thurstone's method "leads to the untenable revival of the ancient doctrine of 'faculties'." Stephenson⁷⁵ has come to Spearman's defense with the statement that the general factor theory is the only one in which some reference is made to the characteristics of the tests used, and thus has some connection with reality. To a degree this is true, but one might equally well accuse Spearman and his followers of arguing *a posteriori*.

Thurstone is not unaware of the criticism leveled against his techniques and has vigorously defended his method in a recent article.⁷⁶ He believes that factor analysis is a tool to be used to discover the principal categories of mentality in order that these may be subjected to laboratory investigation. That this is not an academic discussion is evidenced by his experimental work on the perceptual factor. At the

⁷³ G. Thomson, Recent developments of statistical methods on psychology: II, *Occup. Psychol., Lond.*, 12, 1938, 319-325; The factorial analysis of ability: I. The present position and the problems confronting us, *Brit. J. Psychol.*, 29, 1939, 71-77; The factorial analysis of ability. Agreement and disagreement in factorial analysis: A summing up, *Brit. J. Psychol.*, 30, 1939, 105-108.

⁷⁴ C. Spearman, Thurstone's work re-worked, *J. Educ. Psychol.*, 30, 1939, 1-16; The factorial analysis of ability: II. Determination of factors, *Brit. J. Psychol.*, 30, 1939, 78-83.

⁷⁵ W. Stephenson, The factorial analysis of ability: IV. Abilities defined as non-fractional factors, *Brit. J. Psychol.*, 30, 1939, 94-104.

⁷⁶ L. L. Thurstone, Current issues in factor analysis, *Psychol. Bull.*, 37, 1940, 189-236.

same time he points out applications in other sciences, and the use of factorial analysis in the appraisal of individual subjects.

Wilson and Worcester⁷⁷ claim to have demonstrated that the Hotelling method does not yield meaningful traits and cite an example of gas mixtures as proof of their contention. They go on to demonstrate by an example from geometry that the results of Hotelling's analysis are necessarily relative to the population at hand. Other evidence on this point will be reviewed later. Since Hotelling's and Thurstone's methods can be related by rigorous equations it would seem to the writer that Wilson and Worcester's objections could equally well be leveled against Thurstone's technique. Kelley,⁷⁸ in defense of Hotelling, maintains that the criticisms of Wilson and Worcester are irrelevant.

Let us now turn to some of the applications to practical problems and certain methodological developments. Cox set up certain variables experimentally and determined their correlations and factor loadings.⁷⁹ By means of these loadings he was able to reproduce rather closely the theoretical and sample correlations with which he started. Mosier studied the effect of chance error on simple structure and as a result of an empirical investigation concluded that in an experimental situation the use of inaccurate coefficients and estimated communalities still permits accurate determination of the primary trait loadings, provided that the rank of the centroid matrix is equal to or greater than that of the underlying primary trait matrix.⁸⁰ He also considered various methods for determining the completeness of factorization but found none that was wholly satisfactory.

Hoel also considered this last problem and he has developed a test for determining the minimum rank.⁸¹ Young deals with the same problem by developing an index of clustering.⁸² His function is simple and has the advantage of allowing one to get a visual picture of the tendency to cluster.

The mathematical problems involved in orthogonal rotation have been examined by Tucker,⁸³ Landahl,⁸⁴ Lederman,⁸⁵ and Thurstone⁸⁶ with the result that more efficient methods are available for rotating axes.

⁷⁷ E. B. Wilson and J. Worcester, Note on factor analysis, *Psychometrika*, 4, 1939, 133-148.

⁷⁸ T. L. Kelley, Comment on Wilson and Worcester's "Note on Factor Analysis," *Psychometrika*, 5, 1940, 117-120.

⁷⁹ G. M. Cox, The multiple factor theory in terms of common elements, *Psychometrika*, 4, 1939, 59-68.

⁸⁰ C. I. Mosier, Influence of chance error on simple structure: An empirical investigation of the effect of chance error and estimated communalities of simple structure in factorial analysis, *Psychometrika*, 4, 1939, 33-44.

⁸¹ P. G. Hoel, A significant test for minimum rank in factor analysis, *Psychometrika*, 4, 1939, 245-253.

⁸² G. Young, Factor analysis and the index of clustering, *Psychometrika*, 4, 1939, 201-208.

⁸³ L. R. Tucker, A method for finding the inverse of a matrix, *Psychometrika*, 3, 1938, 189-197.

⁸⁴ H. D. Landahl, Centroid orthogonal transformations, *Psychometrika*, 3, 1938, 219-223.

⁸⁵ W. Lederman, The orthogonal transformations of a factional matrix into itself, *Psychometrika*, 3, 1939, 181-187.

⁸⁶ L. L. Thurstone, A new rotational method in factor analysis, *Psychometrika*, 3, 1938, 199-218.

Harman has demonstrated, if the appropriate estimates of bi-factors are put into equations linking the several systems of multiple factors with the bi-factor system, that values of the multi-factors are obtained that are very much like the complete approximation in each case.⁸⁷ It may well be that in the future an initial analysis will be made in terms of bi-factors and that these values will be substituted into the proper equations to give any desired system of factorization. Another shortcut is one developed by Lederman for finding the regression values for estimating the regression of the scores on mental factors.⁸⁸

If factor analysis is to have other than esoteric values for those who determine them, it is necessary to show that the loadings determined for any test are constant from battery to battery and from test situation to test situation. It is for this purpose that Thurstone developed his "simple structure." The final test, however, must rest on experimental proof. Smart, in an early study of this problem concluded that such stability was absent, at least in the small correlational matrices he employed. Humphreys⁸⁹ reworked Smart's data and concluded the factors were stable, and that in those cases where erratic behavior was noted, it could be explained. Holzinger and Swineford secured 26 measures on each of 300 seventh and eighth grade students in two schools.⁹⁰ In one school most of the parents were foreign born, in the other they were chiefly American born. They concluded after applying bi-factor analysis to both groups, that "generally speaking the test material determines what factors may be measured by the tests and that the nature of the group tested determines in a large measure the actual size of the factor weights." The implications of this study, if their findings can be duplicated, are obvious for workers in race differences.

Blakely⁹¹ re-analyzed the data of Brown and Stephenson by means of Thurstone's method. He found the data could be explained by three group factors, and concluded that Thurstone's method was equally efficient as the tetrad technique employed by Brown and Stephenson.

The problem of comparing the results of two different factor studies, provided they have tests in common, and of testing the stability of simple structure has been treated by Mosier,⁹² Dwyer⁹³ and Harman.⁹⁴ The techniques they developed should have wide usefulness in any attempt to purify batteries of tests. It is now possible to add a new test to a battery without having to repeat the laborious factor analysis, yet to determine the effect of the new test on the factor loadings.

⁸⁷ H. H. Harman, Systems of regression equations for the estimation of factors, *J. Educ. Psychol.*, 29, 1938, 431-441.

⁸⁸ W. Lederman, On a shortened method of estimation of mental factors by regression, *Psychometrika*, 4, 1939, 109-116.

⁸⁹ L. G. Humphreys, The stability in pattern of factor loadings: A comment on Dr. Smart's conclusions, *J. Educ. Psychol.*, 30, 1939, 231-237.

⁹⁰ K. J. Holzinger and F. Swineford, A study in factor analysis: The stability of a bi-factor solution, *Suppl. Educ. Monog.*, 1939, (no. 48), 1-91.

⁹¹ R. Blakely, A re-analysis of a test of the theory of two factors, *Psychometrika*, 5, 1940, 121-136.

⁹² C. I. Mosier, Determining a simple structure when loadings for certain tests are known, *Psychometrika*, 4, 1939, 149-162.

⁹³ P. S. Dwyer, The determination of the factor loadings of a given test from the known factor loadings of other tests, *Psychometrika*, 2, 1937, 173-188.

⁹⁴ H. H. Harman, Extensions of factorial solutions, *Psychometrika*, 3, 1938, 75-84.

That this problem has not yet reached a final solution is emphasized by an article of Thomson and Ledermann.⁹⁵ They maintain that "new factors" can be created or old ones destroyed, but these changes in the number of factors are only among selected variates, and the remaining variates can still be analyzed into the old number of factors, although with new loadings." Young and Householder⁹⁶ claim that the requirement of invariance imposed by Thurstone's simple structure removes the indeterminacy in factor determination and there is, therefore, a basis for integrating various factorial studies. They also discuss the problem of selection of significant primary factors, but in the writer's opinion they have not given an infallible method for assigning meaning to the factors. Closely related to these studies is that of Crissy in which he shows empirically the invariant nature of factor loadings.⁹⁷ More important are his findings regarding the effect of partial correlation on the loadings. Thurstone's method seems less likely to be affected by partialling than the Kelley method, and is also likely to yield weights that are more invariant than weights yielded by Kelley's solution. It is apparent from the above comment that more work is needed before it can be stated finally what the effect of shifting a test or changing populations will have on the factor loadings.

The matter of correlations between persons and the analysis of matrices of such correlations has been mentioned above. Burt has applied his method,⁹⁸ Holzinger's method, and that of Alexander to the same set of data. The empirical results of the three methods agreed more closely for the correlations between persons than for those between tests. The general theory underlying the factorization of correlations between persons has been discussed by Burt and Stephenson in a joint article.⁹⁹ The statistical procedures of these two are comparable, but they disagree in their interpretation of the psychological implications of the results. The chief point of disagreement is that of the reciprocity principle mentioned above.

Another attempt to bring order to the present method of factoring correlations between persons is the article by Davies.¹⁰⁰ An important point brought out by Davies is that there is little reason to believe that any person would have a measurement of precisely zero for any particular factor. If this hypothesis is granted there seems to be no need to raise the question of advisability of rotating axes to obtain simple structure.

Ferguson administered three forms of the same test and split them into odd-even halves.¹⁰¹ He then determined the correlations and applied the bi-factor technique to the resulting matrix. He found a factor which he calls "temporal contiguity."

⁹⁵ G. H. Thomson and W. Ledermann, The influence of multivariate selection on the factorial analysis of ability, *Brit. J. Psychol.*, 29, 1939, 288-306.

⁹⁶ G. Young and A. S. Householder, Factorial invariance and significance, *Psychometrika*, 5, 1940, 47-56.

⁹⁷ W. J. E. Crissy, The effect of partialling on two factor methods: Thurstone and Kelley (modified), *Psychol. Rec.*, 3, 1939, 138-144.

⁹⁸ C. Burt, Factor analysis by sub-matrices, *J. Psychol.*, 6, 1938, 339-375.

⁹⁹ C. Burt and W. Stephenson, Alternative views on correlations between persons, *Psychometrika*, 4, 1939, 269-281.

¹⁰⁰ M. Davies, The general factor in correlations between persons, *Brit. J. Psychol.*, 29, 1939, 404-421.

¹⁰¹ G. A. Ferguson, A bi-factor analysis of reliability coefficients, *Brit. J. Psychol.*, 31, 1940, 172-182.

Here is a specific application to a practical problem, and although he has only succeeded in "renaming the rose" (quotidian variability) the implications for other applications are apparent. In concluding this section on the analysis of mental abilities attention should be called to a monograph by Gengerelli on the structure of mental capacities.¹⁰² Gengerelli begins with Tryon's analysis of the correlation coefficient and develops a method for further solution of the extent of the common factor in correlated performances. He finds it necessary to invoke the assumption that a more complex task will involve a more complex neurone matrix, and that greater retroactive inhibition would be expected with greater overlap of such matrices. He has checked the hypotheses experimentally and determined values for his common factor. It is too soon to evaluate the importance of this work but it certainly should be examined by anyone planning a systematic study of the organization of mental ability.

In conclusion it would appear then that the entire field of factor analysis is, to quote James, in "a blooming, buzzing, state of confusion." Buzzing, yes, but the writer believes that we are emerging slowly but surely from the state of confusion. What we have at present are several systems for reducing the number of variables to smaller numbers, some rules for selecting one system in preference to another, extensive methods of computations, a growing body of evidence from applications to various experimental situations, and certainly a healthy distrust of any statement that factor analysis will reveal invariant and fundamental psychological entities.

Calculation. Statisticians, psychologists, and educators are as loath to work as any other group; as evidenced by the flood of material on ways and means of reducing computational labor. Enlow has discussed the labor saving advantages of the statistical slide rule.¹⁰³ Zubin has prepared a nomograph for determining the significance of the difference between two contrasting groups,¹⁰⁴ and Bolles and Zubin also have a nomograph for this purpose.¹⁰⁵ Casanova has an abac designed for the same purpose.¹⁰⁶ Sawkins has outlined a graphical method for determining the mean of either the whole distribution or of various designated strata.¹⁰⁷ Tables of functions of the normal curve have been prepared by Conrad and Krause¹⁰⁸ and by Palmer and Klein¹⁰⁹ which have certain advantages over the older tables. DuBois has been

¹⁰² J. A. Gengerelli, The structure of mental capacities, *Univ. Calif. Los Angeles, Stud. Educ. Phil. Psychol.*, 1, 1939, 193-268.

¹⁰³ E. R. Enlow, What the statistical slide rule will do, *Peabody J. Educ.*, 16, 1939, 292-295.

¹⁰⁴ J. Zubin, Monographs for determining the significance of the difference between the frequencies of events in two contrasted series or groups, *J. Amer. Statist. Assoc.*, 34, 1939, 539-544.

¹⁰⁵ M. M. Bolles and J. Zubin, A graphic method for evaluating differences between frequencies, *J. Appl. Psychol.*, 23, 1939, 440-449.

¹⁰⁶ T. Casanova, A simple graphical method for determining the significance of a difference, *J. Educ. Psychol.*, 30, 1939, 289-294.

¹⁰⁷ D. T. Sawkins, The use of cumulative graphs for estimation of means, higher moments, etc., *Metron*, 13, 1938, 321-345.

¹⁰⁸ H. S. Conrad and R. H. Krause, Students' tables of the unit normal curve, for abscissae expressed in terms of the probable error or P.E.: I. Areas corresponding to abscissae. II. Abscissae corresponding to areas, *J. Educ. Psychol.*, 29, 1938, 491-500.

¹⁰⁹ C. E. Palmer and H. Klein, A table of the double integral of the Gaussian probability function, *Child Develop.*, 11, 1940, 61-68.

exceptionally active recently in preparing computational aids.¹¹⁰ His method for determining the mean and standard deviation is simple and practical. He also has developed tables for use in rank correlation and has simplified the computation of chi square.

Hayes has a table which facilitates the computation of tetrachoric correlation when the percentage differences are known.¹¹¹ Dunlap has shown how Hollerith tabulating equipment can be used whenever there is need to compute a large number of such coefficients, as for example, in item analysis.¹¹²

The use of punched cards and tabulating equipment has been treated at length by Dwyer for determining moments and product moments.¹¹³ Marsh has developed a scheme for coding material by means of punched holes, and then by the use of sorting needles is able to locate all cards having a given class value.¹¹⁴ Such a method may have practical advantages where tabulating equipment is not available. An interesting note by Myers and Herschfeld deals with the effect of rounding off errors on summation formulas, and they present a table which shows the allowable maximum discrepancies.¹¹⁵

The old problem of computation of regression weights and multiple correlation has been treated by Wren¹¹⁶ and by Wherry¹¹⁷ with the result that simpler methods are now available.

Of more general value is the article by Stock on graphic interpolation for quadric and cubic equations,¹¹⁸ and Stevens' excellent article on integration and interpolation.¹¹⁹ Stevens' method is adapted for machine computation, and he gives sufficient examples so that the procedure may be followed readily.

Tests, scaling, construction, and scoring. The application of statistics to the special field of testing has been the province of several notable advances. Bradway successfully applied Thomson's method of scaling to scaling items and to determin-

¹¹⁰ P. DuBois, Formulas and tables for rank correlation, *Psychol. Rec.*, 3, 1939, 46-56; A statistical time-saver for means and sigmas, *J. Consult. Psychol.*, 3, 1939, 80-82; Note on the calculation of the Chi-square test for "goodness of fit," *Psychometrika*, 4, 1939, 173-174.

¹¹¹ S. P. Hayes, Jr., Converting percentage differences into tetrachoric correlation coefficients, *J. Educ. Psychol.*, 30, 1939, 391-396.

¹¹² J. W. Dunlap, Note on the computation of tetrachoric correlation, *Psychometrika*, 5, 1940, 137-140.

¹¹³ P. S. Dwyer, The computation of moments with the use of cumulative totals, *Ann. Math. Statist.*, 9, 1938, 288-304.

¹¹⁴ C. J. Marsh, A new punched card method for the computation of certain statistical measures, *Psychol. Bull.*, 35, 1938, 519-520.

¹¹⁵ R. J. Myers and A. Herschfeld, The summation check in statistical calculations, *J. Amer. Statist. Assoc.*, 34, 1939, 545-548.

¹¹⁶ F. L. Wrenn, The calculation of partial and multiple coefficients of regression and correlation, *J. Educ. Psychol.*, 29, 1938, 695-700.

¹¹⁷ R. J. Wherry, Two methods of estimating beta weights, *J. Educ. Psychol.*, 29, 1938, 701-709.

¹¹⁸ J. S. Stock, A method of graphic interpolation, *J. Amer. Statist. Assoc.*, 34, 1939, 709-713.

¹¹⁹ W. L. Stevens, Integration and interpolation, *Ann. Eugen. Camb.*, 8, 1938, 387-401.

ing age values for subtests.¹²⁰ Champney and Marshall¹²¹ have an excellent exposition of the rationale in support of their arguments that the optimum number of intervals in a rating scale is not 7, as Symonds has stated, but somewhere between 12 and 30. Their empirical evidence showed that the reliability of rating scales increased markedly up to 12 intervals, and thereafter slightly up to 30 intervals.

Thurstone's psychophysical method of successive intervals for scaling items has been shown by Saffir to be equivalent to the Law of Comparative Judgments. While this method is simpler than most methods it is laborious to compute. Mosier has presented a modification of the method which gives almost identical results,¹²² yet which is reliable and can be determined in about one-fourth of the time required by Thurstone's method. Richardson has attacked the complicated problem of the scaling of psychophysical judgments when more than a single variable is under consideration.¹²³ This work shows considerable promise and its further development could be of value in many problems in education and social psychology. Wherry has developed a scheme for the order of presentation of pairs of stimuli,¹²⁴ which takes into account the following principles: (1) elimination of space and time errors; (2) avoidance of regular repetitions which might influence judgment; (3) maintenance of the greatest possible spacing between pairs involving any given number of the stimulus group; and (4) balancing out of the effects of fatigue.

A new method for selecting test items has been proposed by Barry which lacks something in mathematical elegance, but has the distinct advantages of speed and simplicity.¹²⁵ Barry supports the method with experimental data by which it is shown that tests constructed by his method are at least as valid as those based on the use of bi-serial r .

The problem of like-mindedness is of peculiar interest to those who are trying to identify and classify individuals. It is important to advisors, counselors, and guidance officers to be able to match profiles against some standard. The problem of devising such standards has been considered by Zubin.¹²⁶ Zubin's technique splits a group into subgroups of like-minded individuals. The technique consists of three steps: obtaining agreement scores of each individual responding to the inventory; preliminary division of the group on the basis of these agreement scores; and finally, the determination of a pattern that produces agreement in each subdivision. The method should be useful not only for identifying groups of similar psychological syndrome but for identifying groups of similar social pattern.

¹²⁰ K. P. Bradway, Scale calibration by the Thomson method, *J. Educ. Psychol.*, 29, 1938, 442-448.

¹²¹ H. Champney and H. Marshall, Optimal refinement of the rating scale, *J. Appl. Psychol.*, 23, 1939, 323-331.

¹²² C. I. Mosier, A modification of the method of successive intervals, *Psychometrika*, 4, 1939, 149-162.

¹²³ M. W. Richardson, Multidimensional psychophysics, *Psychol. Bull.*, 35, 1938, 659-660.

¹²⁴ R. J. Wherry, Orders for the presentation of pairs in the method of paired comparisons, *J. Exper. Psychol.*, 23, 1938, 651-660.

¹²⁵ R. F. Barry, An analysis of some new statistical methods for selecting test items, *J. Exper. Educ.*, 7, 1939, 221-228.

¹²⁶ J. A. Zubin, A technique for measuring likemindedness, *J. Abnorm. & Soc. Psychol.*, 33, 1938, 508-516.

A paper of considerable theoretical importance to test constructors is that of Mosier in which he demonstrates the interchangeability of theorems in psychophysics and mental test theory, and thus has reduced both fields to a common postulated basis.¹²⁷ This paper merits serious consideration on the part of all those interested in either of these fields.

The problems attendant on test scoring have not been neglected as evidenced by the work of Bennett¹²⁸ and of Dunlap.¹²⁹ Bennett points out the loss of time in scoring personality blanks where a large range of positive and negative weights are used. He used a smaller range of weights for scoring the Bernreuter scale with a saving in scoring time and with only a small loss in accuracy.

Peterson and Dunlap were concerned with the same problem but confined their work to simplifying the scoring of the Strong Vocational Inventory. They demonstrated that the use of unit weights reduced the time for machine scoring approximately 65%. The correlations between scores obtained by the unit weight method and scores obtained from Strong's weights were on the average 0.97. Advice given to the students on the basis of Strong's weights would have been altered in only one case in thirty-three.

An unexpected outgrowth of the International Business Machines' test scoring machine is its adaptation to problems of computation. Kuder has developed a method by which the machine can be used to get means, standard deviations and intercorrelations very rapidly.¹³⁰ Another adaptation of the machine is that of Flanagan for calculating the standard error of measurement and the reliability coefficients for tests.¹³¹

The tremendous development in the scope of the applications of statistical method has resulted in a very great increase in the number of students interested in the subject. This increase in the number of fields of application and of the number of students is reflected in the astonishing number of text-books that have appeared in the last three years. No attempt has been made to thoroughly canvass this field, but incidentally some thirty texts have been noted.

It requires very little imagination on the part of the writer to estimate the magnitude of the boredom that this survey of the literature has engendered. There are, however, certain other points which might well be emphasized. One would think in view of the plethora of statistical literature, and the number of individuals studying the subject, that consumers and users of statistics would be very sophisticated. Such, however, is not always the case. In addition to some workers being statistically naïve, there is a tendency on the part of certain others to feel that having applied formulas extensively and prepared formidable tables their respon-

¹²⁷ C. I. Mosier, Psychophysics and mental test theory: fundamental postulates and elementary theorems, *Psychol. Rev.*, 47, 1940, 355-366.

¹²⁸ G. K. Bennett, The relative efficiency of fine and coarse weighting of questionnaire items, *Psychol. Bull.*, 35, 1938, 642.

¹²⁹ J. W. Dunlap, Simplification of the scoring of the Strong Vocational Interest Blank, *Psychol. Bull.*, 37, 1940, 450.

¹³⁰ G. F. Kuder, Use of the international scoring machine for the rapid computation of tables of intercorrelations, *J. Appl. Psychol.*, 22, 1938, 587-596.

¹³¹ J. C. Flanagan, Note on calculating the standard error of measurement and reliability coefficients with the test-scoring machine, *J. Appl. Psychol.*, 23, 1939, 529.

sibility has ended. The writer believes that it is the duty of the investigator to consider the implications of his data, to select the proper statistical measures, and finally to accept the responsibility for drawing conclusions within the realm of his data.

One tendency that is to be noted is the use of complicated statistical treatment when a simple statistic will serve the purpose. It may be worthwhile to comment on a few typical types of errors that have come to my attention during the past few months.

One worker reported that a group of individuals who later became successful had an initial mean error score much higher than another group of individuals who later were unsuccessful. Examination of the data revealed that both groups were small (the N of each was 10) and that the discrepancies in the means, from what might have logically been expected, were entirely due to the score of one individual who did not understand the task on the initial trial. Obviously, this extreme deviate would have been cared for, had the median been used instead of the mean. The mistake of selecting the wrong measure of central tendency is not at all uncommon in the literature.

Another fallacy which keeps appearing is the belief that if test scores have been expressed as standard scores, scaled scores, or T scores, they are directly comparable from one distribution to another. For example it has been argued recently, that if one wishes to do away with the discrepancies between mental ages secured from two different tests that all that is necessary is to express the scores of each test in terms of standard scores of the group on which the test was standardized. Obviously, if one group consisted primarily of feeble-minded individuals, a standard score of $+3$ would have quite a different meaning from a standard score of $+3$ obtained from a group of superior children. This matter of displacement in the means shows up in yet another place; namely, the problem of averaging correlations.

Although it is well known that it is poor policy to average correlation coefficients, many workers are guilty of the procedure. If one has a series of sample correlations and wishes to estimate the correlation in the universe from these sample correlations, the proper procedure is to express each correlation in terms of Fisher's z , determine the average z , and determine the corresponding value of r . Even for this purpose many workers fail to take advantage of the proper method of estimation. If, however, the purpose is to determine the correlation existing between the two variables for all the observed data, then Fisher's method is not appropriate, and one should use an algebraic method of combining correlations such as that proposed by Dunlap in the article on combinative properties of correlations. This method gives an exact value for the correlation over the entire range of observed measurements. There are two reasons why it is inappropriate to average correlations derived from different samples: first, the differences in the variances, and what is more important, the displacement that may occur from sample to sample in the means. It is for this latter reason quite possible to have two samples in each of which the correlation between a pair of variables is positive, yet when the two samples are combined to have a negative correlation.

The underlying meaning of the correction for attenuation seems to have escaped many investigators. The purpose of this function is to estimate what the correlation in the universe would be, if the available measures were perfectly reliable. It is,

therefore, quite erroneous, to correct the observed coefficients, and then to either argue or to use in other equations, these corrected coefficients as if they were observed values. Occasionally, for example, in attempting to establish the validity of a new test, the correlation between the test and some criteria is determined. This is then corrected from attenuation and the corrected coefficient reported as the validity index of the test. Such a coefficient is only of value in discussing a theoretical situation, and has no meaning in the world around us.

So much has been said and written on the standard error of a difference that it seems impossible that even a novice in experimental work would use the so-called short formula. The correlation term is always present; true, in a given case the correlation may be zero and this term drops out; nevertheless it should always be considered. In a relatively recent study, an investigator was considering a problem in which public safety was of no little importance. He reported the proper correlations in the text of his article, yet in tables where he presented the means, their differences, and the standard errors of the differences, no consideration was given the correlations reported in the data.

It has been my privilege to examine a great many technical manuscripts in the fields of education and psychology. Oddly enough, one of the most common errors, which has been observed not only in the work of young writers but also in that of mature scholars, is the failure to introduce the value of N in the standard error of the difference of means.

These are all obvious errors, so obvious that perhaps their very prominence is the reason they are overlooked. It is only on these grounds that I can plead for your indulgence of my temerity in calling them to your attention.

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JACK W. DUNLAP

STEREOSCOPIC DEPTH FROM SINGLE PICTURES

In the period around 1910, when interest in stereoscopy was high, it was widely known that the "plastic" effect could be obtained almost as well by viewing a single picture through a lens as by the use of disparate pictures in the binocular stereoscope. The phenomenon has been largely forgotten during the last two decades, and does not seem to be mentioned in any of the standard introductory texts in psychology. At present, however, amateur photographers are again becoming familiar with the effect through the use of lenses in viewing miniature prints. A surprisingly large proportion of psychologists are unaware of the phenomenon, and somewhat at a loss for an explanation.

First it should be stressed that the plastic depth that can be obtained monocularly is very striking, and must be seen to be appreciated. For optimal results the viewing lens should have the same focal length as the camera lens with which the picture was taken, but any ordinary reading glass works fairly well on pictures from 1-3 in. in size. In a typical snapshot of a person against a mixed background, the person stands out clearly, and plastic space can be seen between him and the background, as well as between the objects of the near and far background. In a good picture the person takes on solidity and roundness, with the slope of the lapel and the angle of the arms clearly in three dimensions. Like the binocular stereoscope, the lens will often clear up a confused photo. For example, the writer has seen a picture

of a fountain, composed of several figures against a background of buildings. The whole picture is extremely confusing when viewed normally; but a lens brings the figures out clearly in depth, and a number of criss-crossing lines become jets of water, properly oriented in three dimensions.

The explanation of the phenomenon is simple in outline but complex in detail, especially as applied to a single example. One may see a picture as either (1) a picture representing depth, or (2) as actual objects deployed in depth. In the first perceptual condition one remains aware of the flat nature of the picture, but under the second 'set' he gets the plastic effect. Depth, then, is not merely something added to a picture in various amounts, but rather a way of perceiving. Any picture contains a number of depth cues, such as shading, clearness of outline, perspective, and superposition;¹ but it also presents a number of cues for 'flatness'.² Identity of the binocular fields is one of the most important of these cues. Others are surface glare, failure to obtain monocular parallax changes when the picture is moved, cues from accommodation, and a number of subtle factors such as the margin of the picture. In normal binocular inspection of a picture the 'flatness' cues are strong enough to force the observer to see a flat picture; but, if 'flatness' cues can be eliminated or weakened, or if the depth cues that are present can be sufficiently exaggerated, the perception takes on depth.

There are a number of ways in which 'flatness' cues can be eliminated, permitting the depth cues to yield a plastic effect. Some of these follow:³

(1) *Looking at a picture from a distance.* This places all the pictured objects at such an apparent distance that no binocular differences would exist if they really were objects. With the ordinary photograph and most paintings this procedure, however, yields incorrect linear perspective, disturbing the perception.

(2) *Monocular viewing.* This method cuts out binocular 'flatness' clues in a positive fashion. The effect can be improved by the following:

(a) *Looking through a tube.* This cuts out cues from surrounding objects and margin, but still leaves accommodation to hamper the depth. Zoth has devised a simple instrument for this purpose, and discussed the factors involved.⁴

(b) *Looking through a lens.* This permits proper viewing distance with accommodation appropriate to the apparent distance of the objects. If the viewing lens has the same focal length as the original photographic lens, linear perspective becomes normal, as will the apparent size of objects. One of the most noticeable results is the loss of distortion of near objects, such as the nose or the feet. This is not a primary effect, of course, but is dependent upon the return to normal depth.

¹ For excellent treatments of these factors, see the following: A. Ames, Depth in pictorial art, *Art Bull.*, 8, 1925, 5-24; The illusion of depth from single pictures, *J. Opt. Soc. Amer.*, 10, 1925, 137-148; and H. C. Carr, *An Introduction to Space Perception*, 1935, 1-413.

² In passing, it should be stressed that 'flatness' is a special form of depth. Two-dimensional visual localization is concerned only with the direction of a point from the observer, or with 'lines of sight.' The judgment of 'flatness' can be made only if there are factors present which enable the observer to judge several points to be equidistant from him. Helmholtz (*Physiological Optics*, 3, 1925, 156-159) is particularly clear on this point.

³ Cf. Ames, *opp. cit.*

⁴ O. Zoth, Ein einfaches "Plastoskope," *Zsch. f. Psychol.*, 49, 1915, 85-88.

Several lenses have been marketed for this purpose, notably Zeiss' Verant.⁵ Zeiss discontinued the Verant some time ago, and now simply lists "viewing lenses" that seem to be simple convex lenses mounted on handles.

(c) *Looking at a picture monocularly in a mirror.* The mirror seems to break up the surface cues and may well have other less obvious effects, as destroying orientation. A combination of mirror and lens is especially effective.⁶

(3) *Partial binocular vision: (a) Blurring the image in one eye during binocular vision.* This may be done with a spherical or cylindrical lens. Apparently the blurred image does not give enough detail to show disparity or its absence.

(b) *Prisms to displace or rotate the image in one eye.* The explanation is probably similar to that used in (a) above, although it may involve the suppression of one image, as well as interference with normal convergence.

(4) *Full binocular vision.* The plastic effect often persists if a picture is viewed binocularly through a large lens. Distortion of the two fields may produce enough binocular disparity to cause confusion of binocular cues. Karpinska has pointed out, however, that the plastic effect can be obtained from two identical pictures viewed through two Verants.⁷ It would seem that enough of the other 'flatness' clues are changed by the use of the lenses (see 2b above) so that the effects of identity of the two retinal images are overcome. Thus absence of binocular differences is not an all-powerful clue to 'flatness.'

(5) *The Iconoscope.* This instrument is the opposite of the telestereoscope in that it decreases the effective interocular distance. It makes actual relief look flatter, as might be expected, but it also yields the plastic effect from a picture. The explanation is probably similar to that in (4) above.

It seems unnecessary to follow through our explanation in detail for each of the methods outlined above. The actual analysis of the factors favoring depth and flatness will be peculiar to each situation for each picture. Detailed analyses of some examples are provided by Ames,⁸ and Carr⁹ has devoted some space to the topic. The present discussion departs chiefly from those of Ames and Carr by stressing what might be called the "all or none" character of the effect. We do not have a simple addition and subtraction of factors, with more or less depth resulting. The perception seems to exist in two modes. In one it is still a picture. In the other mode we find objects in depth, with the characteristic plastic effect. It is true that there is some variation around the modes. A picture may be more or less flat, but still be seen as a picture. Or we may get the plastic effect, but find the depth more adequate in certain parts of the view than in others. The plastic effect, however, seems usually to be either clearly present or absent.¹⁰

⁵ A non-technical description of the Verant may be found in E. von Aster, Beiträge zur Psychologie der Raumwahrnehmung, *Zsch. f. Psychol.*, 43, 1906, 161-203, esp. 201.

⁶ M. Ponzo, Un appareil pour la vision plastique de photographies, *Arch. Ital. de Biol.*, 56, 1911, 125-126.

⁷ L. von Karpinska, Experimentelle Beiträge zur Analyse der Tiefenwahrnehmung, *Zsch. f. Psychol.*, 57, 1910, 1-88.

⁸ *Op. cit.*

⁹ *Op. cit.*

¹⁰ There are, of course, definite limitations on any method that does not make use of binocular viewing of disparate images. Disparity is particularly important with pictures of unfamiliar forms or objects, as anatomical models, where size, superposition, and clearness are of little help in perceiving depth.

In view of the simplicity of what seems to us the obvious explanation, it is surprising how much difficulty the monocular plastic effect has caused certain writers. For example, Stöhr used it as evidence for a sort of retinal stretching, particularly of the cones, which restored symmetry of stimulation.¹¹ Thus he gave the retina a special function for perceiving depth directly. Judge,¹² in his book *Stereoscopic Photography*, was very worried by the phenomenon. He was so thoroughly convinced of the need for disparity to obtain depth that he denied that a single picture viewed through a lens gave more than "a stage scenery effect." This he explained in a tentative fashion by "an overlapping of the different rays due to aberrations" (referring apparently to the binocular effect described in (4) above). It is largely the preoccupation with the spectacular (and more easily understood) effects of binocular disparity and convergence that has made the understanding of the monocular plastic effect difficult.

The discussion of monocular plastic has certain implications for perception in general. As we have pointed out above, and as the Gestalt Psychologists have long emphasized, depth perception cannot be considered as a mere addition and subtraction of factors of differing importance. This does not mean that we cannot make quantitative observations of the influence of single factors.¹³ It does mean that we must be careful in planning experiments. The classic method of varying one factor while the others are kept constant must be used with discretion. For example, if we wish to study the importance of accommodation we may arrange a binocular apparatus that keeps convergence constant while accommodation is varied. Our results will then show something about the relative importance of these two factors when they are in opposition, and for a particular range of distances.¹⁴ If, however, we really wish to find out how judgments vary as a function of accommodation, we must eliminate the effect of convergence (this can sometimes be done by using only one eye) rather than maintain convergence at a constant value. Furthermore, we must be prepared to find that certain of the actors yield different types of mathematical functions. For example, convergence can be quantified, but a factor like interposition is an all-or-none affair, even though it is powerful in overcoming other factors. The writer believes that the failure to appreciate the complex interaction between various depth cues is responsible for many of the discordant results in this field.

To summarize: the phenomenon of monocular plastic depth is due to the release of certain monocular factors from overpowering cues, largely binocular, that show the picture to be flat. These 'flatness' cues must be eliminated, and not merely held constant. Holding a factor constant is not always the best method of controlling it. This is true for depth perception, and probably for a number of other psychological processes. As a matter of fact the caution holds true pretty generally throughout all experimental fields. If an engineer were trying to plot speed against horsepower he would scarcely hold certain factors constant—for example, the car brakes firmly

¹¹ Abstracted in *Zsch. f. Psychol.*, 62, 1912, 292-293.

¹² A. W. Judge, *op. cit.*, 1926, 1-240.

¹³ M. D. Vernon (*Brit. J. Psychol.*, 28, 1937, 1-11, 115-149) has presented a somewhat pessimistic, but otherwise admirable discussion of the difficulties involved.

¹⁴ Cf. H. A. Swenson, The relative influence of accommodation and convergence in the judgment of distance, *J. Gen. Psychol.*, 7, 1932, 360-380.

applied! Similarly, when convergence or binocular differences are held constant one finds out more about them than he does about the factors he is deliberately varying.

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HAROLD SCHLOSBERG

RESEARCH INTERESTS IN PSYCHOLOGY

Although Volume 13 of the *Psychological Abstracts* was somewhat affected by political disturbances throughout the world, it may nevertheless be taken as a fair mirror of the status of psychological writing at that time and any changes that the present world war may produce in psychology may well be determined partly by checking against this volume. The present analysis shows how research interest varies from one section to another of the *Abstracts*. It also examines any differences in this respect between the members and associates of the American Psychological Association as well as those between the Association on the one hand and all others writing on the selected topics on the other hand.

The 1940 Yearbook of the American Psychological Association lists 664 members and 2075 associate members; 382 members and 505 associates were authors or joint authors of books and articles abstracted in Volume 13 of the *Abstracts*. In other words 59% of the members are represented in the volume as opposed to 24% of the associates.

The 1939 volume of the *Abstracts* contains 6557 abstracts; but because of joint authorship and repeated publication by individuals the total entries to be accredited to individual authors is 8046. Of this total, members of the Association contributed 1031, the associates 957, and all others writing on the topics concerned 6058. The American Psychological Association thus contributed about 25% of the authorship of the publications listed in this volume. What percentage of the remaining contributions were made by foreign psychologists and what by non-psychologists cannot well be determined from the available sources. If one deducts all non-Association contributions to the section on the nervous system (571) and to the section on functional disorders (995), thus reducing the total authorship from 8046 to 6480, the Association contributed 30% of the authors of this volume.

The *Psychological Abstracts* seeks to cover the world's literature in psychology and in the closely related topics of border fields. Its editorial point of view is interdisciplinary with respect to the problems considered. That is to say, no query is raised concerning an author's professional affiliation, whether he be biologist, chemist, educator, engineer, physicist, or psychiatrist. The only question is whether or not the publication contributes to the understanding of the problems included in the *Abstracts*. These problems are in general those of the scientific study of human behavior (and consciousness, for those who speak that language). Usually contributions in the following fields are excluded: anatomy, biochemistry, circulation, digestion, endocrinology, medical therapy, metabolism, and organic, toxic and epileptic disorders. It is obvious that no clear line can well be drawn between psychological problems and those of related sciences. The *Psychological Abstracts* represents the editors' best judgment on what is most pertinent and helpful to the field as a whole. Individual psychologists who are working on border problems must constantly turn elsewhere to complete their bibliographic requirements. This is inevitable, but fortunately other abstract services exist and can serve these needs.

The research interests here analyzed must then be viewed in the light of the editorial policy of the *Psychological Abstracts*.

For purposes of convenience the *Psychological Abstracts* now classifies all material in one of the following sections: (1) general, including statistics; (2) nervous system; (3) sensory and perceptual problems; (4) learning, conditioning, intelligence, including attention and thought; (5) motor and glandular responses, including emotion and sleep; (6) psychoanalysis, dreams, hypnosis, including psychological research and para-psychology; (7) functional disorders; (8) personality and character; (9) general social processes, including esthetics, music, and language; (10) crime and delinquency; (11) industrial and personnel problems; (12) edu-

TABLE I
THE DISTRIBUTION OF AUTHOR ENTRIES IN THE VARIOUS SECTIONS OF THE
Psychological Abstracts, 1939, VOLUME 13

Sections	American Psychological Association							
	Members		Associates		Total		All others	
	No.	%	No.	%	No.	%	No.	%
1	121	10	81	9	202	10	503	8
2	35	3	27	3	62	3	571	9
3	89	8	83	9	172	9	820	14
4	180	18	136	14	316	16	270	4
5	93	9	70	7	163	8	632	10
6	12	1	34	3	46	2	168	3
7	71	7	71	7	142	7	995	16
8	55	5	69	7	122	6	151	2
9	127	12	113	11	240	12	698	12
10	5	0.5	22	2	27	1	177	3
11	48	5	47	5	95	5	174	3
12	75	8	104	10	179	9	480	8
13	37	4	38	4	75	4	60	1
14	85	9	62	7	147	7	329	5
Totals	1031		957		1988		6058	

cational psychology, including vocational guidance; (13) mental tests; (14) childhood and adolescence. Much by way of constructive and destructive criticism can be said concerning this classification or concerning any substitute for it. In the present context, however, it is only necessary to have the list of sections and to note that frequently a publication, for example, an article concerning the effect of brain lesions on the learned behavior of children, might reasonably be placed in any one of several sections.

The essential results of this analysis are presented in Table I which gives the number and percentage of author entries in each of the sections of the *Abstracts* for members and associates of the Association and for all others. The percentages are secured by dividing the number of author entries in the various sections by the total author entries of members, associates or all others respectively. Thus of the 1031 member authorships 10% concerned general problems, 18% concerned learning and intelligence, and 12% concerned general social processes.

There is a close parallel between the distributions of research interests of the

members and associates which is perhaps not surprising in view of the fact that many of the associates have recently completed research under the direction of members. Associates, however, are somewhat more interested than members in Sections 6, 8, 10, and 12; while the members are more interested than the associates in Sections 4, 5, and 14.

The individuals who make up the membership of the American Psychological Association are a much more homogeneous group than are those who make up the all others' group of contributors to psychology. It is not unexpected, therefore, that the two groups should show considerable variations in the distribution of their research interests. Author interest in the 'all others' group was greater than that of the Association group in Sections 2, 3, 5, 7, and 10 but less in Sections 1, 4, 8, 11, 13, and 14, when judged in terms of the percentages of contributions in each section. The rho between the ranked interests of the various sections for the 'all others' group and the Association is 0.52, whereas the comparable rho for members and associates is 0.91. The chief factors producing the former low correlation are the interest of the 'all others' group in Sections 2 and 7, where the authors are so largely neuro-physiologists and psychiatrists not members of the Association, and the comparatively low degree of interest of the 'all others' group in Section 4.

In spite of the great practical interest which American psychologists have in clinical and industrial psychology, a fact which is indicated in the Yearbook of the Association, Sections 7, 10, and 11 of the Abstracts are in the lower range of publication (research) interest for the Association group.

Brown University

WALTER S. HUNTER

THE BERKELEY MEETING OF THE WESTERN PSYCHOLOGICAL ASSOCIATION

The twenty-first annual meeting of the Western Psychological Association was held at the University of California at Berkeley, Friday and Saturday, June 13 and 14, 1941. A total of 123 persons registered.

There were 58 papers read at the six sessions. The most interest centered about the symposium on the various projection techniques as employed in clinical work.

At the annual banquet held Friday evening, the retiring president, Mary B. Eyre, gave her address on "Psychology in Armageddon."

Officers elected for the coming year were: President, Ernest R. Hilgard, of Stanford University; and Vice-President, Howard C. Gilhausen, of the University of California at Los Angeles.

The Association is scheduled to meet next year in Seattle, at the University of Washington, probably June 19 and 20.

University of Washington

RALPH H. GUNDLACH

Francis Aveling: 1875-1941

With the death of Dr. Aveling, on March 6, 1941, in his sixty-sixth year, British psychology lost one of its most outstanding personalities.¹

Born at St. Catherine's, Ontario, on December 25, 1875, he had an exceptionally brilliant educational career, first at Bishop Ridley College, then at McGill University, Keble College, at Oxford, Canadian College, in Rome, the University of Louvain and the University of London.

Like several other eminent psychologists of his generation, he came to psychology relatively late and by way of other studies. His early training in theology and philosophy left upon him enduring influences, without narrowing his outlook or dimming his scientific clarity, for he was a clever experimentalist, conspicuously bold in thought, being more aware than most experimentalists of the relativity of hypotheses and the conventions of logic.

After taking his Ph.D., at the age of 35, under Michotte at Louvain, he came to the University of London where, in 1912, he became lecturer in the department of Spearman, the first professor of psychology at University College, University of London. It was in this year that he received the D.Sc. degree, with the additional distinction of the Carpenter Medal, mainly for his work "On the Consciousness of the Universal and the Individual: A Contribution to the Phenomenology of the Thought Process."

Aveling was profoundly interested in epistemological problems. One of his greatest contributions to knowledge resided in his relating of developments in the psychology of cognition to persistent problems of philosophy. His vigorous and penetrating mind was always alive to new implications, yet singularly immune to deception by passing fashions. By reason of these qualities and a common interest in the philosophical roots of psychology and in continental experimental schools, he was an ideal colleague for Spearman, whose genius he was quick to recognize and whom he steadfastly admired. Their appreciation of philosophical foundations was much more than a sharing of the English tradition of Ward and Stout. They discovered a common loyalty to the logical clarity and systematic categories of the Scholastic philosophers, which is well expressed in Aveling's article in 1911 on "The Modernism of St. Thomas Aquinas." Perhaps the most complete expression of Aveling's application of psychology to philosophy is found in his *Psychological Approach to Reality* (1929), in which he employed Spearman's noegenetic principles brilliantly to explain the concept of causality, which had so puzzled Malebranche, Hume and Mill, and to demonstrate a new way out of the ancient dilemma of solipsism.

Except for the war years, which he spent in France as an Army padre, being mentioned in despatches and receiving the Military Cross, Aveling worked continuously in Spearman's department until 1922. Then he moved to University College's largest sister college in the University of London, King's College, becoming Reader and director of the department, in succession to C. S. Myers. Shortly afterwards (1925) he was married to Ethel, daughter of Mr. S. G. Dancy of Steyning, Sussex.

Between 1922 and his death he built up the department from the status of a

¹ Dr. Aveling's full name is Francis Arthur Powell Aveling, but he preferred not to use the middle names.

barely tolerated parvenu to that of a respected pillar of college scholarship. Its progress may be illustrated by the history of its housing. The laboratory was at first a couple of cellars, two flights below the level of the Strand and smelling faintly of Thames River water. These cells had been dungeons for the least popular lodgers when Somerset House accommodated Elizabethan political prisoners. It finished in a suite of airy rooms behind the main pediment of the building. Against the obstacles facing psychology in an English University it is likely that nine psychologists out of ten would have failed where Aveling succeeded. His training in divinity and philosophy, his knowledge and appreciation of classical discipline, his remarkable all-around scholarship (for he then held the degrees of Ph.D., D.Sc., D.D. and D.Litt) enabled him to understand and to be understood by one of the oldest colleges of London University.

At King's College Aveling's interests spread from his pre-occupation with cognitive psychology into a variety of new fields. One of his articles soon after the war had to do with observations of fear responses in war situations. An interest and a rich fund of observations concerned with emotion and personality had remained in reserve from his experience as a priest and a padre. These topics became systematized in his studies of motivation, personality and will, studies which were complementary to Spearman's school of cognitive research at University College, yet methodologically in line with it. By reason of the breadth of his experience, Aveling was able to take an unprejudiced interest in the ideas arising in psychoanalysis, though he questioned its methodology. Following Plato and the Scholastics, he preferred the term 'orectic,' connoting 'conative' and 'affective,' to define this field of study, and in methods he chose to follow the strict experimentalism of his teacher, Michotte, and of Ach. Meanwhile he also continued to be an intellectual midwife to the ideas of Spearman, for, although he did not share Spearman's mathematical gifts, he had a mathematical sense of logic and beauty, which appeared in his use of words. As a philosopher, he respected mathematics for its precise symbols, and he built up impressive equations not only of mathematical solidity but also of rich Latin sonority. The debt which Spearman owes to Aveling's scholarship and judgment he acknowledged during Aveling's lifetime, describing him as "the most sympathetic, wise and intimate counsellor I have ever had." Aveling's contributions to psychology can best be understood in the light of this fruitful and life-long symbiosis.

Aveling was not a man to suffer fools gladly, and the only thing which annoyed him more than sloppy, silly thinking was pretentious, artificial systematization. Like other eminent Canadians he was in some respects more British than the British, a state of mind which may partly explain the ease with which he fitted into the intellectual societies of London. Part of this pattern was a certain formidable reserve, which, in combination with his potent terseness, made him a teacher difficult of approach for most undergraduates and some graduates. Those who knew him for any length of time, however, appreciated the warmth and rich humor behind his uncompromising exterior, so that he became ultimately the object of many deep personal loyalties.

As a critic and evaluator he shone. On one important point he parted company with Spearman, for, whereas Spearman turned from the continent towards the vigor, empiricism and adaptability of American psychology, it must be admitted that Aveling reacted against what he considered the extravagance, indiscipline and lack

of perspective of certain movements in this country. He was not impressed by Watsonian behaviorism. He had so exact an historical perspective that he could give verbatim chapter and verse for basic psychological concepts in, for example, Plato or Aristotle. Thus, when some 'new' notion swept the psychological world, he was less moved by the independence of the modern discoverer than by the fact that the 'new' idea could have been obtained in a more perfect form, shorn of naïve errors, by a little wider reading.

In the course of the journey from his early concentration upon cognitive problems to his later researches on personality and will, he naturally tended to deal with certain problems that link the two fields. One problem which continually exercised him was the influence of conation upon cognition. Here his interest extended from the investigation of the effect of mental set upon the cognition of tachistoscopically exposed symbols—an undertaking in which he arrived at some unexpected generalizations—to studies of the subjective character of cognition and the effect of striving upon the awareness of the self. Here too, incidentally, he made some advances in the technique of the psychogalvanometer, which he was using as a tool.

A concise interpretation of his contribution to psychology is not easy. He was essentially too broad in his interests to be classified in one school and had a Galtonesque facility for tackling the most yielding salient of obscurity with the most handy weapon. As Boring has said, comparing German and English psychology, "German psychology was institutionalized . . . we find schools and their leaders. In Great Britain we have to deal with persons." Aveling was surely one of these persons. Indeed his life is of particular interest because it epitomises, in itself, the journey which psychology and all free psychologists made in that generation.

Partly because of his late start and partly because of his high standards of self-criticism, he was not greatly productive. Indeed, there are not many European psychologists of his ability less known on this side of the Atlantic. One of his greatest contributions, however, was as an organizer and the wielder of a shrewd pruning hook on the tree of psychological development. Not only through his official positions in the British Psychological Society, of which he was president from 1926 to 1929, but also in the many personal influences which his sturdy character and supreme scholarship enabled him to exert, he did much to establish the status of psychology and of psychologists in Great Britain. In addition to his connections in academic psychology he was a member of the Council of the International Congresses, of the Aristotelian Society, of the council and advisory board of the National Institute of Industrial Psychology, of the council of the British Institute of Philosophical Studies and of the Child Guidance Council.

It is doubly sad that the influence of a man of such scholarship and organizing wisdom should have been withdrawn at the moment when British psychology faces the emergency and the opportunity of the war.

Harvard University

RAYMOND B. CATTELL

William Henry Burnham: 1855-1941

News of the passing of William H. Burnham will probably come with a touch of surprise to the many psychologists who had not thought of him as a living personality for many years. His was a long life clouded toward its close by years of failing health and, nearer the end, by complete blindness. He was the last survivor of that distinguished group of men who, led by G. Stanley Hall, once made Clark University famous for its teaching and research in the fields of psychology and allied subjects.

Dr. Burnham died on June 25, 1941, in Dunbarton, New Hampshire, the town in which he was born, nearly eighty-six years before, on December 3, 1855. It was a place he loved and one in which he had spent his summers for many years. He was the youngest of seven children. He was a Harvard man of the class of 1882, graduating with honors in philosophy. After teaching for three years, partly at Wittenberg College in Springfield, Ohio, and partly at the Potsdam State Normal School in New York State, he went to Johns Hopkins University to study for the doctorate. This degree he obtained in 1888, remaining at Hopkins as an instructor for two more years and then going to the newly established Clark University in 1890. At Clark he became successively docent, instructor, assistant professor and finally full professor of education and school hygiene in 1906. His retirement came in 1926, and Clark University honored the occasion by conferring upon him the LL.D. degree. Thereafter he remained almost continuously in Worcester.

His life was comparatively quiet, amazingly undisturbed by the stresses and distresses of the university he served. He travelled but never extensively. His former students, who have good reason to recall his mastery of the German language, will be surprised to learn that his mastery of that tongue was acquired almost wholly in this country. Study and a long residence in the home of a German family during student days account for his proficiency. He went to Europe once but was in Germany only a short time.

Psychologists not trained at Clark will recall Dr. Burnham first for his contributions to our knowledge of memory. Many others will think of him for his energetic efforts in the field of mental hygiene. For him this subject was but incidentally the prevention and care of mental disease for he thought of mental hygiene as everything which contributes to the wholeness and efficiency of living. His books on mental hygiene have been widely used and are well-known. He became a member of the National Committee for Mental Hygiene in 1917 and continued to serve in that body until his death. He assisted in the organization of the Massachusetts State Society for Mental Hygiene and was its president for five years (1916-1920).

His former students will recall Dr. Burnham as a careful teacher of subjects largely secondary in the training of a psychologist—education, learning and school hygiene. In these fields he often left his students bewildered by his many references and obvious mastery of a complex and voluminous literature. His students and his fellow townsmen will also recall the sly, penetrating, subtle humor which characterized his every address or lecture. He was a quiet, laughing, happy man. No one ever loved a good story better and few ever surpassed him in the art of telling one. Many scholars also will long be indebted to him for his work as an editor. During many of the years in which he was listed as assistant or associate editor of the *Pedagogical Seminary* (1900-1941), he carried the burden of editorial work. It

was his judgment which selected and rejected papers for publication, and it was his supervision which resulted in their accurate publication.

No mention of the life and work of Dr. Burnham would be complete in his judgment without the inclusion of a word concerning the peculiarly valuable service he enjoyed from his very competent secretary, Marian Ross. Few men have ever been so well served. She began work with him when she was just out of high school in 1910 and remained with him thereafter continuously. He gave her much credit for that mass of detail work every scholar knows is essential to effective study, teaching and publication. His last coherent utterance concerned her.

Dr. Burnham was not by temperament an experimentalist, yet no one delighted more than he in the results of good experimental work, and few could be more acute in the perception of errors in experimental procedure. He was to some extent a creative thinker, but his genius lay in the consolidation of everything ever published in any language in the fields of his interest. He loved new ideas, new conclusions, new ways of doing things, yet with that love he never lost the simple common-sense which usually characterizes the great scholar.

Indiana University

EDMUND S. CONKLIN

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

The Factors of the Mind: An Introduction to Factor-analysis in Psychology. By CYRIL BURT. New York, Macmillan Co., 1941. Pp. xiv, 509.

Writing especially for the general psychologist who may have little mathematical or statistical background, Burt undertakes to discuss factor analysis in terms of its logical foundations. The book is divided into three parts, the first two dealing with "the logical and metaphysical status of factors in psychology" and "the relations between different methods of factor analysis," respectively. The third part is included primarily as a concrete demonstration of the theoretical conclusions reached in the earlier sections; in it the various techniques discussed by the author are applied to data on "temperamental types." Two appendices on computational methods are included, one outlining the steps to be followed in the various factor analysis techniques and the other giving the general algebraic problem of the analysis of a matrix into its latent roots and vectors, a problem which is basic to all factor analysis techniques using "weighted summation" or "least squares" methods.

In his discussion of the *nature* of "factors," Burt takes issue with the views of many other factor analysts, such as Holzinger, Spearman, Stephenson, and Thurstone, and adheres closely to an operational definition of factors. Much of the book centers about the logical demonstration that factors are not to be regarded as concrete psychological entities or abilities, hereditary determiners, or hidden underlying causes of behavior, but as "terms of reference" and logical "principles of classification" for the description of behavior. Burt points out that "a factor is simply an average or sum total of certain measurements empirically obtained" (p. 74) and appears "as a synthetic rather than an analytic result" (p. 75). The primary task of factor analysis, according to Burt, is to supply a systematic hierarchy of independent concepts, in terms of which the behavior of an individual to a test situation can be described. Since such behavior varies not only discontinuously or in kind, but also continuously and in degree, quantitative description is introduced as a refinement of the classificatory concepts, but such numerical expression is "merely a device for making our efforts at systematic classification more rigorous and more precise" (p. 138). Finally, Burt insists that, although the philosopher may speak in causal terms and the practical worker may use such terms crudely in reference to prediction, the empirical science of psychology must remain at the level of description and speak of factors only in terms of "functional relations." Such factors refer not to the tests, nor to the persons, but to the behavior of individuals to the test stimuli.

It also follows logically from this conception of factors that whether we begin by correlating tests, as in the more usual procedure, or by correlating persons (so-called "inverted factor analysis," recently come into vogue for the investigation of "types") our analysis should yield the same results. By either procedure, the general factor which would be obtained by the alternate procedure is discarded. In trait correlation, this general factor corresponds to one individual's all-around higher level of

performance than another's; in person correlations, the general factor represents "the relative ease or difficulty of the tests" or "the relative strength of the abilities in the whole population tested" (p. 198).

In his survey of different factor *theories*, Burt characterizes all such theories as special cases of a fundamental "four-factor theory" which includes general, group, specific, and error factors. As far as psychologically significant factors are concerned, he considers a broad form of multiple factor theory to be the best working hypothesis. Factor analysis *procedures* are classified into (1) analysis of variance and (2) factor analysis in the narrower sense, the latter including analysis of co-variance and of correlation. The techniques are also sub-divided into group factor methods (in which the correlation matrix is partitioned into sub-matrices) and general factor methods, including weighted summation and simple summation of the correlation coefficients. Burt concludes that "the results of the different procedures, though often discrepant at first sight, could nevertheless be regarded either as approximations to, or as linear transformations of, one and the same set of theoretical values" (p. 256). He further maintains that there is no one best method of factor analysis, each type of problem requiring "its own peculiar devices" (pp. 269-270). In the last section of the book, Burt demonstrates the equivalence of many of the techniques he discusses by applying them to ratings of personality traits obtained on 124 delinquent or unstable children and on a larger sample of 1096 subjects consisting of several groups differing in age, sex, intellectual, and educational level.

The book seems to be somewhat loosely organized and would probably gain in clarity and effectiveness by a rearrangement of the sequence of topics. The extensive use of small type passages and lengthy footnotes in still smaller type further handicap the reader's progress. These weaknesses are understandable in the light of Burt's own statements in the Preface that the book evolved from lectures and mimeographed notes for class use and was published rather more hastily than had been planned. It might be remarked, however, that clarity of presentation is of especial importance in a field such as factor analysis which is notorious for its amazing misunderstandings and the protracted and wordy controversies arising from them. Many examples of such controversies are cited by Burt himself. Finally, it might be questioned whether the material presented by Burt is suited to publication in book form. Much space is taken up with controversy and the refutation of criticisms. Most of the points regarding the nature of factors, furthermore, have been made before by other writers; no reference is made to a number of highly relevant American studies on this problem. For readers familiar with factor analysis, the book is too repetitious and many points are unnecessarily drawn out. On the other hand, it is doubtful whether the statistically unsophisticated reader, to whom the book seems to be especially addressed, can learn much from it. To such a reader, one of two alternatives seems to be open: either he must accept on faith many of the points presented in large type; or he must postpone reading the text proper until he has mastered the appendices and perhaps also studied some of the suggested mathematical readings to fill in the gaps in his preparation.

Queens College

ANNE ANASTASI

Comparative Psychology. II. Plants and Invertebrates. By CARL J. WARDEN, THOMAS N. JENKINS, and LUCIEN H. WARNER. New York, Ronald Press, 1940. Pp. xiii, 1070.

The entire psychological profession must applaud the fact that the second and last of this three-volume series has appeared. Taken as a whole these three volumes are a monument to a great scientific movement which is still advancing. In half a century scientific comparative psychology has risen from almost nothing to a point at which it is recognized as a field fundamental in certain respects to all the rest of psychology.

To the present volume, which because of the order of publication is to be considered as Volume II, a bibliography of almost 5000 papers is appended. Reference is made in the text to each of these papers! Many of these research articles represent work carried on in European laboratories and published not infrequently in obscure Continental biological journals. It is all the more valuable therefore to the American psychologist to have this material brought together in coherent, systematic form. The book has been so arranged that the specialist can easily work back to any original source. On the other hand, this volume is not a mere series of abstracts. The authors have assimilated the almost overwhelming mass of material with which they have dealt, and the result is a surprisingly coherent and interesting presentation.

The book is divided into seven chapters which are really sections. The first deals with protista, isolated cells, and tissue cultures. Here the fundamental life processes of living cells which are basic to an understanding of behavior are dealt with. One who is interested in the present factual state of the field once burned over by the great tropism controversy will find this chapter especially interesting. Chapter 2 deals with the metaphyta or multicellular plants. Here the great advance in a truly fascinating field since the publication of such books as Darwin's *Movements and Habits of Climbing Plants* is presented. In the third chapter the behavior of the porifera and coelenterata is described. These two groups include the simplest of the multicellular animals. Here the reader is given a story of the gradual development of the specialized structures and functions which later blossom in high forms as the typical response mechanism of the vertebrates. Still, in these animals behavior is to be considered more as the sum of part activities than as a function of an integrated or unified organism. The next chapter deals with the echinodermata, a group which includes such animals as starfish and sea urchins. Here for the first time the literature on the modification of behavior begins to occupy a large section of the chapter. Chapter 5 discusses the behavior of the platyhelminthes, nemathelminthes, trochelminthes, and annulata. These organisms were all placed by Linnaeus in a single category of worms but are now recognized as independent groups by taxonomists. Here the coördination of movement initiated by specialized receptors begins to be the central theme. This section ends with a review of the dramatic experiments of Yerkes, Heck, and others in which the maze learning of earthworms is demonstrated. By many readers this group may then be considered as marking a great turning point in the story of the development of mind. This is not to say that anything radically new has entered at this stage but merely that development has progressed so that we see in recognizable form an emergent property that is centrally characteristic in the mental processes of

high forms of life. In Chapter 6 the mollusca, the phylum which includes such animals as snails and oysters, is considered. This group of animals has many peculiarities related to their rather specialized soft anatomical structure. Some of these organisms possess remarkably well-developed nervous systems. Piéron, for example, suggests that the octopus is superior in learning capacity to some of the lower vertebrates.

The seventh and last chapter deals with the arthropoda. This is of course the phylum which includes such organisms as the lobster and the insects. There are more species in this phylum than in all the other phyla combined. In the bibliography of this chapter more than 2206 references are given. Here not only are receptors and nervous system highly developed but also more complex forms of integrated behavior develop. Certain of these behavior patterns are quite rigid and relatively unmodifiable. On the other hand the ability to learn habits requiring a high level of discrimination has been demonstrated in this group. It is interesting to note that certain forms of standard apparatus for experiments with learning which are now used in the study of mammals and even in human psychology were first developed for the accurate investigation of habit formation in insects.

The book here discussed is a really great and useful piece of work and the trinity of volumes shows that American science in the field of behavior has come of age. The reviewer cannot resist a word of personal gratitude to the able collaborators who conceived and carried through this most difficult and important task. The work of any student of behavior in the future has been made more easy and effective because these three volumes have been published.

Tufts College

LEONARD CARMICHAEL

Political Propaganda. By F. C. BARTLETT. Cambridge, University Press, 1940. Pp. x, 158.

This volume not only tells about political propaganda in an able way; it is in itself political propaganda of the highest type. The author devotes the brief volume to a critical comparison of propaganda in dictatorships and in democracies. In so doing, he has not only presented a very valuable essay on the basic assumptions and mechanisms of propaganda but has provided as well a moving treatise on the assumptions and mechanisms of the two contrasting forms of government. The result can scarcely be counted as another victory for totalitarianism.

The central theme of the book is a discussion of the totalitarian conviction that propaganda must be based upon "an unshakable belief that people in the mass exhibit a childish, primitive, inferior, mean and altogether despicable intelligence." Bartlett holds this view to be unnecessary, destructive, and "opposed to the most fundamental of all the characteristics of human development." He shows that propaganda may be directed either towards the creation of blind acceptance or towards the development of a careful weighing of available evidence on the part of the recipient. Looking at recent propaganda on the Continent, he finds "the same forms and characteristics . . . repression; censorship; the secret exercise of force; growing interference with education, especially early education." Prestige, he finds, has been maintained by such extraneous aids as physical force, control of the printed word and the published picture, the use of radio, and the employment of uniforms, titles, and other symbols. This prestige then is used to sanction direct

propaganda aimed at maintaining the public in a condition of stress and crisis, re-enforced when necessary by indirect propaganda.

Quoting frequently from the writings of the totalitarian leaders, Bartlett considers the various devices that are employed by propagandists. Repetition with variation, replacement of argument by categorical statement, strong arousal of emotion, the use of symbols and slogans, the use of humor, the employment of 'barrage' tactics, and the use of apparently sound statistics are among the devices scrutinized. Discussing the last-named device, Bartlett points to the deliberate misuse of statistics as one more evidence that "more education is required to destroy what a little education has helped to build."

A chapter on the effects of propaganda is written with the realization that methods of 'measuring' such effects are crude and primitive. Those who indulge in long-term propaganda are more interested in effects than in the precise timing of the effects. The short term propagandist uses a dangerous weapon, for he must adjust his timing to the tempo of the group, he may obtain a premature sectional reaction or a stampede, or he may succeed only in eliciting boredom. Long-term propaganda, couched in the idiom of the group and designed to be internally consistent, is a much more dependable medium.

The brief volume closes with a chapter on "Propaganda for Democracy." Holding that "no great nation group in the world can afford to neglect political propaganda," Bartlett points to the tragic effects of abandoning propaganda during peaceful periods in democratic states. A reliable news service, operating in war time under a benevolent censorship of elected individuals who select, shape, and transmit news, is found to lie at the core of effective democratic propaganda. In peace-time censorship is to be abandoned. Democracy can well learn from Totalitarianism "a whole-hearted belief in its own weapons; persistence; a readiness to tackle any problem. . . . But on almost all counts it is radically different. It does not despise the intelligence of those whom it addresses. . . . It knows that the stability of a social order does not depend upon everybody's saying the same things, holding the same opinions, feeling the same feelings, but upon a freely achieved unity which, with many sectional and individual differences, is nevertheless able to maintain an expanding and consistent pattern of life. It does not consider that the only thing that matters is the next moment, but plans ahead and for the long view. It is an incident in an educative process: dictator propaganda, as it has been developed, is one of the prime enemies of education."

Believing that Bartlett has provided in this book an excellent example of the very type of democratic propaganda just described, the reviewer urges this thoughtful volume upon his colleagues and upon their students.

University of Maryland

JOHN G. JENKINS

Problems of Ageing: Biological and Medical Aspects. Edited by E. V. COWDRY. Baltimore, Williams & Wilkins, 1939. Pp. xxx, 758.

A sampling of the contents of this book would take one from the "mummy" wheat seeds of the Egyptian tombs to the effect of uranium nitrate on the liver, from fraudulent oil promotions to the five thousand year old Big Tree of Tule, and from hypothetical time and rats genes to the ultraviolet content of the Colorado sunlight. The freest of free association principles would fail to account for such a

bewildering range of thought and it must be noted at the outset that this work is a coöperative one with 26 different authors contributing to as many aspects of a single broad problem.

Professor Dewey, who contributed the "back-drop" for the stage on which the 25 scenes of the ageing drama are presented, has done a characteristic bit of writing, revealing in broad strokes the inexorable effect of a rapidly increasing average life span—the "population pyramid" has already disappeared and is supplanted by a contour resembling "an egg cut off at the base"—upon the problems of industrial efficiency, employment, immigration, public health, education, and even political and general social attitudes. The biological conditioners of these changes are, of course, of utmost importance, and the symposium has been commendably arranged to assay present knowledge of the biological (and to a very minor extent, psychological and cultural) effects of age.

The first half-dozen chapters deal with the ageing of total organisms: plants (Crocker), protozoans and invertebrates (Jennings), insects (Howard), sub-human vertebrates (Todd), man as anthropologically considered (Wissler), and man treated biometrically (Dublin). Chapters 7 to 19 are concerned with tissue systems: cardiovascular (Cohn), lymphatic (Krumbhaar), digestive (Ivy), urinary (Oliver), skeletal and locomotor (Todd), skin (Weidman), glandular (Carlson), female reproductive (Allen), male reproductive (Engle), "psychosexual" (Hamilton), nervous (Critchley), ocular (Friedenwald), and aural (Guild). The remaining chapters employ functional differentia: psychological aspects of ageing (Miles), chemical aspects (McCay), homeostatic mechanisms (Cannon), tissue fluids (Cowdry), tissue susceptibility and resistance (MacNider), and ageing clinically viewed (Barker).

Perhaps the greatest value of the book lies less in the facts it reveals than in those it fails to disclose. One is constantly impressed, from chapter to chapter, with the paucity of well-rounded information. If the stock-taking is a fair one, and it must be supposed that it is in view of the authorship, exact knowledge concerning ageing has been accumulated rather than sought as an end in itself. In each field, it appears that a number of minor problems have been worked through to conclusiveness but the attack on the main theme has not, in the main, been systematic or embracing. The difficulty of manipulating age as a controlled experimental variable is obvious and the temptation has apparently been strong, on the parts of many, to extrapolate into the later decades descriptive facts and empirically established principles gained through a study of the first half of life. We have gotten away from the picture of the youngster as a "little man" and perhaps we need also to relinquish the conception of the oldster as simply "old man."

It may be that the whole problem of ageing will grow in insistence as its practical implications become more obvious. The social and economic consequences have already demanded recognition; social security programs, after all, are some distance removed from the primitive practice of killing the aged. When it is contemplated that within forty years the proportion of those in the population exceeding 65 years of age will have more than doubled, there can be little indecision as to whether scientific information of the most systematic sort, covering all aspects of senescence, will be needed. Pediatrics and paidology have had a natural and healthy development; geriatrics and gerontology will be forced, by social and cultural demands, to take a similar course.

University of Virginia

FRANK A. GELDARD

The Essentials of Mental Measurement. By WILLIAM BROWN and GODFREY H. THOMSON. Fourth Edition. Cambridge, The University Press, 1940. Pp. x, 256.

The first edition of this book (by William Brown) was published in 1911. With Godfrey H. Thomson as co-author, the book was rewritten and the second edition, containing 10 chapters, was published in 1921. Since that time the book has not been rewritten. A number of notes were added, chiefly at the ends of chapters, and one new chapter was written for the third (1925) edition. The authors explain that "in the circumstances of the present year" (1940) it is impossible for them to rewrite the book. Hence, the fourth edition is the same as the third, with the addition of four new chapters, which are reprints of recent papers by each of the authors. A number of topics of current interest are not covered; e.g. there is no reference to the work of R. A. Fisher and others on small samples or the analysis of variance, and only four pages are devoted to multiple correlation. On the other hand, the recent work on factor analysis is covered in considerable detail, this being the main subject of all five of the chapters which have been added since 1921.

The first four chapters deal with psychophysical problems, and statistical concepts are introduced only as they are needed in understanding, describing, or analyzing the data. Consequently, those statistical methods which are immediately applicable to the data are sometimes unduly stressed at the expense of other methods which are applicable to other psychological problems.

The next four chapters give a good explanation of the meaning of correlation and point out certain pitfalls in interpretation. In common with many statistics texts, the computational methods illustrated are not always the best for practical use (either with or without a calculating machine). Thus, class intervals are not always equal (p. 115), and partial regression equations are given in terms of deviations from the respective means, instead of in terms of gross scores (p. 148).

Chapters IX through XV, covering theories of mental ability from Spearman's Theory of Two Factors through modern factor analysis, deal with controversial material and, in the reviewer's opinion, vary considerably in their quality. Chapter XIII concludes that "the main purpose of our research has been achieved; namely, to establish Professor Spearman's Theory of Two Factors on an adequate statistical basis." But some psychologists may object to this conclusion, for it was arrived at after (1) devising "tests that, approximately, should fit a theoretical criterion (that of zero 'tetrads')," (2) omitting two tests, (3) partialling out the v -factor from six verbal tests, (4) partialling out the specificity from the two Code tests, (5) partialling out the specificity from two of the perceptual tests, (6) discarding tetrads which involve r_{6-18} (as 7 of the 23 largest tetrads did), and (7) omitting one test which "can be considered to account for nearly all other tetrad differences greater than 0.1000." Chapters XI and XV, also on factor theory, are sincere and competent attempts to examine controversial theories in an unbiased and strictly objective manner.

On the whole, the book is a scholarly presentation of the field of mental measurement. It contains only a small number of minor errors, and it is written in an interesting manner. The large amount of original material it contains will make it valuable not only as a text for students but also as a reference manual for research workers in psychophysics, psychological statistics, and factor theory.

Life Insurance Sales Research Bureau

ALBERT K. KURTZ

Vowel Sounds in Poetry: Their Music and Tone-colour. By M. M. MACDERMOTT. London, Kegan Paul, Trench, Trubner & Co., 1940. Pp. 148.

That reader who is more the psychologist than the phonetician will be grateful, or should be, for the specific information that he will find what he is most likely to be looking for in this book on pages 88 to 93. But what he finds there will startle him less if he comes upon it after having groped his way through the preceding 87 pages of exposition and narrative, in which an English lover of poetry tells how he constructed a complicated phonetic technique with which to discover what he sets forth boldly and concisely in his concluding statements. What he sets forth is nothing less than a catalog of the subjects—the kinds of sounds, objects, moods, motions, attributes of personality, sensations, emotions, activities and aspects of reality in general—that can best be, or should be, or tend to be expressed by high and low "pitched" vowels, respectively, in poetry.

The procedure used by Macdermott in analyzing "hundreds" of poems or parts of poems is much too involved to be presented in a brief review. It depended upon the use of a phonetic alphabet, somewhat more highly differentiated than that of the International Phonetic Association; a seven-fold classification of metric forms, involving consideration of both stress and duration; a classification of vowels as to "pitch," based on the work of Paget, Crandall, Fletcher, and others; a consideration of the harmonic characteristics of vowel sounds and of their mechanical means of production; and the practiced use by the author of his own abilities to read poetry interpretively, to make valid phonetic discriminations by ear, and to relate the semantic shadings of words with the phonetic or tone qualities of vowel sounds. He gives no indication of the degree to which he agreed in the exercise of these abilities either with himself or with other workers. Until some statement of the reliability and validity of his procedures can be made, the author's conclusions can hardly be said to be true or false.

The reviewer would call attention to the fact that the author's conclusions, as above abstracted, might readily be checked to an important degree by means of a phonetic analysis of terms classified according to their standard dictionary meanings, and by a study of the changes in vowel quality which occur with changes in semantic value expressed by means of any given word. In these ways, among others, the issue of the semantic significance of speech sounds, as such, might well be somewhat clarified. It is this general problem which Macdermott has attacked, and it is an important one. His treatment of it is rather more scholarly than methodologically defensible. His conclusions, certainly, give considerably more information about Mr. Macdermott than about the "music and tone-colour" of the vowel sounds in poetry.

University of Iowa

WENDELL JOHNSON

Dostoevski: The Making of a Novelist. By E. J. SIMMONS. New York, Oxford University Press, 1940. Pp. x, 416.

The present study is an essay on the development and fluctuations of Dostoevski's ideas as they are revealed in his literary art. In executing his plan Simmons discovers certain contrasts and fixed ideas which are present from the beginning of the author's creative activity, but which gradually attain definiteness and amplitude of content in later work. These ideas form the persistent background of all Dostoevski's work

in philosophy and fiction. They reveal at once the type and motivation of Dostoevski's personality and the dominant social and political tendencies of the age in which he wrote, which was the heart of the Victorian period from about 1845 to 1881. In an introductory chapter on "Creative Beginnings," Simmons points out Dostoevski's middle class social status, the conflict between his education in military engineering and his literary aspirations, his predilection for writers of the Romantic School, and the presence of romanticism as well as realism in all of his writings. The following chapters present a systematic analysis and interpretation of Dostoevski's writings from the modest initial effort in *Poor Folks* to the magnificent novels of his final creative period. There are twenty-two chapters of analyses and interpretations in which the general creative situation as well as the special setting of each of the works is carefully set forth.

Dostoevski belonged to an early revolutionary group with the result that he suffered imprisonment in Siberia where he experienced a kind of spiritual conversion, becoming a bitter critic of a later generation of radicals. Youthful participation in a radical movement, prison life in Siberia attended by spiritual and religious conversion, and later, unfortunate experiences with women, became the determinants of the contents and motivation of Dostoevski's literary works and the theory of human nature which they express. Yet no less real is the reflection of life-long conflict in the personality of the creative artist, the struggle of faith with doubt, the struggle between spiritual mysticism and rationalism. It is a conflict that persists throughout life and lends vitality and plausibility to all of his characters whatever their type or social and intellectual level.

Dostoevski's characters reveal the special conflicts of their author and his tendency to dissociation. Most of the characters belong to one of three well defined types which Simmons classifies as: (1) the meek unselfish and almost selfless type such as Prince Myshkin in *The Idiot* and Alyosha in the *Brothers Karamazov*; (2) psychological doubles or dual personalities like Raskolnikov in *Crime and Punishment*; and (3) the egoistic aggressive, amoral, and often criminal type such as Stavrogin and Verkhovenski in *The Possessed* as well as Ivan Karamazov.

Simmons' scholarly work will remain of permanent value to psychologists interested in the motives and mechanisms of artistic creation. Without claiming to offer a complete psychology of Dostoevski but merely an account of the artist's creative processes, Simmons throws more light upon the motivation of both the author and his characters than can be obtained from many professedly psychological accounts. There is also an excellently selected bibliography of seven pages which will facilitate further study on the part of the psychologist.

University of Cincinnati

CHARLES M. DISERENS

The Psychodynamics of Abnormal Behavior. By J. F. BROWN. New York, McGraw-Hill Book Co., 1940. Pp. xi, 484.

Psychodynamics is the study of the integration and disintegration of the human personality. Personality is the pattern or arrangement of the traits in an individual—his physical characteristics, his health and stamina, his intellect, his emotions, and his temperament; not simply the sum total of all these traits, but rather *the way in which they are combined to give the individual his individuality*, that dynamic organization or configuration of traits which determines the individual's unique adjustments to his

environment. Abnormal reactions, exaggerations or perversions of normal mental phenomena, are subject to the same laws, and have meaning and significance in the total situation. Explanations of abnormal reactions as either organic (somatogenic) or functional (psychogenic) are unsatisfactory, and organismic solution of the mind-body relation is selected as the basic postulate of this book.

"From this viewpoint," Brown asserts, "the problems of psychologist versus psychiatrist, functionalist versus organicist, hereditarian versus environmentalist, will be seen to be meaningless." As the development of the physical sciences has been from class theory to field theory, so psychopathology must concern itself less with descriptive symptomatology and develop a psychodynamic theory. Though all the important modern theories of psychodynamics have points on which they are in general agreement, the author prefers the psychoanalytical theory of Freud and devotes four chapters to the exposition of the Freudian psychology, dismissing other theoretical approaches in a single chapter.

Karl Menninger, head of the Menninger Clinic at Topeka, collaborated with the author in the section of the book devoted to psychiatry. Here again we find the emphasis on the teachings of Freud. Having described Freudian psychoanalysis "as the most systematic and logically developed theory of personality genesis and of the nature of the various personality disorders," the assertion is made that "it is also the most effective form of psychotherapy for many of the diseases and may be said to have its value as a therapeutical instrument in practically all the diseases." The chapters on *Character Disorders* and on *Genius* are notable. The section on mental deficiency (*Hypophrenia*) leaves, however, much to be desired.

This book will be more satisfactory as a class text than might at first appear. Some teachers will object to its definite bias in favor of the psychology and psychotherapy of the Freudian school of psychoanalysis. In spite of this bias, it is, however, systematically logical and not dogmatic. Its plea for an experimental psychopathology is more than a mere gesture. It is certainly less dogmatic even in regard to psychoanalytical theories than many texts that slight or oppose these theories. Apart from controversy, the sustained emphasis on psychodynamics versus symptomatology, on an "hypothetico-deductive" methodology, and on the "dynamic-genotypical" versus the "static-phenotypical" approach to the problems of abnormal behavior will certainly warrant consideration of the book as a text for college classes.

Ohio State University

FRANCIS N. MAXFIELD

The Doctor and the Difficult Child. By WILLIAM MOODIE. New York, The Commonwealth Fund, 1940. Pp. ix, 214.

The title of this book is somewhat misleading for it might suggest that it is addressed to pediatricians and concerned with their office-practice. As a matter of fact, it is an account of the work of a completely equipped and organized child guidance clinic and it discusses the work of the social worker and psychologist almost as much as that of the pediatrician and psychiatrist.

The first part of the book is a discussion of the child guidance clinic and its method of studying and treating behavior problems in children. Without becoming involved in any of the psychiatric schools, the author gives a common-sense account of common-sense psychopathology which ought to be quite reassuring to the layman. It would be interesting for anyone who has been connected with child guidance

clinics in this country to read this account of a very similar development in England and to note the major resemblances and minor differences. The chief difference is an intangible one, but the reader can hardly fail to feel that the author has a more sharply defined notion of what is "right and proper" for children than we in American clinics have.

The second part of the book deals with the origin and treatment of many different sorts of behavior problems. Here again, straightforward common sense is the outstanding characteristic, although there are some statements with which most American psychiatrists would be in disagreement. In a discussion of "violent behavior" the author makes no mention of encephalitis as a cause nor does he suggest that this behavior may be primarily attention-getting. It is in his discussion of sex that differences are most marked. One feels a certain English reserve in this whole discussion. He says in two different places that children have a natural reticence about sex, or a natural tendency to shrink from it. American psychiatrists will agree that the reticence is there but will insist that it is not natural but has been caught from reticent adults. His discussion of masturbation is curiously illogical. He says flatly that the ill effects of masturbation are due to the patient's attitude toward the habit and to the attitude of those in authority over him, but in some way fails to draw the conclusion that the way to avoid the ill effects is to change the attitudes. Rather, he insists that the patient is to be encouraged in his struggles with the problem, to be praised for his success, and to be given quiet confidence that he will "win through." Why he should be encouraged to win through from something which is not harmful in itself is not made clear.

These are, however, minor criticisms of a book which is basically very sound. It should be of interest to all who are acquainted with child-guidance developments in this country and of real help in orienting psychologists who are interested in the rôle which psychology plays in the child guidance clinic.

Providence Child Guidance Clinic

TEMPLE BURLING

Schizophrenia in Childhood. By CHARLES BRADLEY. New York, Macmillan Co., 1941. Pp. vii, 152.

While dementia praecox and schizophrenia were originally proposed as clinical concepts from the study of adult patients, the concept of childhood schizophrenia appeared later when children were discovered having behavior symptoms similar to those of adult schizophrenics. Bradley's purpose in this book is to define childhood schizophrenia, to review the literature on the subject, and to present material from his own clinical experience. Precision in definition is necessary, for in the past the value of much work in this field has been impaired by a lack of standard age limits and also by variability in diagnostic standards among different workers. Childhood schizophrenia has not been distinguished clearly enough from other serious mental conditions among children on the one hand, and from the milder behavior disorders on the other.

The greater part of the book is devoted to a review of the literature, covering such important topics as incidence, symptoms, course, types, etiology, laboratory findings and anatomical pathology, differential diagnosis, treatment, and prognosis. The author has done well here in bringing together a mass of extremely heterogeneous and sometimes contradictory material. The general effect is still somewhat confusing

to the reader, but he will find here very convenient access to the original sources. The author has been wise in not attempting to impose a superficial and artificial order upon material which in its present state of development is naturally immature and disorganized. Generally he is content to summarize without undertaking a critical analysis, and in some cases this is to be regretted.

Very little detail is possible in the review of a book which is itself largely a review. The chapters on incidence and etiology are inconclusive, but this is due to the nature of the material and not to any fault of the author. Childhood schizophrenia is an extremely rare condition and any one worker is not likely to see many cases. Statistical studies made up of cases from different writers are of little value because of variability in the diagnostic criteria employed. In this connection the chapter on differential diagnosis is especially valuable. A number of case reports are given both from the author's practice and from the literature. These are particularly significant since at present it seems that careful case studies will do more than anything else to clarify the clinical concept of childhood schizophrenia. The case reports given here support the general view of most investigators that no known treatment is of great value in childhood schizophrenia, and that the prognosis is a gloomy one. There is a very useful bibliography of 118 titles.

University of Pennsylvania

MILES MURPHY

The Psychology of the Physically Handicapped. By RUDOLF PINTNER, JON EISENSON, and MILDRED STANTON. New York, F. S. Crofts & Co., 1941. Pp. vi, 391.

The authors in the preface state that "this book has been designed by us as a textbook for the ever-increasing number of courses in colleges and universities devoted to a study of the psychology of the physically handicapped, as well as for those professionally engaged in work with the physically handicapped in some form or other." The text is designed for students with two or three years' training in general psychology and a knowledge of the critique and technique of test procedures.

The authors are wise in avoiding the assumption of some special type of psychology or some special psychological dispensation peculiar to the physically handicapped. Otherwise excellent books on adolescence, crime, the primary child, and so on have been badly marred by positing some unique neuro-psychology of crime, of adolescent reasoning, of the first grader, or what not. The authors have a definite philosophical attitude which the reviewer found satisfying. They say, "In one sense there is no special psychology of the physically handicapped individual as contrasted with the individual without any serious physical impairment. The same psychological mechanisms are at work in all cases. But any physical defect whether major or minor, presents problems to the individual."

The first three chapters constitute an elementary, eclectic review of personality, mental hygiene, and the neuro-glandular mechanism. It might be of considerable value to older, specialized teachers of the handicapped who have been out of touch with psychology for some time. The fourth chapter, "Psychological Tests for the Physically Handicapped," gives a brief annotated list of tests suitable for the deaf and for the blind. It is a handy, practical chapter for ready reference, giving publishers, addresses, and the usual test classifications.

The remainder of the book deals with the largest groups of the handicapped; namely, the deaf, the hard of hearing, the blind, the partially sighted, the crippled, the defective speech, and finally some attention is given to minor and less easily classified groups. With a careful reading of the suggested references, the chapters should prove an excellent basis for the college student, the special student, or the supervisor of special classes seeking a refresher course or a general view of the field of the handicapped.

University of Pittsburgh

W. T. ROOT

Social Psychology. By CHARLES BIRD. New York, D. Appleton-Century Co., 1940. Pp. xi, 564.

Bird's book is a summary of the researches in the traditional areas of social psychology. It reports competently many laboratory studies and social surveys concerned with motivation and social incentives, attitude measurement, imitation and suggestion, propaganda, crowd behavior, and leadership. In addition, the last three chapters deal with the practical problems of juvenile delinquency, the social significance of age, and war. The traditional topics of language, personality, the socialization process, and the nature of groups and institutions receive only incidental treatment.

The strength of the book lies in the attention given to attitude measurement and motivation. Bird's recognition that research in social psychology is dominantly concerned with attitudes makes his volume timely. With the exception of Murphy, Murphy, and Newcomb's book, no other is as complete in its coverage of attitude studies. The chapters on social incentives which review the work on punishment and reward, rivalry, competition and coöperation are likewise of considerable current interest. Another element of strength is the emphasis upon factual findings whether of the animal laboratory or of sociological investigation. On the other hand, the space devoted to attitudinal material, much of which is indicative of the content of American culture rather than of psychological mechanisms, date the book. The volume is perhaps weakest in its use of anthropological results and in analysis of the social field in which behavior occurs. Little consideration is given to the theoretical and experimental contributions of field theory and of neo-Freudianism.

In general the selection of material is good for the areas covered. The treatment tends to be judiciously critical. The presentation does not altogether avoid the usual textbook failing of being too sophisticated for the sophomore and too elementary for the serious student. If the book lacks something of the sparkle of the more original treatises of Brown and Freeman, it is also true that it contains more meat for the fact-hungry student. The field of social psychology can well utilize this able compilation of so many of its researches.

Princeton University

DANIEL KATZ

The Science of Psychology: An Introductory Study. By RAYMOND HOLDER WHEELER. Second edition, completely revised. New York, Crowell, 1940. Pp. xviii, 436.

This book, apparently designed for a half-year course, is based upon the author's book of the same name published in 1929. The former book has been "rewritten,

reorganized, and abridged." The order of topics in the two books is the same; both begin with social behavior and end with the nervous system. The author emphasized in his first book a small number of organismic laws, particularly the laws of configuration and of least action. In the second book he lays more stress than ever before on a set of 13 laws—which belong to no particular science but which are expressed in specific principles in different sciences. The whole book is now organized around these laws, with the result that the present edition is less like a conventional textbook of psychology than the first. The book will therefore probably appeal to a smaller group of potential users—only to those teachers who wish to present a strongly configurational approach to psychology of Wheeler's type.

The laws are of the utmost generality; for instance, "electrical, chemical, biological, psychological, and social systems all obey" the law of maximum work. "When a person fights to prevent his destruction his behavior is to be explained not by a mystical instinct of self-preservation but by the Law of Maximum Work." In addition to the emphasis on laws, there is also an emphasis on the predictions of greater or less precision that can be made by applying them. In spite of the book's apparent brevity and in spite of its emphasis on configurational laws, it presents a surprising amount of factual material. Certainly all of the conventional topics of the elementary course are to be found in it. The author's style of presentation has much improved since his first edition; he is direct, clear, and vigorous, and the illustrations which he gives of various principles are usually well chosen.

Rice Institute

FRANK A. PATTIE, JR.

Dictionary of Terms and Expressions of Industrial Psychology ("Psychotechnics"). By MICHAEL ERDÉLYI and FRANK GROSSMAN. New York, Pitman, 1939. Pp. viii, 98.

This dictionary offers 66 pages of 4 columns each for the presentation of its materials. German words are shown in the first column followed, in the remaining columns, by their English, French, and Hungarian equivalents. There are three indices of about 10 pages each, listing the English, French, and Hungarian terms alphabetically, followed by page references for these terms and their equivalents in the main section. Approximately 1800 terms are thus presented in the four languages.

The criterion for the selection of the terms is not apparent to this reviewer. Thus the term *Akkord* (harmony) is given, but *Akkordarbeit* (piece work) is not, although the latter is clearly more important in psychotechnics. We find the English words moral and morality but not *morale*. Terms relating to time-and-motion study are not included nor are German terms dealing with military psychology—important as this phase of industrial psychology has been of recent years. Translations are frequently inadequate: *Entwicklungspsychologie* cannot be translated as psychology of evolution but must be rendered as developmental psychology; *Minderwertigkeitsgefühl* is feeling of inferiority rather than inferiority complex; *Fortbildung* is not higher education; *freier Beruf* is not free trade; and there are many other instances of the sort.

On the formal side, we find all the German words capitalized—a genuine handicap in a language where differences between upper and lower case letters are significant.

There are numerous errors in spelling and inconsistencies in capitalizing. Despite these shortcomings, a parallel set of terms in four languages has a certain interest and the information supplied may be taken for its suggestive, if not for its authoritative value.

Brown University

H. L. ANSBACHER

Community Hygiene. By ELIZABETH S. SOULE and CHRISTINE MACKENZIE. New York, Macmillan Co., 1940. Pp. 218.

In this compact volume the authors have attempted, with considerable success, to summarize what the average student of nursing, physical education, or home economics ought to know about community health problems. No effort has been made at completeness, but in 10 short chapters the most important public health problems are briefly and ably discussed. As the authors explain in their introduction, they have compensated for the brevity of the text by providing at the end of each chapter a fairly full bibliography and reference list, referring chiefly to publications easily accessible to the student.

Having discussed the history of public health work, the authors then proceed in the next seven chapters to sanitation, housing, communicable disease, maternity, infancy, and child hygiene. In the ninth chapter, the reader is rather hurriedly introduced to the problems of industrial hygiene, heart disease, cancer, diabetes and malnutrition. The tenth and last chapter is devoted to public health organization and administration.

Though this text leaves something to be desired in the way of attractiveness of arrangement and of illustrations, it is written to fill a very definite need for a "simple text, enriched by wide reading assignments" and within those limitations the reviewer feels that the authors have succeeded very well.

Cornell University

D. F. SMILEY

The Psychology and Ethics of Spinoza. By DAVID BIDNEY. New Haven, Yale University Press, 1940. Pp. x, 454.

In this careful and exhaustive study, the author undertakes to present Spinoza "as he himself wished to be regarded; namely, as essentially a modern or Renaissance philosopher" (p. 11). His criticism of previous studies is that, in general, they either read into Spinoza's thought the principles of some subsequent philosophy or interpret it primarily by reference to Greek and Scholastic thought. The fundamental thesis which the author advances and which he seeks progressively to prove is that there is a logical and historical conflict in Spinoza's thought between the tradition of Plato and Aristotle on the one hand and the atomic-mechanistic tradition of Democritus, Epicurus, and Lucretius on the other hand.

In Part I, the author examines Spinoza's doctrine of the emotions, that is, his psychology. He is led to the conclusion that contemporary historians of psychology have overlooked the most original contribution of Spinoza; namely, his doctrine that the soul is a spiritual automaton or mechanism and have consequently failed to recognize him as the originator of the theory of laws of mind. In Part II, concerned with Spinoza's Ethical Theory, the author points out that the philosopher advanced diverse theories of value which are to some extent conflicting, but in which all the major issues of contemporary axiology were considered though not clearly defined.

Throughout the book, the author considers in detail other interpreters of Spinoza

and contrasts his own interpretation with theirs. His criticism is frequently unfavorable but always sympathetic and fair, and always carefully stated and supported. Not the least value of the study is the completeness with which the author seeks to point out the influence of Spinoza on modern and contemporary thinkers.

Tulane University

MARTEN TEN HOOR

The Development of American Philosophy. By WALTER G. MUELDER and LAURENCE SEARS. Boston, Houghton, Mifflin & Co., 1940. Pp. x, 533.

This is an anthology of American philosophy from Jonathan Edwards to John Dewey and contemporaries. The material is arranged under eight headings, as follows: Early Philosophical Theology and Idealism, The Period of the American Enlightenment, Transcendentalism, Evolution, Idealism from William T. Harris to James E. Creighton, Pragmatism and Critical Empiricism, Realism and Naturalism, and Recent Perspectives in American Idealism. Each part is provided with an introduction, to which is appended a selected bibliography of primary and secondary sources. An index concludes the volume.

What authors and what selections should be included in a book of this sort is a question to which various answers are possible, of course, and perhaps no answer would satisfy everybody. I am glad that the editors included the symposium on *Law and Design in Nature* by Newcomb, Porter, Clarke and McCosh; and their conclusion of each of several parts with a selection from an outstanding critic of the general point of view represented seems to me admirable. It strikes me as odd, however, that they deemed John Woolman worthy of more than twice the space given to Benjamin Franklin, or that they excluded W. M. Urban from the list of representatives of contemporary idealism; and I am surprised that they should have thought Emerson's "transcendentalism" is better expressed in his famous Phi Beta Kappa address than in, say, his essay on *The Over-Soul* or, better still, that on *Circles*. The volume remains, however, a convenient compendium of original material for use by classes in American philosophy; and it supplements, rather than duplicates, similar material contained in another recently published anthology, *Philosophy in America*, by Anderson and Fisch.

Cornell University

G. WATTS CUNNINGHAM

Hoaxes. By CURTIS D. MACDOUGALL. New York, Macmillan Co., 1940. Pp. viii, 336.

The student of social psychology who is looking for case material will do well to add this volume to his standard list of references. Here are displayed some five hundred hoaxes, spreading in time over two millenia and in kind over a wide variety of fields. They are reported in orderly and sympathetic terms, with adequate documentation to aid the serious scholar. The author must have spent years in collecting the basic materials and he writes about them with insight and enjoyment.

The first half of the book attempts to explain why hoaxes succeed. "Indifferent, ignorant, vain, suggestible, awed by the real or feigned prestige of those who speak with authority, man believes what he wants to believe." Each of these descriptive adjectives comes in for discussion as a source of belief in hoaxes and each is illustrated by means of examples, usually well chosen. The second half of the volume traces hoaxing through such fields as history, literature, religion, science, and jour-

nalism. The story of literary hoaxing alone is enough to make one despair of identifying the real authors of the classics; and the section on hoaxing in history makes one's blood run cold at the thought of the long line of earnest students who have been completely taken in by fraudulent evidence.

This is no careless job of popular journalism: it is a serious attempt to portray the motivation, reception, and perpetuation of hoaxes in their manifold forms. Pictorial illustrations are used to good advantage and an excellent index will aid the scholar in making repeated reference to individual cases. From the story of the Winstead Liar to the detailed account of Mencken's irretrievable petard on the history of the bathtub, here is a rich hunting ground for those who study the credulity of *homo sapiens*.

University of Maryland

JOHN G. JENKINS

How to Counsel Students. By E. G. WILLIAMSON. New York, McGraw-Hill Book Co., 1939. Pp. xx, 562.

This book, an outgrowth of the development of a graduate course for the training of counselors, is particularly concerned with the functions of one type of personnel worker, the clinical counselor, and has as its fundamental purpose "the adaptation of principles, procedures, and techniques of clinical psychology to the adjustment problems of high school and college students."

Since the orientation of this book is practical there are numerous statements to which the critical psychologist may take exception. In endeavoring to afford a comprehensive treatment to topics disparate in name only, each type of problem is discussed under the following headings: incidence, causes, analyzing and diagnosing, techniques in counseling, and methods of prevention. An unpleasant amount of repetition results which, however, may well be a virtue in view of the inadequately and heterogeneously trained workers who at present are exercising counseling functions throughout this country.

Cursory examination of this book is likely to yield a conclusion that the author is much too optimistic and unhesitating in his advocacy of counseling. If so, the reader should not neglect the final chapter. Williamson's aim is the professionalization of counseling, in itself an art, but dependent upon scientific techniques which have not yet passed beyond a crude stage of development. Despite a fluctuating level of excellence in the treatment of topics, this book represents an important contribution because of its emphasis upon the needed individualization in education and its indication of those areas demanding research worthy of the name.

University of Maryland

ALAN M. KERSHNER

Born that Way. By EARL R. CARLSON. New York, John Day Co., 1941. Pp. ix, 174.

This book is a popularly written and very readable autobiography of a "spastic" (infantile cerebral palsy resulting from birth injury) who eventually became a widely known physician specializing in the treatment of such cases. The author traces in some detail his gradual improvement and development through his childhood struggles, college and professional education, vocational experiences, and medical practice. A more detailed description is given of the clinic organized by him at the Neurological Institute in New York City. The author has subsequently been

instrumental in the establishment of several similar clinics in different parts of the United States and in a number of South American countries.

Both in the account of his own efforts to overcome his very serious handicap and in the discussion of the methods which he instituted in his clinics, Dr. Carlson brings out certain points of psychological importance. The improvement which follows when the interest and attention of the spastic are absorbed in matters other than his handicap, for example, is stressed and illustrated throughout the book. The importance of academic instruction and 'mental training' is likewise emphasized. Such instruction is effectively combined with physical therapy in the program followed in the author's clinic. The rôle of emotional factors is frequently illustrated, as in the beneficial effects resulting from the spastic's first job, and the aggravation of his symptoms when he is put in an unfamiliar or embarrassing situation. Not only does this book have a certain practical value for psychologists who may have occasion to work with spastic cases, but it furnishes interesting illustrative material for the general psychologist as well.

Queens College

ANNE ANASTASI

Investigaciones Referentes a la Psicología de la Juventud Peruana. By WALTER BLUMENFELD. Lima, Peru, *Rev. Cienc. Univ. San Marcos*, 41, 1939, No. 430. Pp. 103.

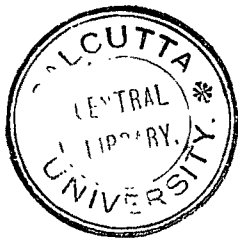
The general pattern of this study is common in this country but unusual in South America. It consists of a summary of aptitude test data on three successive samples. The investigator used conventional data analysis methods for interpreting results obtained from 667 students who were tested at time of entrance into the university. The test items included relations, 'complements,' opposites, directions, analogies, and number series.

Results suggested some increase in test scores during the three year period and an elaborate interpretation of this finding is presented. Speed and level of test performance were related. Some of the other findings were: low intercorrelations between tests (most of the coefficients were in the 20s and 30s); a reasonable relation between indices of academic achievement and test performance (coefficients in the low 40s); no sex differences; and an inverse relation between age and test performance up to 23 yr.

Blumenfeld rightly suggests that the tests should not be used for selection purposes without support of additional data from other sources.

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